The Phenomenology of Economics

Life-World, Formalism, and the Invisible Hand

Till Düppe

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The Phenomenology of Economics

Life-World, Formalism, and the Invisible Hand

Fenomenologie van de Economie

Levenswereld, formalisme, en de onzichtbare hand

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εστι γαρ απαιδευσια το μη
γιγνωσκειν τινων δει ζητειν α
ποδειξιν και τινων ου δει.
For it is uneducated not to have an
eye for when it is necessary to look for
a proof, and when this is not necessary.
(Aristotle, Metaphysics 1006a)

For Nino and Chantal

Foreword

The Phenomenology of Economics is part of a larger project. While the target here is one particular economic discourse, economic science, a full-blown phenomenological economic criticism had to include a (genetic) phenomenology of economic life, in particular of today and the last centuries of capitalism. It had to include traditional texts of phenomenology, read them as economic writings and ask which interest they serve therein. It also had to include other economic writings, ancient as well as early modern, more of the socialist and Marxian tradition than could be considered here, present popular economic writings, and, not to forget, those diagnoses of present-day capitalism that are based on a critique of the phenomenological tradition as in much contemporary philosophy – yet economic science seems endlessly remote from being even able to recognize these writings as a challenge. Worlds of concepts, methods, styles and interests, of histories and cultures separate the scientific from other economic discourses. For the reason of this remoteness of economic science from any conception of economic life, and from the concerns of other economic discourses the phenomenological critique at hand is confined to economic science.

The Phenomenology of Economics can thus be read as a prolegomena to a renewed sensibility in economic life which is undermined as long as there is one economic discourse that claims authority in the name of science. At its bottom-line it contributes to the tradition of pamphlets against scientism in economic writings with an historical argument. The history of the scientification of economics that I describe along the endeavor for a generic object of "the economy" (the invisible hand), a tendency to formalism, and the ideological suspicion economic science evoked, so this historical argument, was the history of the oblivion of economic life and the world in which it is lived.

For who then is *The Phenomenology of Economics* written? It is addressed first of all to academic economists, in particular to those who seek alternative ways of practicing economics, who worry about the discipline and the scholarship it allows for. Moreover, it also addresses all who have to deal with academic economists, such as other social scientists, politicians, and whoever concerns about the state of present economic life in 21st century capitalism. When addressing such audience, I had to keep the inner problems of the phenomenological tradition to a minimum, be they exegetic problems between Hua I and XXXIX, or between Heideggers GA1 and GA90 – debates I share and even belief to be discussed in economic terms (see e.g. Düppe 2008).

Although the critique of science is vital for the phenomenological tradition in that it allows for the phenomenological reduction, this critique too often does not address more than its own preconceptions of science, let alone the actual scientist. My experience within the institutions of economics was that the phenomenologists trying to enable a *reflection* (*Besinnung*) by recalling the oblivion of the life-world is closest (not without disclaimers) to what the historian does. Phenomenology of science amounts to the same as an intimidating historical style, with which I attempt to remain true to a genetic rather than hermeneutic type of phenomenology. I mobilize Husserl's insight that every notion of science needs to include the fact that science is a product of historical life. Only in history we can pose the question that defines, according to Husserl, a "responsive critique" – namely, a "self-reflection aimed at a self-understanding in terms of what we are truly seeking as the historical beings we are" (Hua VI: 73, E.: 72).

Rather than an exercise in phenomenological exegesis, I thus attempt to let the historical horizon show (*zeigen lassen*) from which and into which the practices of economic science take place. The material I present stems largely from the social history of economic ideas. There I show that precisely such a responsive reflection on the motives of science is impossible as long as one claims scientific authority. As soon as one does, one too easily ends up not claiming anything. This gives voice to much of the uneasiness, disorientation, irony, and sometimes even cynicism that has been felt by many economists and observed by their commentators during the last decades. Yet I expect many to agree: There is no longer anything to be gained by continued lamenting about economics. We should take a step ahead and acknowledge that economic science came to be – too late to hurrah or boo; too late to celebrate or complain. The question now is no longer, 'Which economic science?' but 'When does it disappear?'

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Introduction

In her poignant memorial speech before the collegiums of the University of Berkeley, Chantal Debreu de Soto revealed the following about her father, Gerard Debreu (1921-2005), a rather inconspicuous but nevertheless Nobel Prize-crowned mathematical economist.

> He (my father) seemed either to not have the needs other humans had or he managed never to show that he had those needs. Other people, other fathers got tired, got hungry, got thirsty, had a sweet tooth, sweat when it was hot, shivered when it was cold, got distracted when they worked on a difficult task. Other father's clothes got wrinkled or got food stains on them. Other fathers got sad, discouraged or angry. For the longest time I believed that my father never did any of these things. I believed he slept fully clothed in his dress shirt and the bow tie that was his trademark for so long, with his dress slacks, his leather belt, his watch and his dress socks (Debreu de Soto 2005).

Chantal Debreu speaks about the human reality of her father, about which his colleagues were only partially aware of, and his readers may not even have thought about, but which she herself was exposed to daily. What is surprising about these lines is not what commentators of science – who are sensitive to this reality – are usually interested in: the exposing of concrete contingencies of what appears to be sheer epistemic authority. Debreu's daughter was able to reveal nothing of these hidden, thereby thrilling, and yet so human aspects of the scientist's world. There was simply nothing to disclose. Debreu's person was as formal as his work.

Debreu seemed to have incorporated in not only his professional life but even in his most intimate social life what could be called *the* imperative of modern science: to be discreet about one's own particular reality. The distance that scientists take from their own reality pervaded Debreu's entire life, right down to his dress socks. Does not his daughter's portrayal suggest that Gerard Debreu not only behaved discreetly with his closest fellows, but was even wary of himself? Debreu appears to have felt uncomfortable with the distraction of being hungry, of a wrinkle in his garment; he was cautious of being vulnerable and afraid of being humble. Perhaps he really did sleep fully clothed in his dress shirt and bow tie.

If I consider such distancing from one's own reality as the imperative of modern science, my critique moves away from questions of the legitimacy of science – be it in light of well- or badly-founded epistemological principles (as in the philosophy of science) or in light of revealing particular interests of scientists (as in the sociology of science). It directs my attention to the difficulties of incorporating the imperative of modern science *for* the scientist. For does this imperative not work against the intellectual sensibility of the scientist? If it is true, as phenomenologists insist, that all the motives that keep science going stem from the reality of the scientist's life, then the problem of modern science is that its *practice is the oblivion of the motives that give rise to it.* This, in a nutshell, is the phenomenological diagnosis of modern science. It has come to be headed under the title "oblivion of the life-world". This is the case I shall make for economic science.

The Reality "of" and *of* Science – The Continuing Crisis of Economics between its Authority and its Significance

The reality "*of*" science. The ambiguity of this expression describes the full difficulty modern science represents for the scientist. The reality "of" science as the reality to which science claims authority and the reality *of* science as the reality of this claim itself do not match. The epistemic imperative of modern science, since the 17th century, has been to present knowledge as though it were independent of the reality in which and through which it comes about. This reality includes above all its history, its institutions, its subjects and their biographies – that is, all that through which one acquires an intellectual ethos. In the words of one of the few outspoken economists: "When we are told that we must understand a man's life to understand what he really meant, we are being invited to abandon science" (Stigler, in Popkin 2008: 30). Within the claimed reality of science, the reality of the claim itself is not supposed to appear. In other words, the commitment of science to the transparency of the world has its drawback in a more fundamental in-transparency – namely, of the world the scientist lives and which allows him to commit himself to science. Such is the phenomenological concern of the commentary of science. It points to the difficult relation between science and its experiential reality.

More than 70 years ago, Edmund Husserl evoked this ambivalence with regard to the reality "*of*" science when in 1936, in the last years of his life, he spoke about the *Crisis of European Science* (Hua. VI) – European because science came from Greece and then took root in modern times first in Italy, then in France and England. In this late work, Husserl moved away from his lifelong belief in science and initiated perhaps the first move beyond that modern philosophy, which has aimed at a foundation of science. "Is there, in view of their constant successes, really a crisis of the sciences? " (§1), he opened this last publication. Although in its own terms (in terms of the reality it claims) science was successful in a way it had never been before, Husserl announced a crisis. It "concerns not the scientific character of the sciences (*Wissenschaftlichkeit*), but rather what they, or what science in general, had meant and could mean for human existence" (Hua VI: 3, E.: 5). The crisis consists of the manner in which science has set its terms of success, in the "the whole manner in which it has set its task" (Hua VI: 1, E.: 3). For this reason, Husserl spoke of the "loss of its meaning for life" (*Verlust ihrer Lebensbedeutsamkeit* §2), which became the archetype of continental critique of modern science in the 20th century. "The oblivion of the life-world" became its catch phrase.

Considering the decades following Husserl's announcement, he remained right. The last century of the history of academic institutions, at face value, can only be called a success of science. Academic life became dominated by and even identified with the character of the scientist at the cost of, say, the savant, scholar, or literate. The scientist represents academia in society, and, having increasingly fewer opponents within academia, can rely on a ready-made social identity – or better: a ready-made ethos as a scientist. The distance between the practitioners of science and their critics seems greater than ever before. For this reason science occupies an ambiguous position in society. Although the promises, the grants, and perhaps also the belief in science are greater than ever before, we pause when speaking of its achievements. As firm as is the character of the scientist, so great is the doubt that there is nobody who takes responsibility for the "progress" of science. How else could it enter its devastating alliances with 20th century politics, technology, and culture in general if not by virtue of its social irresponsibility, and by means of those who knew how to exploit science for their own purposes? What else could lead from the moment when Galileo cast the first stone to the two drops of August 1945? Believing in the authority of science on the one hand, how could we possibly trust the ethos of the scientist on the other?

Such social responsibility of science was what Husserl meant when he spoke of the crisis of science that "concerns not the scientific character of the sciences (*Wissenschaftlichkeit*), but rather what they, or what science in general, had meant and could mean for human existence" (Hua VI: 3, E.: 5). While the difficulty of incorporating the imperative of modern science is the concern of the following exercise, social responsibility is its normative horizon.

After 1945, the crisis of science deepened – and in particular in economic science. Economics played a decisive role in the success of the institutions of 20th century science, regarding, in particular, its socio-political consequences. Economic science came into its own after WWII, transforming from a discipline struggling for a suitable identity into a powerful, rapidly growing institution of Big Science. We should not forget that economists – at least some celebrated members of the field – have contributed to the open violence that has been done in the post-war decades in order to secure Western freedom. Economists have considered the two bombs as an "allocation of resources". And neither should we forget how economists contributed to the silent violence that, since the 1970s, neoliberalism has exerted on the political discourse by restricting its terms of critique. Economists have thought of political life as the "maximization of votes".

Inside academia, economics came to dominate the scientific standards of social theory. Even if the size of the institutions of economics today decreases in terms of students moving to neighboring sciences, and even if these sciences exert a strong influence on economics, economic theories have already made their way into these neighboring disciplines. Think of statistics (which *is* econometrics), but also of political sciences, sociology, and even anthropology. I have just read a call for the application of formal methods such as game theory to the epistemology of religion – modeling "miracles" and the like. Economics sets standards of scientific authority. It *is* the attempt to cultivate scientific authority in all discourses that aim to understand the social world in which we live. In this attempt economics had unsurpassed success, in that the figure of the *social scientist* – a novelty in the 19th century – became an established character within the academia. In the second half of the 20th century, the ethos of the social scientists lost a great degree of contestability. Gone are the days in which they evoked the suspicion with which clergy once regarded natural scientists. The lion's share of the credit of this success goes to economists.

Despite the success of economics in the last five or six decades, it remains in a crisis. It is in a crisis precisely regarding "the whole manner in which it has set its task" (Hua VI: 1, E.: 3).

Since the 1970s at the latest, there is on all levels of practitioners and commentators a sense of, say, uneasiness about the discipline. Is there not a secret agreement among economists that economic science does not deserve and cannot take responsibility for its institutional pervasiveness? Is there not a secret uneasiness that other modes of economic reflections, even if their relevance is implicitly acknowledged in their importance, are pushed into the background of the discursive stages of the Western world? After the formalist revolution of the 1950s and '60s – a "coma" of modern economics since most economists were excluded from the core – is the re-awakened profession not plagued by a desperate search for novelty, for a new beginning, for new roots? Is not the intense search for theoretical innovation since the 1970s, rather than a sign of proficient productivity, a sign of disorientation? I think of the many "neoisms" in economics, the New Classical, the neo-Ricardians, the neo-Keynesians, the neo-Austrians, etc. I think of the adaptation of new methods from other sciences, such as the renaissance of cognitive sciences, the boom of computer sciences, and the rise of laboratories. I think of the search for new paradigms beyond equilibrium and rationality, such as bounded rationality, principal agent, asymmetric information, mechanism design, etc. Is the tirelessness of these innovations not an expression of the insecurity to which a theoretical innovation really amounts? Does it not give an odd unity to economics that all these innovations are made for the sake of overcoming the omnipresent spectre of so-called "neoclassical economics" – in particular because all attempts end up re-enforcing economists' incontestable authority as a science, and with it the empire of their institutions?

Skepticism regarding the state of economics as a science is no novelty. Perhaps arguments of this sort have by now already managed to represent the common sense of the profession. Most economists today may not even consider themselves as scientists. Yet there is no alternative with whom else one could identify, as the following statement of an economist shows that I consider to be representative: "People ask me if I'm a scientist, I'd say 'No, I'm an economist.' However, that doesn't mean I don't think it's rigorous, and I think it's as rigorous as science" (in Reay 2008: 146). Economists are in an ambiguous relationship with science – attracted yet unwilling to embrace it. One of the early economists who has shown skepticism about economics as a science, and formulated a conflict that will figure prominently in my account of economics, is Frank Graham:

> Economics has always been under suspicion as a 'science,' and the consequent defensive attempts on the part of its exponents to force their theory into rigid scientific forms has frustrated its application to the facts of life (1999 [1942]: 28).

This frustration was shared later by economists such as Robert Gordon, who spoke of a trade-off between rigor and relevance (1976). He set the tone of much of the critical commentary in the 1980s and 1990s, from Klamer's interviews onwards (1984). The common motif of this critical commentary is the affirmation of the urge to do more relevant work, yet, simultaneously, the acknowledgement of one's own powerlessness. In the last decades, the pressure for economics to be relevant is higher than ever, yet has never been as difficult to achieve as today. As an economist who was trained in the mid-1980s said after 20 years at academia:

> If I knew how to make academic economics more directly relevant to what non-academics are interested in I would push for some changes. But I don't know how to do that. Indeed, there may not be any way of doing that (in Colander 2007: 89).

The clearest symptom of the crisis of economics is the existence of leading economists who themselves do not believe in the worth of their work. In economics we find a unique attitude unparalleled in other social sciences that economists do not necessarily believe in economics. It is possible to be a successful economist while maintaining a skeptical or even ironic stance toward one's own discipline. Here for example Robert Clower, a former editor of the *American Economic Review*:

> Much of economics is so far removed from anything that remotely resembles the real world that it's often difficult for economists to take their own subject seriously. Publishers have sometimes asked why we economists don't write as if we were intellectually engaged; why don't we produce books about the marvels of our science? The answer is simple. Economics doesn't have much to marvel at (Clower 1989: 23).

The repelling attraction of economic theory is more frankly expressed in the following, more recent statement by a leading game-theorist: "my greatest dilemma is between my attraction to economic theory, on the one hand, and my doubts about its relevance, on the other" (Rubinstein 2006: 866). Showing such an attitude can be said to be normalized in economics, perhaps even fashionable. The crisis in economics consists of this conflict of being an economist on the one hand, and scientist on the other: the conflict between making an economic claim and making it in the context of science.

An apt example of this conflict is the following reply by Sir John Hicks (1904-1989), one such economist whose belief in science backfired. In his younger years he contributed to a development, about which he later showed a sense of remorse. Perhaps for this reason the person of Hicks, like Debreu, is rendered discreet. Although he received the Nobel Prize, and although each student of economics knows his name for the "Hicksean demand function" – the inversion of the Marshallian demand function, remember? – in the small village Blockley where he passed away, nobody knew of him. Asked about economics after 1945, he seemed to have lost touch with his intellectual means.

> (*He hesitates*). I do feel that most of this stuff that I pick up and see in the journals seems to have very little relevance to the sort of practical problems that really bother people.
> *Such as?*
> I mean…what have these mathematical theories got to say about whether Britain should go into the EMS (European Monetary System)? Nothing! That is the sort of question about which economists should have something to say.
> *Do you want to say something about that?*
> I'd like to if I could. But I think it is beyond what is left to my capacities (in Klamer 1989: 180).

Such can be called the phenomenological symptom of modern economic science: that its practice is the *unlearning* of the ability to express what one actually or at least initially was up to. In the words of one of the formulas of the coming exercise: The practice of economics is the oblivion of the motives that give rise to this practice.

To be sure, economists always knew, at least in secret, that the ethos of a scientist is not sufficient for becoming a fully-fledged economist. Adam Smith certainly knew it. How else could there be a Smith problem? John Stuart Mill knew it perhaps even better than Karl Marx. "Political Economy, in truth, has never pretended to give advice to mankind with no lights but its own." (Mill 2000 [1873]: 263) From Alfred Marshall to John Maynard Keynes, economists thought of themselves as the long arm rather than the analyst of practical man, producing not more than elaborated common sense. "The common sense of a person who has had a large experience of life will give him more guidance in such a matter than he can gain from subtle economic analysis" (Marshall 1938 [1890]: 84). And, also the curious Chicago economist George Stigler knew it when asking "Do Economists Matter".

> A curious tension emerges from the simultaneous workings of two influences upon us. We wish to be scientists, with sound logic in our theories, reliable procedures in our empirical applications or those theories, and objective and fair-minded statements of the limitations of our knowledge. We wish also to be important – or in the language of this day, we wish to do good – much good, and generally recognized as such (Stigler 1982: 66).

But the former comes at the cost of the latter. From Smith to Stigler, economists knew that one needs more than a sense for science in order to make an economic claim.

Most economists may still agree today. However, this insight no longer echoes in the halls of the institutions of economics. To point to the insufficiency of the practices of science for being a full-fledged economist bears the mark of anachronism. Although most economists are aware of this insufficiency, the reality of their ethos has become ossified. The conditions that a reflection on the ethos of economists make a difference and take root in the profession are no longer met. It is difficult to come up with an alternative ethos without actually moving to another department.

The Clarion Call of the Phenomenology of Economics: Economists of the World – Disperse!

Since the 1970s at the latest, the perception of this crisis in economics has clearly been in the air. It is part of the consciousness of all economists, and has been stated in this or that way by many people from both inside and outside, and both left and right wings of the profession. There is something of a postwar tradition of lamenting about economics after the formalist revolution took hold with a new theoretical core: the conditions under which a general equilibrium holds. Let me recall some of the main actors, since it is their laments to which The Phenomenology of Economics responds.

Fierce critique began in the 1970s in some of the presidential addresses of the American Economic Association. Popular economists such as John Kenneth Galbraith have expressively pointed to the costs of gaining scientific authority (1973). Economists who favour empirical work may still remember Wassily Leontief's critique on the mathematical overweight of economics (1971, 1982). Another unforgettable one has been the ethnographic satire of "life among the econs"-tribe of Leijonhufvud, who just rescued Keynes from IS-LM (1973). Those who held on to positivist beliefs about science, as the tradition from Hutchison (1938) to Blaug (1980), contributed to the sense of a crisis, too. Even philosophers of science who typically have goodwill in defending theoretical aloofness, showed skepticism about the "vanity of rigour" in economics, such as Cartwright (2007). Part of this tradition is certainly the anti-economic activism that ranges from the "anti-Samuelson" of Lindner (1977) to the "post-autistic economics" movement of 2000 (Fullbrook 2003). It also includes those who seek new forms of commentary between McCloskey's *Rhetoric of Economics* (1985) and Ruccio and Amariglio's *Postmodern Moments of Economics* (2003). In the last decades such skepticism has been normalized.

One study needs to be noted especially for it came closest to the phenomenological locus of criticism – namely *The Crisis of Vision* by the historians Heilbroner and Milberg (1995). Having made a life-long argument for a more "worldly" ethos of economists, they announce:

> A deep and widespread crisis affects modern economic theory, a crisis that derives from the absence of a "vision" – a set of widely shared political and social preconceptions on which all economics ultimately depends (1995: Announcement).

Most of the aforementioned commentators and practitioners have put their skepticism in terms of economics having lost touch with something to which it should *refer*. Heilbroner and Milberg put it in terms of a "rule" that they may have heard in their youth in the 1950s when Alfred Schütz lectured at New School – namely, that "regnant ideas must be relevant to lived economic experience" (Ibid.: 2).

Am I going to repeat this sort of argument? Is it this old song to rescue economic science by means of either bringing it back to its proper realm or leading it to the relevant questions that came to be excluded? In light of this standard critique of economics, the reader may expect from The Phenomenology of Economics, with the subtitle at hand, an argument such as this: Economics does not relate sufficiently to the "life-world" – the world as ordinary people experience the economy – because economic theory is "too formal" – too much mathematics, too abstract, too much model building for its own sake – in particular in the theoretical tradition associated with the "invisible hand", namely general equilibrium theory that culminated in the formalist revolution of the 1950s. Have we not heard this song too many times already? Is there a single economist left who does not agree? Does the all-pervasiveness of this critique not make us wonder if it is perhaps the last unifying element of economics today? Is it not time to go beyond lamenting the state of economic science?

The Phenomenology of Economics does not repeat the dominant demand of most standard critiques. I do not suppose there is a proper realm or authentic questions of economics. If this were all there were to understand about present-day economics, I would not speak of a crisis at all. There is thus not a crisis in the sense Kuhn coined the term: a transitional phase between one and the other paradigm. There is a crisis only in the sense that there is a common awareness of the eminent risk of working for nothing but the garbage bin; however, simultaneously, the insight that one lacks the means to do something about it. Economists are ensnared. In one of her columns for the *Eastern Economic Journal* Deirdre McCloskey gave a "brief list of devastating internal criticism of modern economics that have not been answered" (2000: 244). Among the four dozen attacks, her charge of the misuse of statistical significance ranks high. It has not been answered.

There is no reason to enlarge this list of unanswered challenges and contribute once more to the tradition of lamenting the narrow limits of economics. Perhaps we should come to the point of acknowledging that economists cannot do better than that without loosing their discursive identity. Perhaps there is no better economic science. Perhaps the history of the scientification of economics is somewhat complete. Perhaps there is no renewal to come. Perhaps we should give up the belief in a new wave towards a more "reasonable" economic science. Perhaps the times in which economists had to say something as scientists have already passed. Perhaps there is no longer any need to claim scientific authority. Perhaps economists could even do better without.

The task today is to move beyond mourning in ever more severe tones. Instead, in order to come to appreciate the liberation from science, I invite the reader to a reconsideration of the horizon from which economics *could* make sense. What were the motives that led into this crisis and what does it tell us about economics as such? What made some believe in economic science in the first place? What has necessitated the present crisis of economics? Such questions were hardly posed in the postwar critique of economic science, because hardly anyone was willing to give up the belief in a brighter future or a noble past. Hence I aim not at stating, but at making intelligible the genesis of the crisis in economics.

Instead of thinking about this genesis, is it not much more urgent to advance a new, better foundation of economics? I am far from taking the wind out of the sails of the standard critique and depriving it from the intellectual forces that keep it going – namely the conviction that economics does matter. Instead I want to channel these intellectual forces away from science. I do indeed share much sympathy with those who reply to the nerve-crumbling mourning with "Can we please move on?" – as Galbraith recently (2002) reacted to another wave of complaints about the lack of 'worth of standard microeconomics' (Guerrien 2002). "Aren't you tired of embedding your originalities in critical restatements, however elegant, of what is already clear to thousands of bright undergraduates on the second day of class?", Galbraith asked (Ibid.). He presents a list of ten points by which to arrive at a new curriculum, including, not surprisingly, more empirical work, more consideration of industrial power, and teaching of the classics.

Yes, we should indeed move on. I do address those economists who got tired of their critique. "*Zu den Sachen selbst*" was the (today already somewhat dusty) clarion call of Husserl's philosophy. I do, however, neither provide a new phenomenological methodology, nor new material for a renewal of economic science. I am not immediately concerned with *what* economics should be about – let alone how to form theories about it. I am afraid that these attempts at a better economic science could deepen the crisis. Perhaps pursuing "good economic science" made sense for some time, but today is running in neutral. Perhaps the commitment to science works against the very sensibility regarding what matters in economics? Instead of bringing economics back on track, I want to renew a sensibility for economic life that is concealed in the moment one claims epistemic authority. I thus rely on a certain skepticism regarding economic science, but aim to advance, rather than exploit, it.

Here, thus, is the case to be made: Claiming scientific authority in economic talk is phenomenologically unbearable. It represents a conflict *for the economist.* The understanding of this locus of criticism is vital. I do not argue that there is anything wrong with the being of "the economy", that is much too complex for science and not capable of truth (I believe the notion of "the economy" is a misunderstanding of science). I do not argue that economic science excludes the truly human aspects of "the economy", or that economics does not meet any other standards I impose (I believe the question of scientific standards is from yesterday). I do not argue that economic science requires thinking in a narrow way about the world (which world could that be?). Neither do I argue that economics makes economists like the people in their models: selfish ("autistic" I find more accurate), marketers (willy-nilly perhaps yes), brute calculators (at most as an effect of being bored), or whatever has been said about the character of economists.

Rather, I argue that *whatever* the economist wants to be, seeking scientific authority is of no help for his or her expressive life. The very experience of practicing economics is the locus of criticism of The Phenomenology of Economics. For this and no other reason does it deserve this name (and furthermore deserves to be called, yes, transcendental). Practicing economic science, as it is the bottom line of the conflict I explicate, is only possible as the *oblivion of the motives that give rise to it.* In the commentary of economics, Heilbroner and Milberg came closest to this locus of criticism:

> By vision we mean the political hopes and fears, social stereotypes, and value judgments – all unarticulated, as we have said – that infuse social thought, not through their illegal entry into an otherwise pristine real, but as psychological, perhaps existential necessities (1995: 4)

It is precisely this necessity that the following exercise will confront with the concrete historical conditions of claiming scientific authority. Is a *responsive* intellectual life in economic science possible? I do not aim at drawing once more the line between real and sloppy scientific practice in economics. But I show, plain and simple, that economic – pause – science is impossible: *Economists cannot be scientists.*

Economists can have many kinds of characters. They can have the ethos of counting engineers, of painstaking bookkeepers, of preaching moralists, of critical gadflies, of brave revolutionists, of honest statesmen, moderate reformers, partisan scholars, and pretentious saviors. But they cannot be scientists. Claiming scientific authority is not good for the economist, and, in a broader sense, nor is it good for other economic discourses. As long as there is a socially widespread belief in science, other economic characters have difficulty avoiding losing face and being undermined by the regime of economic science.

To make such a case is not only a matter of a new image of economics, but it is intended to have concrete political implications. Economic science, being phenomenologically unbearable, is not worth keeping up a discursive identity, and thus neither is it worth an institutional place within separate departments in academia. I mean it, politically – close them! This is not to say that all economists should find new jobs. Economists could be scattered to the winds of economic talk. They could go to other departments: to sociology (which once came as an alternative to economics), to law (this is where power belongs, doesn't it?), to politics (from which economics actually comes from), to history (where I believe it *really* belongs). Some at the religion department, too, welcome economists with open arms (at least the soft-hearted socialists). They have certainly good chances at the psychology department (recently, even in brain research!). Sure, business school – how could I forget? And those economists who really want should go serious and try to make a career at the physics department (although today there may be no economist left who did not hear of the end of this liaison), go straight to the mathematics department (though one may be disappointed how non-rigorous they have become), or set renewed hopes in a new technocratic empiricism at the IT department. Please. Go for it!

Those economists who struggle to say more than $x \in X$, to talk about more than constant elasticity utility functions, or co-integrated structural VAR's, and already gave up caring heavily about the institutions of economics in the triad of Chicago-Harvard-MIT, those economists, who share a basic skepticism about economic science, I would like to give a light shove. More, if I am right in my criticism, is not needed. No, it is not worth it to stay in the institutions of economic science. Leave them!

Why? Because the Social History of the Scientification of Economics was a History of a Misunderstanding: Gaining Authority by Saying Less

Enough of the pamphletic tone for now! The question, of course, is, how could I possibly argue for this case? Obviously, it is not a trivial task; the reality of economic science is pervasive and sits deep under many skins. But it is not an impossible task, since economic science is finite. It is an event in modern European history. This is its historical condition.

Why does it seem so radical to envision an "end" of economic science? Because economists are used to thinking about their profession, if at all, philosophically – that is, in light of better economic science. Every philosophical critique must implicitly assume some sort of standard and thus a vision of an alternative that meets the respective critique. Do I not merely assume a different understanding of what science is all about, a particular philosophy of science, that is, a particular idea of scientificaty (*Wissenschaftlichkeit*)? Do economists not simply have to adopt the standards that I implicitly apply? Could I not envision a new phenomenologically enlightened economic science? Should I not simply draw the line between economic *doxa* and *episteme* at its real joint, and then announce again: this, dear economists, is the meaning of economic science!

I repeat, the crisis of economics "concerns not the scientific character of the sciences, but rather what they, or what science in general, had meant and could mean for human existence" (Hua VI: 3, E.: 5). The Phenomenology of Economics thus does not aim or suppose an epistemic essence of economics. It is in this sense *not* philosophical. Criticism in the philosophy of science assumes that science is as infinitely flexible as reality demands, so that science can always claim epistemic authority, whatever reality there is. What is forgotten in such image of an infinitely flexible science is the finitude of science itself. What is forgotten is the concrete past that led to the theoretical interest in epistemic authority, its historical finitude. The Phenomenology of Economics shows the tensions for economists that are caused by claiming scientific authority, not *whatever* that claim may be and on *whatever* grounds it is made, but *as it happened to be* as this *concrete* claim in the last two or three centuries of European history.

The Phenomenology of Economics does not ask or suppose what economics really is. I neither scrutinize the specific modes, grounds, principles of claiming scientific authority, nor their historical change. Claiming science is not a matter of an abstract belief in particular principles that was somewhat shared by all economists of the last two or three centuries. It is a concrete practice with concrete material conditions. For this reason I do not have to say much about the definition, scope, or method proper to economics. Today, I will argue, it is even futile to pose these questions. If there were anyone to pose and to answer it, it would be the economist, not those who apply philosophy of science to economics. The adversary to normative epistemology I share with one of the great historians of science, Michel Serres, here in conversation with Bruno Latour:

> Either science must develop its own intrinsic epistemology, in which case it is a question of science and not of epistemology, or else it's a matter of external annotation – and best redundant and useless, at worst a commentary or even publicity (Serres and Latour 1995: 14).

As long as the economist does no longer pose the question of the difference of economic *doxa* and *episteme* – as it is the case – the philosopher worsens the situation when taking over and substituting the task of articulating the foundations of economic science (Düppe 2009).

Philosophical beliefs of economists, moreover, hardly played a role in the social history of the scientification of economics. For the most part of this history, economists did not make up scientific authority on their own. They simply appealed to what was elsewhere acknowledged as scientific authority, saying economics is "like science" – like Bacon, Newton, or Descartes, like mathematics, biology, physics, psychology, engineering, etc. Economic science for the most part was coasting on the success of other sciences. If I am right in my appraisal, economics could indeed never genuinely claim scientific authority in its own right. Interesting are thus not the grounds on which scientific authority was claimed, but, as the phenomenologist asks, *how* it was claimed. This does not concern the principles of science but the reality of claiming scientific authority. And this reality is a finite, a modern, and a European history. The social history of the scientification of economics began at some point in 17th- and 18th-century England with the first believers such as William Petty (1623-1687), and ends today everywhere in economics departments that became dominated by some U.S.-based institutions in the Stanford-MIT-Harvard triad. Within this historical scope, I will show, the "end" of economic science is conceivable.

The Phenomenology of Economics is about economic science, as it *happened to be*. This historical reality encompasses the intellectual becoming and aging of economists, the traditions in which they have been trained, believe, and in which they reconsider their interests, motives, and problems. Phenomenology reads as a particular historiographical style within the social history of the scientification of economics. A social history of scientification concerns the concrete, and continuous attempts to renew scientific authority in the context of other economic discourses. This history is not a philosophical history in that I drew a line at one point when economics once came to be truly science, went off track, and now waits to be informed where to go next – not once when there was pluralism before the war, when there was institutional sensibility before the marginal revolution, when there were "classical" economists who still had a sense for the meaning of the "production of wealth", nor at any other point in history when there were still genuine epistemic concerns shared by economists. I thus do not apply the standard SMMS (Smith-Mill-Marshal-Samuelson) narrative of either progress or decay. Perhaps economic science was based on a misunderstanding all along that was productive for a while but ultimately had to show its destructive force.

This productive misunderstanding of economic (pause) science was that there is something to say, a difference to be made with scientific authority. The misunderstanding, in a nutshell, is that economic science could be instituted and institutionally entrenched just because it was a way to *avoid* making an economic claim. The formalist revolution did not come out of the blue, but was the manifestation of this latent degeneration of the economic claims that are possible in science. Since then, the need for scientific authority in economic talk has been in decline. In other words, my critique points to the anachronism of the attempts in economics, let alone the appreciation of the attempts, to make a difference in economic talk. Did the attempts to be on "the good side" of economic talk, for example, not become redundant? If economics does not allow for such expressive activities, then, I suggest, economists could do better without the institutions of science.

Saying that scientific authority is phenomenologically unbearable thus translates to the historical case that the social history of scientification of economics came to an end. Since the 1950s, after the "formalist revolution", I will argue, the development of economics no longer affects the *ethos* of economic scientists. The formalist revolution was the peak and the end of the social history of the scientification of economics, and was in this sense the end of modern economic science. Further developments will not reinforce the institutions of economics. To the contrary, they contribute to their dissolution. Economic science came to be – too late to hurrah or to boo, too late to celebrate or to complain. The question now is no longer 'Which economic science?' but 'When does it disappear?'

The main ingredients of my argument, as listed in the subtitle, are thus not as mentioned above: With the notion of the "life-world" I do not suppose an ordinary or natural way of experiencing economic life that should serve in one way or another as the foundation of economic science. There is no such genuine world as the idyll of spurious experience in which we all understand each other without words. The notion of the life-world rather directs our attention to *the intellectual life* of economists in that they are *responsive* to something; it directs us to the intellectual sensibility of economists. "Formalism" neither refers to a particular more or less desirable feature of economic theory next to other more or less desirable features, but it refers to the lowering of one's tone when raising one's voice as an economist. By means of formalism the economist withdraws from the heat of other economic talk. The "invisible hand" describes similarly the lack of tangibility and materiality of the generic object of economic science: "the economy". The "invisibility" stands for the lack of an actual economic claim. It stands for saying less. The three terms together thus say: the phenomenological condition of economic science is that it can realize scientific authority only within a tendency to formalism, that is, a tendency to claim less. The history of the scientification of economics is the history of the degeneration of economic claims. Economic science "has nothing to say", and is in this specific sense insignificant.

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This text is organized in three parts: Discourse, History, and Biography. In the *Preliminaries*, I present the conceptual prolegomena, that is, the philosophy of The Phenomenology of Economics. I familiarize the reader with the basic intuition of the notion of life-world as it describes the responsivity of intellectual life, with the critical impetus of the 'oblivion of the life-world' as a historiographic device, as well as with the meaning of formalism and the invisible hand. These three parts, which constitute the body of the text, progress with a rising awareness of the phenomenological problem of economic science. The first part (*Discourse – The Public, Professional, and Pedagogical Ethos of Economists*) begins with the naïve attempt at a simple determination of the social relevance of economists in an informal and descriptive fashion, as though there were no phenomenological problem at all. It has a largely heuristic function, and is also written for non-economists. The remaining two parts are historical in nature. The second part (*History – Economic Science from the Oikonomia to "the Economy"*) presents a grand narrative of economists' intervention in modern history. The critical question I pose here is this: What was the motive of the scientification of economic writings? And what happened to this motive over the course of this scientification? The answer circles around a tendency toward formalism that set off in the political discourse of 17th century England and culminated in the formalist revolution of the 1950s. Finally, the third part (*Biography – Gerard Debreu from Nicolas Bourbaki to Adam Smith*) exhibits the actual experiential problematic of economic science following the biography of a suitable case, Gerard Debreu. It reads as a transcendental parable for the moral end of the ethos of the economic scientist.

As is already apparent in this short sketch, the text cannot be reduced to one particular point of view, but is rather a variation of views. It encompasses, in the words of Husserl, the inner- and outer horizons of economics. It is Big History that encompasses the before of economic science and envisions a time after. But it is also Small History that includes the most incidental and fleeting but nevertheless constitutive moments of science. I skip through that in which others invest most effort, while I remain patient at points others skim through. The text leaps and pauses, and in general favours the non-specialist.

If one keeps in mind the increasing virulence of the problem of economics that accompanies the three parts, they can be read in the order most appealing to the reader. The Preliminaries stand apart, and can be read at any point. In particular if phenomenology is new to the reader, one may prefer to postpone them. The text is designed in such a way that all readers will find passages addressing issues with which he or she will be better versed than I. I am far from being an expert on all issues I address. But the text is also designed in such a way that all readers will find links to other passages that they have not yet considered. It is the task of reading to find the middle ground on which a discussion can take place.

Preliminaries

The Phenomenology of the Life-World – Hermeneutics, History, and Transcendentality

When bringing together two traditions so far removed from each other as phenomenology and economics, I do good to remind one of the main gestures of phenomenology as a philosophy of 20th century: the priority of possibility over actuality. Heidegger, at the occasion of explaining the dedication of *Sein und Zeit* to Husserl, and apologizing that his notion of phenomenology went beyond what Husserl would identify as his philosophy, referred to this priority:

> The following investigation would not have been possible if the ground had not been prepared by Edmund Husserl, with whose *Logische Untersuchungen* phenomenology first emerged. Our comments on the preliminary conception of phenomenology have shown that what is essential in it does not lie in its actuality as a philosophical 'movement'. Higher than actuality stand possibility. We can understand phenomenology only by seizing upon it as a possibility (Heidegger 1962: 62 f.).

The same can be said about The Phenomenology of Economics in a twofold sense. For one, phenomenology is not a "program" applied to economics. As seen from the history of economics there are hardly any interfaces that could motivate such an "application" (apart from rather marginal economists such as Walter Eucken, Edgar Salin, Othmar Spann, or the students of Alfred Schütz). The actual tradition of phenomenology and its conceptual jargon thus stands back for what economics shows from itself.

This is not an excuse to avoid the sophisticated heights of phenomenological conceptualizations. Instead, the priority of possibility over actuality gives us a hint what economics can show of itself, namely the possibilities the tradition of economics seizes upon in order to become an actuality in our world. The Phenomenology of Economics, in other words, is the attempt to let the tradition of economics show in such a way that it testifies the experiences from which its thought gains force. With this interleaving of, say, the pathos of thought and the ethos of experience, I associate the notion of the life-world.

In the following I will present a preliminary conception of phenomenology as it is going to inform my account of economics. What phenomenology can mean for the reader at the end, he or she has to find out by going through. Here, I repeat, instead of presenting a conceptual apparatus, I can bring the reader closer to the sort of reflection that will be required to appreciate my account of economics. Phenomenology in the title refers thus to a free adaptation, or better, as Husserl would have said, a *renewal* of the reflection associated with the notion of the life-world.

In the last years of his life Husserl's efforts circled around the themes of modern science and the life-world. For him, it meant yet another attempt to "introduce" phenomenology in a renewed fashion. Although he used the term of the life-world already before occasionally, it found one of its most insisting expressions at a talk in May 1935 in the *Wiener Kulturbund*. At that time he was no longer allowed to speak in the *Reich*. The title was weighty: *The Crisis of European Mankind and Philosophy*. From this talk sprung his last publication before his death in 1938, *The Crisis of the European Science and The Way into Transcendental Phenomenology*, or short, *The Crisis* (Hua. VI with additions in Hua. XXIX). These texts serve the backdrop of the following exposition of phenomenology.

Husserl's discussion of the life-world can be organized along three interrelated concerns. First, the explicatory question, in which sense is the life-world *pre-given*, and what does it mean that science *presupposes* the life-world? Second, the critical question, what does it mean that modern science *forgot* the life-world? For Husserl as a late modernist believer of science the horizon of the entire exercise was, third, his vision of a *phenomenological science* that overcomes the crisis of modern science.

Within these concerns my text reads as a supplement for the open case of economic science. This case is based on the following adaptation of the three questions that will occupy us in the five preliminary chapters. I explicate the basic meaning of the pre-given life-world for all intellectual life and science along the transcendental notion of *intellectual responsivity* (1). I oppose the concern for the concrete theoretical experience that is implied in this notion to what I call the hermeneutic notion of the life-world. This distinction also helps the understanding of the relation of a phenomenology of science with the two prevailing approaches in the commentary of science, the philosophy of science and science studies (2). I take up the critical theme of the oblivion of the life-world as a historiographic guide for re-writing the history of economics as the oblivion of the motives that give rise to it (3). Decisive point of my adaptation of this critical aspect is that the oblivion of the life-world in economic science does not refer to its objectivism, but rather to its formalism (4). Regarding Husserl's third concern for a phenomenological science, that is, regarding the question whether I can envision an economic science that meets my critique, I am skeptical because the condition of a scientific claim roots in the theoretical perception of the "invisible hand", which excludes the reflection on economic life (5).

(1) Life-World and Intellectual Responsivity

Phenomenology, just as most other philosophies from Descartes to Deleuze, is a philosophy of sense and sensible life. In its post-Kantian (and also post-Hegelian) impetus, it is, more specifically, a philosophy of immanence, saying, that the constitution of sense lies in itself rather than being determined by a philosopheme of another kind such as a category, a concept, or any form of determination or even justification. Sense is not a matter of being constituted by something beyond itself, but a matter of being concrete and actual. Sensible life is the sense-achieving life (*sinnleistendes Leben*), which is one of the weighty expressions of Husserl's writings.

The emphasis on experience in phenomenology comes from this concern for the immanence of sensible life. What is a hammer we know by hammering, what is sense we know by sensing, what is perception we know by perceiving, what are affects we know by being affected, what is a landscape we know by viewing, passing and wandering through. The critical impetus of such a principle was, in a nutshell, that in sense-achieving life there is no space for a philosophical split between what constitutes and what is constituted, between the transcendental and the empirical, between sensibility and spontaneity (Kant), between concept and reality (Hegel), between intellectual and sensible life. Instead, there is a *correlation*, that is, a being assigned to and implied in each other. Just as a feeling cannot be separated from the feeling of it, the world cannot be separated from the living of it. The life-world as the encompassing motif of the entire phenomenological tradition refers to the being enmeshed of the sense of the world in our sensible life. It is this commitment to immanence that makes phenomenology (next to the philosophies of difference) a philosophy of the 20th century.

Within such a general commitment to immanence, I want to place Husserl's notion of the pre-giveness of the life-world. I read it as a notion with which the immanence of intellectual life can be reflected upon. The life-world is the pre-given world for all intellectual life in that it informs, or better: motivates intellectual life. By means of being motivated intellectual life can be an achieving life (Hua IV: 220 ff.). The life word is the locus from which an intellectual life is motivated. Regarding science – clearly a prominent manifestation of intellectual life in modernity – the life-world is that instance through which science can give a response, the world that allows for *intellectual responsivity*. Intellectual responsivity is the corner stone of my adaptation of the life-world as a notion that discloses the concrete experience of science. I employ this notion mainly in order to disclose scientific experience as the locus of critique. I will thus hardly address questions concerning the *genesis* of the life-world explicitly (Hua.

XXXIX, Lee 1993, Steinbock 1995). They do, however, represent the horizon of the critique how economic science deals with economic life. For after all, economic life is a primordial way of world-becoming (*Weltwerdung*).

Here the conceptual outlook: Intellectual responsivity has two connotations, that of *sensibility* and *responsibility*. Intellectual sensibility may seem an odd expression for any reader of Kant and the rest of those who believe in the analytical-empirical divide. It will yet guide us through the description and the critique of the experience of scientific practices in economics. Intellectual responsivity not only describes but also represents the possibility of being guided by an intellectual value. It is the possibility of *intellectual responsibility*. The life-world warrants here the possibility of "justification" in that it constitutes the need of justification (rather than a philosophical principle of it). In this "aesthetical" (the Kantian word) and "ethical" aspect of responsivity, the life-world can be said to be the locus of the *significance of science*. The Phenomenology of Economics is thus not more or less than an enquiry into the constitution of the significance of economic science. It is in this conceptual horizon that my critique of economic science will move, and it is on this conceptual horizon that I will conclude. In this chapter I cannot present more than a preliminary exposition of this horizon.

The Basic Notion of the Life-World: The Lateness of Science and the "Huge Piece of Method" of a Phenomenology of Science

What then does it say that the life-world is pre-given? What kind of priority or precedence does it address? Husserl's thesis of science presupposing the life-world means that science is phenomenologically *late*. Before there is an interest to adopt a scientific attitude, before a scientific practice can be instituted and carried out, already a lot of sense-labor (*Sinnarbeit*) had to take place. Science is not "self-made". Before we are able to claim scientific authority, we already acquired a great deal of the world by means of such acts as reminding, associating, apprehending, anticipating, expecting, being attracted, driven, repulsed or appealed, and other "primordial" forms of motivations that make experiences "lived" (Hua. XI). An epistemic interest is something that has to become. It has to grow. The practice of science is phenomenologically old. Science has a *past*.

Husserl and his followers often describe intellectual activity as a reflection (*Besinnung*) - another weighty word of the phenomenological tradition. In one of the core paragraphs of *The Crisis*, §15 (*Reflection on Method of our Historical Manner of Investigation*) Husserl describes reflection as a *responsible critique*.

> Only in this way [of a responsible critique] can we, who not only have a spiritual heritage, but have become what we are thoroughly and exclusively in a historical-spiritual manner, have a task which is truly our own (...). This manner of inquiring back into the ways in which surviving goals, whose unsatisfactory character again and again necessitates their clarification, their improvement, their more or less radical reshaping – this, I say, is nothing other than the philosopher's genuine self-reflection on what he is truly seeking (worauf er *eigentlich hinaus will*) (Hua VI: 72 f., E.: 71).

The past of science, therefore, is not of an epistemic kind that one needs to consider in addition to one's present issue for the sake of rigor or completeness. The past of science is

Phenomenology of the life-world: exegesis or renewal?

The notion of the life-world was for Husserl a working title rather than a program, and was thus coined by ambiguities that gave Husserl scholars material for years to come. It encompassed, apart from the cited work, other posthumously published works. Important to mention are the *Ideas II* (Hua IV), from which I take the concept of *motivation* as a tenet of transcendental life; his *Analyses of Passive and Active Synthesis* (Hua. XI), where the Kantian distinction of transcendental aesthetics (sensibility) and analysis (spontaneity) is dissolved in the idea of a stream of consciousness that is genetically spelled out along the acts of association, retention, apperception, etc. The work edited by Ludwig Landgrebe *Erfahrung und Urteil* (1975), the third volume on *Intersubjectivity* (Hua XV), as well as the most recent volume Hua. XXXIX gather vital notes on the constitution of the life-world. For the development of Husserl's notion of the life-world, see Kerckhoven (1985), and for a clarification of its basic ambiguity between a foundation of science and a concept of phenomenon, see Claesges (1972) and Kern (1979).

Husserl presented the notion of the life-world as an alternative way into the phenomenological reduction next to the Cartesian, egological way (Luft 2004). Yet it never found a fully elaborated shape. The notion thus also represents the line between static and genetic phenomenology (Hua XI: 339 ff., Lee 1993). Concerning science, in static phenomenology, Husserl still aimed at a *foundation* of a strict and eidetic science, while in genetic phenomenology he aimed at a *genealogy* of science and judgments in general. This half-taken turn of the late Husserl is contested among Husserl scholars. Some stress the continuity of his early and late work and emphasize that the notion of the life-world is meant to account for science without affecting his notion of transcendental subjectivity as such – that is, it remains within the constitutional paradigm of intentional consciousness (see for this interpretation Held 1991, Mittelstraß 1991). In our context it is important that only in departure from such a foundational notion of the life-world can one view the life-world as a pragmatically founded structure as it was popularized by Schütz and Luckmann's *Structures of the Life-world* (1980). This structural notion results in a normative epistemology of "common sense", which was also utilized by Habermas when speaking of discursive communities (1985). For reasons that will become clear later, I call this notion of the life-world "hermeneutic". I also trace this notion in the works of Kuhn, Hacking, and Foucault, in that they refer to a historical apriori (paradigm, style, episteme). This hermeneutic notion functions only as a heuristic step in order to introduce the problematic of science (see the next chapter).

Other Husserl scholars have taken the turn of the late Husserl further and elaborated its genetic, non-foundational character. Life-world here is not a matter of intentional constitution, but replaces intentionality. Intentionality is "historicized" and itself made object of a transcendental genesis. Transcendental subjectivity is thus constituted from its own achievements, which results in a transcendental materialism. This road of interpretation was taken by first-generation Husserl scholars, who did not share Husserl's foundational concerns, such as Ludwig Landgrebe (1982, 1977), Eugen Fink (1927), and, of course, Heidegger (1962). Life-world triggered a reconsideration of the transcendental notion of the world as a horizon as such, and thus the "being" of intentionality. From this point of view, Heidegger's *being-in-the-world* (1962), Merleau-Ponty's *being-to-the-world* (2002), or Lévinas' *interior world* (1979) are all renewals of Husserl's late turn (see further Steinbock (1995), who advanced the idea of historicity to a *generative* phenomenology).

The hermeneutic notion functions for the present endeavor as a heuristic starting point, with the genetic notion as its horizon. The latter will only now and then come to the fore (such as in the discussion of the mathematical experience of Bourbaki). As an introduction to the phenomenology of the life-world, the English reader may consult the monograph of the translator of *The Crisis*, David Carr's *Phenomenology and History* (1974), and, more recently, a likewise profound study by James Dodd, *Crisis and Reflection* (2004). Considering this background, a phenomenology of the life-world, though seemingly an anachronistic project, can serve still today as an innovative input into the commentary of science.

where the motivation of science comes from: One can only have a intellectual task as long as the provenance of one's interest is still present, informs, and necessitates one's intellectual effort. Indeed, intellectual efforts are nothing but the *presentiation* of a past (*Vergegenwärtigung*). Hence thought has always the character of remembering. Thought is not a faculty of synthesis (Kant) that supplements the rest of life, but is a task to be accomplished. Being responsive means that at each point of our intellectual projects we "remember", are still-in-the-grip-of, still-hold-in-grasp, 'presentiate' in the broadest sense what once has informed this engagement.

As a result, to have a grip on something, as it were, cannot be separated from being in the grip of something. Only a responsive science can be *responsible* and produce actual claims that the scientist is willing to explicate in light of the past that gave rise to it. Science only achieves something insofar as its practices correlate with a 'necessitation' of clarification, improvement, reformulation, etc. Science having a past is to be pulled along, to follow while remembering, that is, to have a horizon. In other words, life-world is the title of the bond of intellectual life with the motives that give rise to it. More trivially, the basic reflection is to be able to ask at each point of intellectual life: How did this or that become interesting? How did I get there? What brought me to it? What am I (truly) up to? Which reduces to the question: What does this mean?

Decisive for this image of intellectual life is that the tardiness of science is not of an epistemic kind. It is misleading to say that science *presupposes* the life-world. The pregivenness of the life-world is not that of a presupposition or assumption. The life-world does not describe an epistemic limit like a set of basic beliefs that science never can critically inquire – as in Kant the supposition of the "thing in itself". One cannot by any means or in any sense "derive" science from the life-world. The past of science is not a stock of belief that grants closure to intellectual life. It is not a 'historical apriori'. In this fashion the life-world is often equated in the hermeneutic tradition with a pre-interpreted world, a discursive structure (Habermas), a discursive formation (Foucault), a historical paradigm (Kuhn), an anthropological field, or even a cognitive "mental map". I cannot say, for example, that science abstracts from the life-world as it abstracts from the quality of a thing in order to consider another aspect of it. The life-world cannot be abstracted from; as the past of science it only can be forgotten. Science and life-world are not two separate contexts existing next to each other and related by means of a hermeneutic play of meaning (the dialectic of pre-understanding and projection). Life-world is neither a title of different systems of meaning, and does therefore not reduce to a (positive or normative) conception of ideology. The consequences of this demarcation will haunt us down to the conclusions.

As opposed to its presentation in the hermeneutic tradition, the life-world is opposed to any form of *closure* of science. Husserl's idea of the life-world was *not* to draw a line between science and the rest of life: between *doxa* and *episteme*. The life-world is, as Husserl's often-quoted phrase goes, the 'seat of science in life'. With the life-world Husserl aimed at undoing epistemic hierarchies and showing how the doxa is indeed necessary for any episteme to be established. *Science can never know more than what the doxa can believe*. Only within this continuum science can be conceived as a truly human practice. Husserl even believed that only as long as there is a continuity between doxa and episteme can science truly be what it originally was supposed to be: a *telos* of European (read, Greek) mankind - as he expressed his modernist belief in science in 1937!

Is there, then, any conceivable difference between science and the rest of life? *Episteme* is a particular *doxa* to the extent that science manifests what is latently present in all human practices, in all acts of consciousness: being responsive to the demand of meaning. In every moment, in each act, even while asleep, to live amounts to the same as *to sense*, to be sensitive to what has been and will come, to the various sides and "shadows" of the things we perceive, that is, to sense the *demand* of carrying through our lived experience. We cannot open our eyes without being already enmeshed in the play of meaning that puts us into charge. And the same is true for science. What distinguishes intellectual from the rest of life is a higher degree of self-articulation, self-understanding, and thus self-responsibility. Only a responsive science, according to Husserl, can be responsible for what it claims since it knows, or better: remembers its own conditions. Only then science can account of itself.

This demanding character of experience (the "call" in Heidegger) is meant with one of the tenets of Husserl's philosophy that the being of consciousness is intentionality. Intentional life remains always preliminary, pending, and patient. Science is then that intentional practice, which endures the demand of sensible life patiently, which is not easily distracted, but follows unwearyingly moment for moment the play of meaning. Justification requires thus to remember and to narrate the passage that led to a claim; it is to account for the "history of sense" that motivated a particular claim. Reason, in this view, is the capacity to be in the grip of and to be guided by experience. And so science is for Husserl of highest dignity, and has not only a seat in life, but a throne (see Dodd 2004: 27ff).

The life-world, in other words, is never "in itself" (*an sich*). It is "horizon". It refers to the ambiguities of the world *after* which comprehension, understanding, (re)cognition, certainty, clarity, truth, and whatever epistemic values haunt intellectual life, can be considered as a task in the first place. The life-world as a transcendental notion proper is the world through which there can be an epistemic interest. Such notion informed Husserl's perception of the 'huge piece of method' of a phenomenological critique of science, of the "tremendous task of a true and genuine philosophy of science" (Hua VI: 398*). In the context of the mathematization of the natural sciences, he elaborates:

> The researcher of nature does not make clear to himself that the constant fundament of his – after all subjective – work of thought is the surrounding life-world (*Lebensumwelt*); it is always presupposed as the ground, as the field of work upon which alone, his questions, his methods of thought, make sense. Where is that huge piece of method to critique and clarification [-that method] that leads from the intuitively given surrounding world to the idealization of mathematics and to the interpretation of these idealizations as objective being? (...) [H]ow formulae in general, how mathematical objectification in general, receive meaning on the foundation of life and the intuitively given surrounding world – of this we learn nothing (Hua. VI: 343, E.: 296).

To understand the meaning of economic science, accordingly, I have to embrace a long way between those experiences that give rise to economic theorizing and the act of bothering about existence proofs and robustness tests. A phenomenology of science proceeds, in Husserl's words, as a "retrogression to the life-world" (1975: 41 ff.). It is a regressive analysis (not to be mixed up with Foucault's "archeology"), an analysis along the question: 'What must have been already accomplished in our sensible life so that an interest in science can be motivated?' Husserl spoke of this "regressive analysis" as the 'digging out of buried sense-accomplishments' (*Ausgraben verschütteter Sinnesleitungen*). The answer does not result in a criterion that tells *doxa* from *episteme*. But it exhibits and makes intelligible the possible meaningfulness of epistemic life. The phenomenological meaning of science does not lie in the features that describe its results, but in the attitude one has to adopt in order to find interest in these results.

As Husserl asked what had to be accomplished in terms of the perception of the world that such a scientific thing as "nature" could appear, I will ask respectively: What had to happen that such a scientific thing as "the economy" appears? While Husserl explicated his answer in the history of science along the mathematization of nature in Galileo, I will explicate my answer along the history of claiming scientific authority in economics that culminates in the mathematization of "the economy" by Gerard Debreu. At the heart of this critique of economic science is the question: is a responsive economic science possible?

As introduced *in nuce*, the notion of intellectual responsivity remains abstract. Since responsivity is the operational concept of the following exercise, we will fully grasp it only by means of the narrative it sets free, that is, by means of the problem it informs in economics. For a first intuitive understanding, let me list some of the questions that I will deal with:

How do economist have to be motivated? Which attitude do they have to adopt in order to be affected by economic science? How can economists be "with" their theory as a subject of their practice? What is the locus from which and the ethos with which an economist speaks? What is the scientific attitude of economists? What is the integrity and sustainability of this attitude? In which way are intellectual values manifest in economists' practice? Do they inform their work, or do they remain unexpressed? Or, as recently also Klamer (2008) attempted to answer, how do you get to see yourself as an economist? In the words closer to Husserl, what is the "act of meaning formative for the experience of scientific thinking" (Dodd 2004: 7). Or, as historians of science such as Lorraine Daston came to ask in the last decades, what are the concrete historical conditions for a commitment to abstract intellectual values such as rigor and objectivity to evolve (2000)? Or, in terms of Heilbroner and Milberg, how can economics "generate the resonance necessary for a fruitful vision?" (1995: 113) What moves the economist? What keeps the economist doing science? How could one possibly care about economics? How can economics be significant?

Reflecting on the Significance of Science

The Phenomenology of Economics is a critique of the significance of economic science. Husserl used this term at the occasion of stating the crisis of science in the opening of *The Crisis*. The crisis "concerns not the scientific character of the sciences (*Wissenschaftlichkeit*), but rather what they, or what science in general, had meant and could mean for human existence" (Hua VI: 3, E.: 5).

> In our vital needs – so we are told – this science has nothing to say to us. It excludes in principle precisely the question which man, given in our unhappy times to the most portentous upheavals, finds the most burning: questions of the meaning or meaninglessness of the whole of this human existence (Ibid.: 5, E.: 7).

Modern science is insignificant because it "has nothing to say". Because of such lines, the notion of the life-world became associated with the significance of science. In Husserl's text the notion of significance set the tone, but as a concept remained operational and not explicitly discussed. What then does it mean to reflect on the significance of science?

Clearly, the predicaments of such a reflection in science are against me. For a scientist, to speak of the significance of science cannot be more than blurry talk. Is significance not beyond the possibility of all reasoning? Is it not one of the basic assumptions of science – of its autonomy, as it were – that everything could possibly be made an object of enquiry regardless of the sources of the interest that gave rise to the recognition of the issue? Is not everything virtually significant, even if in an unknown future? Particularly economists may deter from such a reflection. An odd form of pragmatism too often functions as an excuse to engage in matters of importance. Is it not a surprising fact that the limits *within* economic theory (rationality) as well as the limits *of* economic theory (modeling) are excused with the same truncated pragmatism: it depends on what you want?

Economists may have another association with the word "significance". It is used in the context of statistical testing of theoretical hypotheses. Since statistically speaking, everything is connected with everything one needs to cut the edge between considerable (read, reportable, publishable), and negligible results. In order to do so, one speaks of "statistical significance" on the basis of a specific "t-value". McCloskey charged economists since more than two decades to reduce economic significance to these statistical standards (1985b, McCloskey and Ziliak 2008). What is statistically insignificant can be economically significant and vice versa. Statistically small can be economically big. The risk of equating them is the automation of intellectual activity. Statistical significance is an excuse for putting intellectual *effort* in assessing the significance of a correlation *in concrete*. As soon as intellectual activity is standardized, in Husserl's terms, science looses touch with its task. McCloskey's critique thus points into the right direction: The question of economic significance cannot be answered once and for all, just as something can be significant only as long as it is in question.

The exclusion of a reflection on significance describes the gulf that separates the scientific attitude from a phenomenological reflection on science. In modern science a claim gains validity only if one does *not* present it in dependence on an act of theorizing – other scientists could have made this claim too, others should agree when following the evidence too, etc. "Someone who is raised on natural science takes it for granted that everything merely subjective must be excluded" (Hua. VI., 343, E.: 296). Having a grip on something, as the dictum of modern science goes, is precisely not to be in the grip of something. Science has to hide its own particularities behind its claims in order to claim generality. Science has to be aloof from its own reality in order to claim authority over reality. The significance of science is thus operational in science. The phenomenological problem of modern science is then that it excludes *to reflect upon the motives that give rise to it.* But only through that which gave rise to scientific practices, science can understand *itself* in its claim to knowledge. Modern science thus

> lacks precisely the knowledge of what is most fundamental, namely, the knowledge of what could procure meaning and validity for the theoretical constructs of objective knowledge and [which] thus first gives them the dignity of a knowledge (Hua XI.: 121, E.: 119).

Modern science comes down to an "idolization of a logic that does not understand itself" (Hua VI.: 193, E.: 189). Or, in the words of a commentator of Husserl's *Crisis*:

> The apparent meaninglessness of science is not due to a lack of content as such (…) Science has a great deal to say, but it is unable to speak in such a way that the importance of what it says, the significance of its truth is sufficiently clear, even to those who are open to it. The result is that the world has itself become unclear, precisely in the form it has taken as something which has been articulated, or understood, by science. Its very evidence, secured by science, fails to compel (…) This leads to a paradoxical situation – the successful understanding of the world, of its articulation in concepts, strikes most as empty, as not addressing what an understanding of things needs to address. This means that the very way that we comprehend things has become "incomprehensible" (Dodd 2004: 209 ff.).

By means of dwelling upon and deepening this tension between modern science and its possible significance my account of economics will be critical instead of descriptive of the practices of economists. Like other critiques of modern science, phenomenology points to a blind spot in the constitution of science. There is a constitutive instance that has to remain hidden for science to be able to claim authority. This blind spot is nothing but the scientist.

What then is it to reflect on "significance"? Apart from Husserl's loose use, it was conceptualized by Heidegger (as *Dasein's mode of relatedness*). But more telling may be Dilthey's use of the term. In a Neo-Kantian fashion of a tripartite of reason, practice, and feeling, he describes significance as "what is coercive or determining within self-consciousness, which is evidence for cognition, that is, 'ground' of knowledge, the 'ground' of motivation for practice (*Beweggrund*), and the 'ground' of gratification for emotions" (GS 19: 57*). Significance, in my terms, is the concretization of the meaningfulness of meaning. What makes meaning meaningful is not its particular function within statements (as in Kant), but it lies in the achievement of sense (*Sinnvollzug*), that is, in the act of carrying out – with which Husserl anticipated one great gesture of 20th century philosophy to think beyond identity and structure. Significance can then be characterized as the *affective weight* of an act. Any act, whether practical or intellectual, correlates with an effort to be made. By means of these efforts, acts carry out what precedes them as well as what they disclose. Significance is thus what makes us remember and what lets us question further. It is the title of that, which gives something to say.

Saying that science is significant is to say that science has something to bear. Science presupposing the life-world has to bear the *weight of meaning*. It is *not* free in choosing what could be said and what not. *There are* things to say. If what possibly is significant is left to the freedom of research, science easily ends up having nothing to say. Then science forgets the life-world since it is enmeshed in the world that it pretends to apprehend from a distance. The life-world puts the scientist into question instead of providing safe ground. It describes the need for orienting and locating ourselves at each moment of experience - the world that, as it were, never ceases to appear behind the horizon. And only as such the life-world gives us something to say since not everything has already been said. The life-world is associated with the realm of intelligibility not because it represents basic truths, but because it commits us to intelligibility.

The life-world is epistemologically prior only in the sense of constituting an epistemic problem, not an epistemic hierarchy.

The life-world as just described is a transcendental notion. Rather than an object of intellectual life, the life-world "constitutes" intellectual life. Husserl put the entrance door to all transcendental phenomenology in the short words: "The world (…) does not exist as an entity, as an object, but exists with such uniqueness that the plural makes no sense when applied to it" (Hua VI: 146, E.: 144). The life-world is not like the encompassing container of contexts that enable us to understand each other as long as we are in it. The life word, if I take Husserl's phrase serious, is not even a correlate of a consciousness, but, as one commentator said, the "correlate of the general life-care" (Lee 1993: 148*). As the correlate of life-care, the life-world is the locus of our sense of significance, and the carrier of the weight of meaning. The life-world is then not constituted by the interest we have in the world, but the life-world is constitutive for any interest in the world. As opposed to any forms of hermeneutic and structuralist critiques of science, I do not consider science as a particular way of viewing the world. Rather, I am interested in the world that *institutes* the necessity to view the world scientifically. We *have* a world in the sense of a "passive having of a world" (Hua XI.: 110, E.: 109) *before* there are objects in which we can have an interest. And this world is the life-world. Here the supporting lines of Husserl.

> It is clear what makes for the radical distinction here. The life-world is the world that is constantly pregiven, valid constantly and in advance as existing, but not valid because of some purpose of investigation, according to some universal end. Every end presupposes it; even the universal end of knowing it in scientific truth presupposes it, and in advance; and in the course of [scientific] work it presupposes it ever anew (VI: 462, E.: 382).

This step from the life-world as constituted-by to being constitutive-for our interests can be understood as a consequence of a basic phenomenological thought that sense is prior to reality. The life-world is not the correlate of a particular interest, but the world *in, within, and from which we can have an interest.* Science presupposing the life-world means that it is *the condition of the possibility of being significant.* And so the main question of The Phenomenology of Economics is not what *is* the significance of economics, but could it possibly be significant? Without this transcendental notion we will not be able to appreciate the conclusion I have announced.

The peculiar character of this transcendental notion of the life-world is that it does not exclude an actual engagement with the materiality of science. It does not exclude the empirical, as opposed for example to Kant. In Kant the "transcendental deduction" of categories like "causality" and "necessity" were supposed to be free of "empirical terms". For this reason Husserl charged Kant for his unintelligibility and formalist tendency (Hua VI: §§ 30 ff.). The life-world, instead, is both condition and the concrete materialization of science. This represents the post-Kantian impetus of my phenomenology of science. The simultaneity of the empirical and transcendental gives phenomenology its peculiar taste that may appear like an irritation on the first glance, but has it potential in the disclosure of a renewed sensibility.

Such transcendental materialism Emmanuel Lévinas acknowledged in a rather untypical moment of his writings:

> The method practiced here does indeed consist in seeking the condition of empirical situations, but it leaves to the developments called empirical, in which the conditioning possibility is accomplished – it leaves to the *concretization* – an ontological role that specifies the meaning of the fundamental possibility, a meaning invisible in that condition. (1979: 173)

Transcendental discourse, as it is so fascinating and challenging of phenomenology, does not have to be carried out as an abstract philosophical discussion of categories, but can be carried out within the *concrete history* of acts of consciousness.

Summing up the preliminary exposition of my philosophical intuition, the reader may remember the following. As long as we live the world, we are taken into charge: the life-world is not the world that "makes" sense, but is the locus of the need of making sense. The life-world refers to the "twilight of comprehension" (Dodd 2004; 175), which keeps epistemic claims ambiguous, commits us to further questioning, and in this sense describes the finite reality of science. The life-world is what keeps us from judging the world and thus allows us to "have something to say". To fully understand "is to grasp what is problematic about it, what is being risked and for what, thus what conflicts and decisions have defined it as a concrete life experience" (Ibid.: 55). All discourse, according to Husserl's idea of the multi-layered intentional life, is a response to the lived character of experience, that is, to the affective weight of experience. This is the phenomenological ground of the possibility of intellectual responsivity. The life-world, if I had to provide a definition, is the phenomenological locus from where the problems come from. The hold that the life-world has on the scientist, which makes science bound by something, is the problem.

(2) Scientific Practice and the Hermeneutics of the Life-World

While the basic intuition of the notion of the life-world is intellectual responsivity, the object of concern of my phenomenological critique is the *concrete practices of claiming scientific authority* in economic talk and writings. Bringing these practices to the foreground directs our attention away from the principles by which the scientist draws authority – as in the philosophy of science. It neither directs our attention to the explanation of scientific practices – as in science studies. It aims, instead, at the concrete conditions of the significance of economic science. In this chapter, I explicate this status of scientific practices in comparison with the role they occupy in these two prevailing approaches to science. In association with science studies, I then introduce the hermeneutic, as opposed to the transcendental, notion of the life-world.

The Subjective Accomplishment of Scientific Authority as the Establishment of an Ethos: The Seat of Science in Life

In the phenomenology of science, science is understood from the point of view of science-conducting life, that is, from the "subjectivity which accomplishes science" (Hua VI: 343, E.: 295). Such approach follows from the basic concept of Husserl's phenomenology: intentionality – not to be confused with the use of this notion outside phenomenology, where it has mostly voluntaristic connotations (see Husserl 1975: 85 ff.). Science, rather than a body of representations of something beyond itself, is, as Husserl would say, the intentional correlate of an act of theorizing that constitutes scientific theories. In a phenomenology of science, science is "exhibited" (*ausgewiesen*) as the intentional sense constituted by theoretical practices. The idea of the pre-given life-world of science can be understood as a concretization of the meaning of this intentional constitution of science.

Science is a subjective accomplishment. What motivates this insight is surely not the recognition of a limit of science – such as the theory-ladenness of observation. It says that even the most general or abstract theory can only be understood through the unique course of efforts that accomplish it. To speak of the subjective accomplishment of science is to focus on the necessity of someone to stand and carry out science: someone needs to keep track of, has to be with, and has to go through the possible claims. To be a subject of science is "to give one's voice" to science. Science thus cannot be viewed from its result – a body of knowledge, in which the history of sense that brought a claim about etiolates in the archive. What motivates this insight, though, is not a romantic desire for soft edges in hard science. Subjectivity of science is constitutive for the possibility of such "hardness". If science is a subjective accomplishment, then what it claims does not deposit in anonymous truth that may or may not turn out to be the case. The truth of a claim is a "claim on me", as a recent commentator of Husserl elaborates:

> Thus to reflect on the possibility of making the claim myself, in my own voice, not only brings the truth of a proposition into question, but it also brings my self into question as well – for the question here takes the form: what would it mean, to be the one who would make such a claim (Dodd 2004: 9).

The question to be posed to the economist when speaking about the subjectivity of science is thus: How is it to speak on behalf of economics? How is it to speak *as* an economist? I will discuss this demand for a responsible subject of science with a more resonant notion, the *ethos* of the scientist. What economists accomplish with their concrete subjective engagement in science is not a set of anonymous prepositions, but an *ethos* through which they can claim truth.

There is indeed a close match between the meaning I gave until now to the notion of the life-world and the notion of an ethos. In ancient Greek thought, most generally speaking, ethos designates a place: the places of habits, customs, and dispositions. Ethos also includes the customary objects of that place – cultural objects. Having an ethos is to have an appropriated world we can rely on, and in return the world through which we can be reliable beings. By acquiring an ethos we become articulated beings, and thus also addressable beings. Specifying the subjective accomplishment of science as an ethos, I thus account for the social and discursive dimension of intellectual life that Husserl too often and too easily ignored (Derrida 1989). The ethos of the scientist describes the "seat of science in life". Occupying such a seat is an "ethical" question of taking intellectual responsibility for claiming epistemic authority.

In Aristotelian rhetoric, ethos has a more narrow meaning (1926: 17). It refers to one of the three modes of persuasion, along with logic and pathos. One persuades by the cogency of reason, by appeal to emotions, and by personal identity. Missing one of them, according to Aristotle, gnaws on one's credibility. Ethos refers, for Aristotle, to the discursive identity of the speaker that grants reliability and credibility. Ethos is what allows one to raise one's voice with weight and thus to demand that others listen. It refers to the attitude, the tone, and the posture with which one speaks to others. In other words, an ethos describes the social relation of the speaker to an audience. It originates equally in the self-understanding of the speaker, and the image the audience holds of the speaker – which may be very different. An ethos can be viewed as a discursive manifestation of a speaker's past insofar as it grants authority, status, integrity, morale, and also expertise. The ethos of the economic scientist, respectively, refers, in a catchier expression, to the social, historical and discursive incorporation of the claim to scientific authority. What someone says, who says it, and who listens are inextricably related, as Shapin too took as a precept of his Social History of Truth:

> What we know of comets, icebergs, and neutrons irreducibly contains what we know of those people who speak for and about these things, just as what we know about the virtues of people is informed by their speech about things (1994: xxvi).

Considering the subjective accomplishment of claiming scientific authority, I thus ask: What is the ethos of economists, and how do they acquire it? What is their integrity? How are economists "with their claims"? What kind of person do economists have to be in order to give voice to an economic claim and defend it? Which past grants them expertise, credibility, authority, and morale? How does one come to see oneself as an economist, and how do others come to support this self-perception?

In order to give a first glimpse as to why these questions could become virulent for the economist, consider that just for the reason of not being assigned a place, of not having an address, of not being reliable, of not sharing a past with others, merchants in the premodern world have been denied any "ethos", be it professional or political. How then could the economic scientist gain professional or political ethos?

How the Practice of Science is Forgotten in the Philosophy of Science and Naturalized in Science Studies

Before elaborating further on this notion of the subjective accomplishment of science and the ethos of scientists, let me first gain some common ground by comparing this phenomenological approach with two prominent genres in the commentary of science: *philosophy of science* and *science studies*. What status does the practice of economics have in these approaches? Is it of concern? If yes, how? If not, why?

The tradition of philosophy of science, as commonly referred to, goes back to the positivist program of the Vienna circle. Its initial hope was that referential truth could be reduced to a matter of logic – *whatever* is the pathos and ethos of truth. The tradition continued in postwar figures such as Karl Popper (1902-1994), who softened the Vienna program with his falsificationism, then found a further critique in philosophers like Willard Quine (1908-2000), who undermined the analytic-empirical distinction altogether, and Imre Lakatos (1922-1974) who built on Quine's holism with his idea of 'research programs' as the units that come to be refuted. This continuous thinning out of the referential relation of theories to reality finds its present-day benchmark in scientific realism (e.g. Kitcher 2001, and Psillos 1999). There referential truth boils down to a basic belief of scientists that only halfheartedly maintains the metaphysical verve of the Vienna program.

I do not need to enter deeply into this tradition for the simple reason that there are hardly any direct interactions with the history of economic science in the 20th century. Until roughly 1975, the history of economic methodology can be written without ever mentioning philosophers of science (except, for example, Otto Neurath). Hardly any philosopher showed great concern for economics. And if they did, it was in different terms than commonly discussed by economic methodologists. Popper's remarks on "situational analysis", for example, figured less prominent for those who discussed Popper in the context of economic methodology (Blaug 1980: 231). However, this tradition is nevertheless the reference point for much of what came to be known in the 1990s as the methodology of economics, as represented, for example, in the textbook by Hands (2001, see also Düppe 2009).

The tradition of science studies also has its origin before the war in such scholars as Karl Mannheim (1893-1947) and his sociology of knowledge, or Robert Merton (1910-2003) and his sociology of science. After the war it was popularized and also historicized by Thomas Kuhn (1922-1996) with his notion of paradigms, Ian Hacking with a historicist approach to scientific objectivity, Bruno Latour who begun his career with an account of how "facts" in laboratories are socially determined (1979), developed by Karin Knorr Cetina (1981). Also Steven Shapin's work could be mentioned (1994). These "constructivist" works share the notion that science is a human practice like any other human project: "Just as we have social histories of eating, dying, breeding, and getting and spending, so too we can have a social history of truth making" (Shapin 1994: xxiii). More theoretically than historically oriented is the Edinburgh school of the sociology of scientific knowledge centered on the work of David Bloor and Barry Barnes (Barnes et al. 1996). This school aims at social theories explaining the formation of (both true and false) beliefs of scientists. Most interactions with the tradition of the philosophy of science take place on their arguments. The works in the commentary of economics that could be linked with the tradition of science studies, from Klamer (1984) to Mirowski (2006), however, are carried out largely independently of these theoretical debates.

The role of both the philosophy of science and science studies for the rise of a separate commentary of economics since the 1970s cannot be discussed in general terms. It needs to be spelled out historically (see Düppe 2009, more informed Mirowski 2004: 3ff., 97 ff.). Also, how phenomenology initially was designed as an alternative to the philosophy of science, and inspired parts of the tradition of science studies needs to be discussed separately. For the sake of brevity, let me leave these historical questions aside and consider here a rather stylized relationship between these two established approaches and the phenomenology of science.

Roughly stated, the philosophy of science represents the official image of science. Praising big titles such as reality, truth, fact, etc., philosophers of science conceptualize issues of scientific justification, explanation, evidence, causality, and also more concrete issues of modeling and testing, such as measurement, probability, stability, robustness, etc. In science studies, instead, one exposes, defaces, and debunks precisely that image as a proper representation of what scientists actually do in their everyday lives. Scientists, as pictured in science studies, have much more mundane interests than to pluck entities from the epistemic sky. They have interests in the institutional power of their universities and associations, in the monetary benefits of their grants, in keeping up discourse barriers, in gaining majorities by discriminating against minorities – just as the rest of mankind does. Decisive factors for achieving these goals, science studies tend to show, are not the hard skills philosophers suggest, but rather soft skills as they count everywhere else in society. While in the philosophy of science scientific authority is *justified* by means of epistemic principles, in science studies the same authority is *explained* by the peculiarities of the scientists' social world.

There is undeniably a conflict between the philosophy of science and science studies. Since the big titles of the philosophy of science appear as mere strategies in a play defined in socio-historical terms, science studies were branded relativist and constructivist. If truth-claims are means to pursue special interests or the effects of a social structure, they do not deserve their name, as many philosophers reacted to the strong program of the Edinburgh school, and battled more recently in the Science Wars. Truth needs to be indifferent to the means by which it is claimed; otherwise it is the truth of the means, nothing more. To claim referential truth describes, as it were, the natural attitude of modern science and it excludes the perception of its social conditions. The standard reaction of philosophers to the social conditioning of science is a gesture symptomatic of their way of thinking: separating the truth from the means by which it is claimed, or, in terms of Popper's famous distinction that has been endlessly repeated and contested: keeping apart the "context of discovery" and the "context of justification" – that is, in its result, keeping apart the reality "of" and *of* science.

So how does The Phenomenology of Economics relate to these two approaches? My approach is philosophical in the sense that it aims at economic science as such instead of economic science at a particular moment in time, in a particular school of thought, in a particular cultural milieu, etc. Phenomenology, however, does not presuppose a particular philosophy of science in that it would impose standards of truth, reality, evidence, etc., and judge whether economics meets these standards – as, for example, in the case of those who complain that 'economic agents are human beings, therefore not completely predictable, therefore economics is not a real science'. The Phenomenology of Economics follows science studies in that it shares the basic intuition of economics not being a representation of something beyond itself, but a practice within a concrete moment of time, a concrete school of thought, a concrete cultural milieu, etc. However, it does not suppose that this has nothing to do with the object of concern of economists – as, for example, in the case of those who complain that 'economists are human beings, therefore merely self-interested, and thus not interested in real science'.

I thus do not understand the claim to scientific authority as a vested interest in something else. I suppose that scientists do aim at epistemic claims of referential truth. It is their intention and interest to do so. A "social" notion of science that neglects this intention is not a notion of science, but a critique of it. The crucial question that combines the two approaches phenomenologically is, rather, *whether a scientific claim is possible given its concrete socio-historical reality*. I am thus neither concerned with economic theory *referring* to the world (as in: 'what is the ontological status of the invisible hand?'); nor am I concerned with the possible properties of this reference, that is, what is discussed in theories of models. Theories, phenomenologically speaking, are re-presentations of the world not in that they depict it, but by means of being *presentiations*, that is, expressions of the lived world that gives rise to an epistemic problem in the first place. Theories "tell" from the world in which they are made.

Pivotal contention of my approach to economic science is thus that its "product" – economic theory as the object of the philosophy of science – reflects and is reflected upon the practices of the economist as the object of science studies. Such follows immediately from the notion of intentionality mentioned above. In phenomenology as a post-Kantian philosophy, what describes the practice of science and what justifies its claims are two sides of the same coin. The separation of theory and theoretical practices is inconceivable. A claim does not point or refer to something beyond itself, but what is claimed is the "intentional correlate" of the claim itself. What is claimed and the claiming of it cannot be separate, just as a feeling cannot be separate from the feeling of it. The reality "of" and *of* science cannot be assessed in different terms.

Only if I think of theoretical re-presentation in such expressive terms beyond the separation that is so critical for the present commentary of science can I conceive of a genuine *theoretical interest* and *practice* without immediately bumping against an oxymoron. In order to save science as a subjective accomplishment, I need to avoid at any cost either reducing scientific practices to their resulting theories, or reducing them to their preceding interests. The meaning of economic theory is the *correlate* of an interest of the economist into something. I cannot speak of this interest in terms of theories representing the world, but in terms of concrete practices of theorizing. And I cannot speak of scientific practices as occurrences in the world, but in terms of that which accomplishes theories. In other words, the scientific interest in both received approaches is an empty spot. In the philosophy of science it is abstracted from in order to gain an object of justification (theory), and in science studies it is objectified in order to gain something to be explained. The theoretical interest itself is never addressed!

Phenomenologically speaking, it is impossible to think of a division of labor between the philosophy of science and science studies. "To speak of a scientific life, of a life of the scientist, is thus no metaphor for, or an allusion to something that were merely an empirical concomitant of science – a sort of supplement to its actual being" (Henry 1994: 195*). Instead, the intellectual life of the scientist is the transcendental condition of a theory. The object of science cannot be separated from the sense-accomplishing life of the scientist.

The risk of separating them is high. Focusing solely on the referential value and epistemic principles of economic theory, one considers its legitimacy independent of its actual practice. Then it is possible to legitimize economic theory even though there is no scientist who actually believes in science – as though the practice of and the belief in science could be subjected to division of labor. Matters of ethos would be undermined at their very root. Regarding the critical interest of philosophers of science, separating issues of justification supplements rather than challenges the philosophical naiveté of scientists since the *need* for justification is nowhere addressed. In this sense the philosophy of science seems to me hardly able to be critical of the culture of science (Düppe 2009). Does the philosophy of science reinforce Husserl's *Crisis* – not knowing the task of science, and nevertheless having success in claiming authority with it? As soon as the philosophy of science ignores scientific practices, does it not too easily function as the ideology of science, so that scientists can happily continue pursuing special interests under the guise of scientific authority?

Similarly, the risk of science studies is to not address the scientist. As long as one tends to take scientific practices as an empirical fact to be explained, science studies often has a defacing, if not alienating effect on the scientist. If all images scientists hold about their activity degenerate to a false consciousness that is reinforced by the philosophers of science, how could the scientist feel addressed? Rather, the scientist would feel reified and naturalized when considered as an "object of discovery". Scientific practices are only considered in their social effects, and regarding the anonymous social *structure* they are subjected to. For this reason science studies have contributed to the decline of the writing of intellectual biographies (Forget 2002: 232). In their concern for social structures, science studies is blind to the accomplishing subjectivity of scientists, as is the philosophy of science. This applies equally to the structural aprioris of historicists such as Kuhn (paradigm), Hacking (style of reasoning), and Foucault (discursive formation). These anonymous structural notions are operational for stabilizing a closed discourse of commentary of science, but do not address the scientist (see for a phenomenologically sensitive critique of Foucault's structuralism Derrida 1980: 31ff). Science studies thus tend to reproduce the difference between the contexts of justification and discovery as the difference between the ideology/structure of science and its empirical reality. In both commentaries the same dichotomy is operational: the reality "of" and *of* science. I thus need to free the intuition of science studies entirely from its tendency to degrade into a science of science that determines the allocation of beliefs.

What I find lacking in both received approaches in the commentary of science is, therefore, that they fail to *address* the scientist in his or her concern for science. A clear sign of this failure is that both tend to be closed disciplines, and sub-fields in the philosophy, history, or sociology departments. To be sure, to address this concern is not to tell scientists with *what* they should be concerned. To address this concern is to show the necessity of the accomplishing life of the scientist that cannot be abstracted from or naturalized without losing its inherent urge that it exerts on scientists themselves – the urge to be the person who stands for scientific authority.

For the sake of mitigation, let me add credits to some phenomenological sensibility in present-day commentary of science. I recognize it in some recent works in the history of science where one tries to write a material history of science, which bridges the philosophy/history divide. The classic edition is that of Lorraine Daston, *Biographies of Scientific Objects*. If her aim was to write a "history that would pose transcendental questions in a highly particularist mode" (2000: ix), it matches with the transcendental materialism that I associate with phenomenology. Another warmly recommended example is the philosophical history of Jonathan Rée, *I See a Voice* (1999). In this history of deafness, Rée meets neatly the tone of the transcendental notion of the life-world.

> With luck then, a philosophical history will allow us to catch hold of the idea of scientific objectivity before it has broken away from subjective experience, and observe it in a pristine state, at the moment when abstraction enters our lives, and sense begins to separate itself from sound (Rée, in Hacking 2004:6).

Characteristically, Hacking was quoting these lines in order to object to the phenomenological idea that "objectivity" can be claimed only as long as it is continuously renewed (2004: 6). He instead believes that "objectivity" is stabilized only by means of an historical a priori. Hacking, to grant him credit too, has nevertheless an equal right to claim precedents in the continental tradition. However, what he and others I mentioned apply is not my and Rée's notion of the life-world. It is one I like to call "hermeneutic", to which I turn now.

Let me give at least two examples of an implicit phenomenological sensibility in the commentary of economics. The phenomenological motif strongly informs the tone of Colander's work when he says to be "descriptive of how economists feel about their profession" (2003: 157). Also the imperative he associates with science studies ("understanding economists is necessary to understanding economics" (Colander 1989: 145), has phenomenological potential in that it ignores the division between science studies and philosophy of science. Furthermore feminist economists have partially taken over the move of intimidating economics and economists when pleading for the recognition of "social embeddedness and material embodiment, values, emotions (…), language, lived experience" (Nelson 2001: 93).

Determining the Significance of Economics by Delineating its Special World: The Hermeneutic Notion of the Life-World

For now, these preliminarily remarks about the relationship of phenomenology of science with other commentaries of science will suffice. It will remain an implicit and now and then explicit issue for the following text. Let me come back to what it means to put scientific practices in the center of a criticism of science. Forgotten or naturalized, how can we do justice to scientific practices and address the economist as the accomplishing subject of science? What describes the phenomenological concreteness of this practice?

Although I do share the basic intuition with science studies that science is a particular human practice in principle not different from any other human practices, I do not *naturalize* these practices. They are not like an incidence in the world which can be described like a fact. Scientific practices are the object of concern, not the object of dissection. When Husserl spoke a century ago about "purposeful action" of science – as many others have – he was far away from naturalizing scientific practices.

> First let us recall that what we call science is, within the constantly valid world, as life-world, a particular type of purposeful activities and purposeful accomplishments like all human vocations (*Berufe*) in the usual sense of the word; to this sphere also belong those practical intentions of a higher level which do not involve types of vocation or goal oriented interrelations and accomplishments, the more or less isolated, incidental, more or less fleeting interests. All these are, from the human point of view, peculiarities of human life and of human habitualities, and they all lie within the universal framework of the life-world into which all accomplishments flow and to which all human beings and all accomplishing activities and capacities always belong (Hua. VI: 141, E.: 138).

In these lines Husserl does not speak of the life-world as a transcendental condition, but as that which indeed describes the locus within which scientific practices in all their contingent 'fleeting interests' take place. The life-world exerts a hold on science by means of embedding its practices into this world. Just as other practices, projects, and their realizations *belong* to the life-world, so it is with science, too, as a human project and praxis. Science, as it were, *has its own* "life-world". With this use of the term, Husserl did provide a framework to describe scientific practices. But for this use of the "life-world" Husserl reserved different terms, namely "special worlds" (*Sonderwelten*), or also "environment" and "surrounding world" (*Umwelt*):

> [S]cience is a human spiritual accomplishment which presupposes as its point of departure, both historically and for each new student, the intuitive surrounding world of life, pregiven as existing for all in common. Furthermore, it is an accomplishment which, in being practiced and carried forward, continues to presuppose this surrounding world as it is given in its particularity to the scientist (Hua VI: 123, E.: 121).

With the notion of the "surrounding world", Husserl addressed what commonly came to be understood as life-world: the cultural specificity of human practices. Cultural worlds, at least since the 18th century, are manifold, or, as Husserl had said, "subjective-relative". This plurality Husserl often illustrated along different professions, to which the profession of the scientist and the philosopher also belong (Hua XXIX: 362 ff.). Husserl speaks only of these special worlds as displaying a relative *closure*. Only here can one speak of life-worlds in plural.

> Each of these 'worlds' [of special interests] has its particular universality determined by the end of the vocation (*Berufszweck*); each has the infinite horizon of a certain 'totality' (*Allheit*). But all these totalities take their place within the world, which encompasses all that exists and all existing totalities, as well as their ends and all purposeful men and civilization (Hua. VI: 460, E.: 380).

Husserl discussed the most encompassing special world that provides the closure of "a certain totality" as the "world-for-everyone" (see e.g. Hua XXIX: 86). Since also this world needs to be continuously renewed by the negotiations between the special worlds, Husserl never conceived of an absolute closure of the life-world as one all-encompassing special world. For Husserl, there is neither an anthropological totality of the life world, nor an absolute separation of life-worlds. Instead, beneath each silence of incommensurability, to use Kuhn's term for closure, there is always a swishing of continuity.

In *The Crisis* Husserl also called such special worlds the *regions* of the life-world, the order of which can found a "regional ontology of the life-world" (§51). Apart from professions, this order can be constituted by various kinds of purposes, kinds of people, aspects of life, styles of discourses, etc. In all cases, the order can be described by means of particular *types* or *styles* of meaning – the so-called "modes of givenness" (*Gegebenheitsweise*). These modes predetermine all that could possibly be meaningful in them, and in this way represent the relative closure of a special world. Only then can the special world amount to an epistemic apriori in the sense hermeneutists cultivated it. In human practices, so goes one of the hermeneutists' tenets, things are not given as such but in a particular mode: they are given *as* something. It is precisely this "as" that constitutes the hermeneutic order of meaning: the so-called *nexus* of meaning (*Bedeutungszusammenhang*). Characteristic for the hermeneutic notion of the life-world is a basic *structure* described in terms of regions that are constituted by different types of meaning.

What would be the result of such a regional ontology regarding economic science? I could *determine* the special world of economists within the world they share with the rest of human beings. I could determine and delineate the type of meaning associated with the academic profession of economists. I could describe a special world in terms of typical motivations (social engineering, praising economic freedom), styles of expression (literate, axiomatic), kinds of writings (pamphlets, treatises, articles), types of social relations (schools, fields), kinds of attitudes (calculating brutally, aloof of moral inhibitions), and types of ends (public esteem, academic success) – that is, all what describes the kind of posture the economist adopts when speaking to colleagues and to others who perceive him or her *as* an economist. Economists' practices (like teaching students, publishing their research, advising governments, reporting to the media, and debating with other scientists, etc.) occupy, then, a particular *region* of the life-world, somewhere in and around Western economics departments. I could describe the economist as seen from the point of view of *you-and-me-and-our-fellows*. To say that economists have their own life-world here comes down to the fact that economists have their own *culture*. The Phenomenology of Economics came close to an anthropology of economics.

Husserl expected from such a description also a foundation of the sciences on the idea of the regions of the life-world. The regions consisted of layers of constitution. The reference back to the life-world happens as a "founding of validity" (*Geltungsfundierung*) of regions in a many-layered scheme (Hua VI: 143, E.: 140). What is a constituted type on one layer is a constitutive type on another; what functions "anonymously" on one layer is "patent" on another. These layers are genetically ordered and reach from passive, pre-egological acts like affects and associations to practical and intellectual life – that is, for Husserl, ultimately, reason.

If we think for example about the *economic* region of the life-world, the triviality of this "regional ontology" becomes clear. We could first think of its typical affective type (such as 'the soberness of evaluations' or 'the rage of rivalry'), the type of practical goals (such as 'life sustainment' or 'profit maximization'), typical cultural objects (such as 'goods' and 'money'), typical mode of questions that arise (such as 'What if the harvest will be too little?' or 'What to do with this profit?'), typical *doxic* positions (such as 'The rich become richer, the poor poorer' or 'Free-trade makes us good beings'), epistemic interests (such as 'How was it possible that we got into that misery?' or 'How can we take the next step on the ladder of growth?') as well as the types of theories and kinds of abstractions made valid for this specific economic region (such as 'accumulation of capital' or the 'homo oeconomicus').

To carry out the "huge piece of method" between science and the rest of life is here simply a matter of dividing up and spelling out the regions of the life-world. Of such a continuum, Husserl thought that it could provide a normative epistemology (see Hua VI.: 143, E.:140). The result would be to determine the significance of economic science in dependence on its position within the structure of the life-world. We could determine what kind of interests one has to share in order to conceive of the importance of economic scientists. Phenomenology would result in the elaboration of statements like this: from the point of view of the cobbler's interest, economics seems incomprehensible, but from the point of view of the politician's interest, important.

The Phenomenology of Economics would thus be *trivial*. The notion of the life-world here boils down to the point that science is nothing but a particular way of "viewing the world". The issue of the significance of economics is here simply a matter of naming it. Such could be called the task of *static phenomenology*, which employs the hermeneutic notion of the life-world. It relies on the life-world as a given *order*. Such a notion would be sufficient if there indeed was nothing problematic about economic science – that is, if economics were a profession as unproblematic as that of the cobbler.

A down-to-earth version of this epistemology has been popularized by the Husserl scholar who most advanced the notion of the "structures of the life-world", Alfred Schütz. He even formulated phenomenological postulates for research, such as the following "postulate of adequacy":

> Each term in a scientific model of human action must be constructed in such a way that a human act performed within the life-world by an individual actor in the way indicated by the typical construct would be understandable for the actor himself as well as for his fellowmen in terms of common-sense interpretation of everyday life. Compliance with this postulate warrants the consistency of the constructs of the social scientists with the constructs of common-sense experience of the social reality (Schütz 1953: 34).

Note that such "common-sense" epistemology is possible only as long as the life-world is already of an epistemic kind, and grants a stock of "primordial knowledge" that is in principle not different from any other knowledge of science. The life-world grants us a doxic stock from which all episteme is nourished. For this reason, Alfred Schütz emphasized in Husserl the character of the *obviousness* of the life-world (1980: ch. 1, 1959).

This hermeneutic notion of the life-world is the closest to the philosophy that informs science studies (for an thorough discussion of the influence of hermeneutics on the sociology of knowledge see Hekman 1986). One finds a similar normative use of the notion of the life-world, for example, in Habermas, when speaking of the life-world in the context of discursive communities. The basic intuition is that science "has" a life-world (cultural specificity) within a pre-given structure of a common world (cultural embeddedness). This intuition is present in various concepts such as situations, milieus, discourses, contexts, paradigms, etc. – notions that have achieved some popularity with the work of English speaking hermeneutists of the 1980s such as George Wright, Clifford Geertz, and Charles Taylor.

Many economists between Marshall and Keynes and beyond would have signed Schütz' normative principle without hesitation. They thought of economics as elaborated common sense. Later on, the hermeneutic notion of the life-world had a short prime in the commentary of economics, in particular in Austrian economics – all the more after they came to see themselves outside the mainstream (Lachman 1991, Lavoie 1991). Inspired by Habermas, Don Lavoie speaks of the life-world as a "linguistically constituted world" (1994: 58). Lavoie translated Schütz's normative epistemology into the main Austrian tenet of purposeful action: "for any economic explanation to be acceptable it must relate the observed phenomena to their underlying meaning in terms of the individual purposes" (1986: 196). The Austrian economist Fritz Machlup did the same some decades before Lavoie. He quoted Schütz's postulate of adequacy in the context of the quarrel he had with Lester on the meaning of marginalism (1969: 292). He did so, however, in order to *defend* the abstractions of marginalism against the objection that it does not describe what people do. Machlup thus used the relative closure of types of meaning as an excuse for what gave rise to the greatest mathematical sophistication.

Is this all there is to the life-world that it provides a set of basic beliefs as a reference point by which all abstractions in science are bestowed with meaning? Sure, Cobb-Douglas functions too *could be* spelled out in terms of common sense with some imaginary acrobatics. Economic instructors have to do so every day. But what if there is nothing inherent in a Cobb-Douglas function that requires being spelled out in commonsensical terms? Can the life-world really be critical for *whether or not* a science is founded in it? Can it serve as a normative epistemology for the significance of science?

The Hermeneutic Analysis of Economists' Ethos – a First Heuristic Step Introducing the Problem of Economics

The hermeneutic notion of the life-world will function as a first *heuristic* step in The Phenomenology of Economics, which will do no more than introducing the *problem* of economic science. In the first part (*Discourse*), I am going to introduce economic science informally by means of such an attempt to simply describe the significance of economics by delineating its place within the life-world. How does economic science appear to you-and-me-and-our-fellows?

Which terms, then, are suitable to describe the special world of economists? Which layer of the life-world can economists settle as their region? In the phenomenological tradition, one spontaneously associates *sensual* experiences of the scientist as the terms of critique. Scientists have experiences in the lab, in the library, in conferences, etc., and need to attune their bodies again and again in a posture that allows for the assertion of objectivity. The reproduction of objectivity certainly needs time and effort, from morning to evening, and haunts some even on their weekends. However, such sensual experiences are phenomenologically virulent only insofar they are excluded from science. This is only the case in the natural sciences, which have always served as the descriptive folio for the phenomenological critique of science. Only in the natural sciences is talk about nature constituted by a permanent reduction of one's own continuing sensual experience of nature.

What is excluded and nevertheless necessary for a claim in economic science is not of a sensual kind, but of a discursive kind. Being enmeshed in the world one attempts to apprehend, in economics, means to participate in one economic discourse among others. The experiences from which a possible interest can arise in economics are of a discursive as well as historical nature. Intellectual sensibility in economics is a matter of *discursive* responsivity, as I take the hermeneutic lesson of the works between McCloskey (1985) and Klamer (2007) (see Düppe 2008b). Scientific authority is claimed *by* particular persons *to* particular persons. Economic science is one economic discourse next to others – such as the political, the public, the popular, the moral, the bureaucratic, the technocratic, etc. Economists participate in that economic discourse which gains ethos by means of stressing *authority in the name of science*. With this specification, I describe in the first part the discourse of economists.

Audience, as hermeneutists and rhetoricians know, is not audience. There are different degrees of discursive *vicinity* between economists and economic talk. Economists can speak to everybody, and only for their own shrine. Accordingly, I will consider the economist first in relation to the indirect and occasional audience who never get in touch with economists themselves: *laymen*. What is the *public ethos* of economists? How does the economist appear in the media? What is their most general public identity? What kind of interest lets us listen to the economist? (1) Then, according to a very old economic idea that Husserl shared when speaking of the divisions of special worlds, I ask for the *professional ethos* of economists. What are economists as a particular profession and what kind of institutions do they inhabit? What do economists contribute to society? How do they make their living? (2) Last, I turn to the most intimate audience, to those who guarantee the generation of a tradition of economics: *the students*. How do economists teach? What happens to students when entering the academic world? What kinds of interests lead students to economics, what kinds of interests keep them going, and, at the end, graduating and finding a job? (3)

The lesson of this heuristic exercise into determining the significance of economics will be that the economists' world is *elusive* of such a hermeneutic notion of the life-world. Economics is, in other words, elusive of its own culture. Why this is so should be obvious to most skeptical commentators of the last decades, though not to you-and-me-and-our-fellows. In spite of the expectations that the general public holds of economists' services in the political realm, it became common sense among the official commentary since the 1970s that economists are evasive of any sensible notion of relevance. The determination of the relevance of economics is not as trivial as that of the cobbler. I cannot simply say 'Given this or that interest, economists provide us with this or that knowledge'. The first part will result in the affirmation of these complaints. The problem is that economists do not seem to occupy a particular seat in life. Not one particular meaning, but the indifference to the very concern of meaning seems to guarantee the economists' discursive identity.

This problem, I will show, circles around "formalism" and the "invisible hand". My description in the first part allows me to introduce both as titles of the hermeneutic poorness of economists' discursive identity rather than as theoretical features. The invisible hand undermines the hermeneutic integrity of the economic profession exactly to the extent that it demands a formal attitude to economic life with which the rest of economic talk keeps itself busy. Formalism undermines the expressiveness of economic theory and thus the possibility of taking a distinct posture toward an audience.

Summing up these preliminary remarks on the hermeneutic notion of the life-world – which I take to be the commonsensical notion of the life-world – let us remember that it will only serve as a heuristic benchmark in order to enter the genetic problem of the relationship between economists' intellectual life and the rest of life. When Husserl spoke of science as a concrete historical practice, he did not appeal to a commonality that unifies all human practices, like a basic discourse through which all accomplishments can be articulated (comparable to a meaning "pole" as Husserl called the ego in his earlier work). The life-world is not the world through which we have at our disposal a perspective that makes all practices intelligible. The life-world does not provide us with a primordial discourse of spurious experience where we all understand each other even without words. It is not the safe ground on which all human matter makes sense. Phenomenology does not have the privilege of assigning suitable places to each practice of life.

The problem of "the seat of economic science in life" is not simply to find out its row and number, where it can sit comfortably next to others with its peculiar perspective on the same hermeneutic play of meaning. The seat of economic science in life is inherently unstable. It cannot stand on its own without being pulled this way and that by the demands of relevance and the demands of those who know that it could not stand at all if it were too relevant. The ambivalence of the discursive ethos of economists makes us forefeel a phenomenologically *older* underground than hermeneutic grounds can be. For the life-world is not the original world, but the *originating* world.

(3) The Historiography of the Oblivion of the Life-World

If the economist's ethos cannot be determined trivially, The Phenomenology of Economics must become critical. The critical question is not what *is* the particular significance of economic science, but *has it ever been* (Part 2) and, moreover, *could it possibly be* significant (Part 3). These questions are critical *for* rather than of economic science in that I do not judge economics by the standards of a given order, but consider its *own* genesis – the possibilities it seizes upon.

With these questions, I thus tackle the critical second concern of Husserl, that modern science *forgot* the life-world. I read this notion as a *historiographical guide* through the history of the scientification of modern economics. As such a guide, the notion of the life-world does not denote an order, but is a genetic title of the provenance of science. Which history triggered some to believe in economics and others to refuse it? What have been the motives that gave rise to the scientification of economic writings? And what happened to these motives in the course of their passing over to succeeding generations of economists? How did economics acquire its history?

Husserl's Historicist Epistemology and the Oblivion of the Life-World as Self-Oblivion: The Phenomenological Traces of the Scientification of Economics

Since the life-world is the title of the past that gives rise to an intellectual life, the oblivion of the life-world directs us to the conflict between *history* and *science*, or better, between the claim to scientific authority and the historical world from and in which this claim takes place. In modern science, history as well as the history of science is not supposed to contribute to its body. History is like a residue of science. "A science that hesitates to forget its founders", as Kuhn quoted the mathematician Alfred N. Whitehead, "is lost" (1970 [1962]: 138). Kuhn considered this weak point of science central for his image of science. "The depreciation of historical fact is deeply, and probably functionally, ingrained in the ideology of the scientific profession, the same profession that places the highest of all values upon factual details of other sorts", he wrote in *Structures* (Ibid.). Later on his judgment reads as more severe:

> The sciences are unique among the creative disciplines in the extent to which they cut themselves off from their past, substituting for it a systematic reconstruction. Few scientists read past scientific work;

> science libraries ordinarily displace the books and journals in which such work is recorded; scientific life knows no institutional equivalent for the art museum (…). When reconceptualization occurs in a scientific field, displayed concepts rapidly vanish from professional view. Such reconstruction is a precondition for the cumulative image of scientific development familiar from science textbooks (Kuhn 2003: 87)

To cut off its own past is constitutive for modern science, constitutive of its perception of epistemic life. As soon as something is known it is immaterial what led to it. More than that, to get rid of the ballast of its past is the very appeal of modern knowledge.

If science forgets its past, it does not merely forget something particular, such as a possible source of errors. Rather, it forgets itself as a concrete practice in a concrete historical situation. "A historical reflection", for Husserl was nothing but a "self-reflection aimed at a self-understanding in terms of what we are truly seeking as the historical beings we are" (Hua VI: 73, E.: 72). For Husserl, knowledge can only be meaningful by virtue of a historical reflection, since only then is it informed by its epistemic task. As long as the past of science is the condition of having a task, to forget the life-world amounts to the same as being incapable of asking the basic questions of history: What was this project supposed to be? How did it become interesting? How did we get there? What brought us to it? And how come it ended up like that?

In this sense, Husserl called the "greatest historical phenomenon", "humanity struggling to understand itself" (*das um sein Selbstverständnis ringende Menschentum*) (Hua. VI: 12, E: 14). Such was the point that drove the intellectual interest of second-generation phenomenologists such as Martin Heidegger, Eugen Fink and Ludwig Landgrebe. In recent decades, historical criticism of science found a philosophical revival in the works between Thomas Kuhn (1962) and Lorraine Daston (2000). I consider the phenomenological notion of historical reflection as an informative corrective for these writings. In this chapter, I present a preliminary image of what can be seen as the historicist epistemology of Husserl.

Since, for Husserl, historical reflection and self-reflection are one and the same, "to remember" one's past – in the broadest sense possible of "retaining-still-in-grasp" (Husserl 1975: 106) – is not one mode of cognition next to others. It is the only possible mode. All intellectual accomplishments (theories, models, concepts, hypotheses, etc.), are intelligible only regarding the concrete history of sense that has led to them. In this respect, one could think of Husserl's epistemology as a (transcendental) historicist epistemology. It is most present in his supplement to *The Crisis* on "the origin of geometry".

> The ruling dogma of the separation in principle between epistemological elucidation, and historical, even humanistic-psychological explanation, between epistemological and genetic origin, is fundamentally mistaken (…). Or rather, what is fundamentally mistaken is the limitation through which precisely the deepest and most genuine problems of history are concealed (...) Every explication and every transition from making explicit to making self-evident (even perhaps in cases where one stops much too soon) is nothing other than historical disclosure (Hua VI: 379, E.: 370).

Intellectual life is the experience of being impressed by, and the continuous renewal of, a past. While in sensual life – to contrast for the sake of clarity – being impressed means to sense the world as though for the first time, and while in practical life we can take on our tasks day by day as though we had never done them before, in intellectual life we must remember. In this sense, Husserl continues speaking pompously of a "universal a priori of history with all its highly abundant component elements" (Ibid.: 380, E.: 371). I read this notion of a "universal apriori" in opposition to hermeneutic-structuralist notions of *a* historical apriori (such as paradigms). History *itself* is the apriori of meaning. Or more technically, history is the manner of transcendental constitution (see for example Lembeck 1987). The meaningfulness of meaning in intellectual life lies in its historicity. "We can also say now that history is from the start nothing other than the vital movement of the coexistence and the interweaving of original formations and sedimentations of meaning" (Hua. VI: 380, E.: 371). In other words, only someone who has gone through something can know.

The separation of history and knowledge, of time and cognition in modern science, leads to two transcendental illusions. Once history and science are separate, history can only be "history of facts" (*Tatsachengeschichte*). The history of facts is that history which has lost its capacity for bearing a problem. Knowledge, in turn, loses its capacity for being a response to a problem. Knowledge degrades to referential truth – the truth for which no one has ever asked. As long as the life-world is the locus where problems come from, to forget is to be incapable of remembering what made problems problematic – that is, why matter matters. The historical consciousness is the discursive sense of the problem. The lack of it, respectively, is the insensibility to the problem. Regarding its impressional intensity, moreover, to forget a problem can be easily associated with a state of having solved it. Since problem-forgetting and problem-solving are two sides of the same act, forgetting can be constitutive of the perception of "progress" in science. The less well science knows its task, the easier it is to present its results as progress. It is in this sense that the separation of history and science is constitutive of the image of progress in modern science.

If there was an epistemological imperative of Husserl, it is this call not to forget,

> keeping always immediately in mind the original bestowal of meaning (*Sinngebung*) upon the method, through which it has the sense of achieving knowledge about the world. Even more, it must be freed of the character of an unquestioned tradition which, from the first invention of the new idea and method, allowed elements of obscurity to flow into its meaning (Hua VI. 46, E.: 47).

This original bestowal of meaning upon method Husserl called *Urstiftung* (primal institution or establishment, Hua VI: 72, E.: 72). The effort of intellectual life, as Merleau-Ponty later commented in his lectures on Husserl, is to "[t]o take up contact with what in us understands the *Urstiftung*." (2002: 32). The institution of science is, as I characterize it, the moment of a rising perception of an opportunity and need for epistemic concerns. It is the rising recognition that something demands patience in being followed in its sense-history.

What happens if one does not "retain-in-grasp" the *Urstiftung* of science and loses track of it? Science, then, is not passed over as the formation and sedimentation of sense, but it comes to "dangerous shifts of meaning" (*Sinnverschiebung*), or to a "covering-over of meaning" (*Sinnüberdeckung*) (Hua VI: 46/47, E.: 47/48). Then the practice of science is not the manifestation, but the *covering* of past sense-achievements. Some readers may object that a covering of sense-achievements still amounts to a formation of sense. In its effect, yes, it does. But the 'covering-over' of meaning cannot count as a subjective accomplishment. Sense-coverings are not harmless sense-modifications. Sense modifications occur only by means of exerting a demand on the subject of science that constitutes itself as a responsive subject in seizing upon this demand. Only then does a subject *accomplish* sense-modifications. But this demand is interrupted by the oblivion of the life world. The sense-covering and shifts happen *to* the scientist, so that intellectual life turns against its own motives. The silent, creeping shifts and coverings of meaning represent the forgetting of the *Urstiftung* of modern science, its past, its task, in short, its provenance from the life-world. The oblivion of the life world is a history not of formations and sedimentations of sense, but a history of a self-infestation of sense.

The oblivion of the life-world, to put it more simply, refers to a history of a rising conflict between epistemic life and the rest of life. It is a history of the degeneration of the epistemic task of science, and thus a degeneration of the sources that could bestow scientific practices with meaning. As a consequence, problems loose the grip they exert on the scientist. This history cannot be called an accomplishment of the scientist. It happens "to" him, but nonetheless "through" himself. The history of sense degrades to a trace, a supplement of what appear to be the "achievements" of science. The oblivion of the life-world as a historiographic guide makes us sensible to the history of the affective traces, existential echoes, and personal side-effects of a 'science that has forgotten its founders'. It is the history of the traces in sensible life that are left by an epistemic life that is cut off from its past.

When writing the history of the oblivion of the life-world in economics, I am thus obliged to organize my narrative of its scientification around the *exclusion of history*. To forget the life-world, as I translate Husserl's notion, means that scientific practices are nothing but the forgetting of what instituted them. The history of modern economic science has to be written as the history of the how it detached from its own sense-achievements, which is its history. To put it more paradoxically, the history of economic science is the history of the separation of that science from its history. The social history of economics, within which scientific authority could be instituted, ends up being the residue of its legitimacy. More concretely, it is the history of the increasing difficulty of making sense of oneself as an economist, of an increasing conflict between the social history of economics and the theoretical practices therein: an increasing gap between the reality "of" science and *of* science.

This notion of the oblivion of the life-world will guide my historical narrative in the second part. What, then, could be the historical material of the history of the oblivion of the life-world? First, considering the history from which the motives of scientification stem, I certainly must include the *social history* of science as its general "environment". It provides the social backdrop before which scientific authority plays out. I consider, furthermore, the *history of economic thought* as the means by which this authority is exerted, and insofar as it is responsive to preceding economic thought and its time. Phenomenologically more challenging is to positively account for the tension that develops between the social history and the history of ideas. How can one write a history of the traces and symptoms of the oblivion of the life-world? Here I need to include the actual theoretical experience of economists themselves, with which the transcendental character of the life-world comes to the fore. I will write on the *becoming and aging* of an economist. I focus on a case in which the affective and experiential tension between the motives and the reality of economic science appeared in a clearest way, namely Gerard Debreu (see more below). To write the oblivion of the life-world, in sum, amounts to the writing of the traces of the scientification of economics.

How a Phenomenological Historiography Helps with the Dilemma between Social History and the History of Ideas

Economic science is a textbook study in the exclusion of its history. History was excluded in all respects – regarding "the economy" as a historical phenomenon, regarding its own tradition, and regarding the intellectual biographies of economists. What other than such profound forgetfulness could have led from the epistemic concern of the 'beneficiary consequences of mercenary motives' to that of the 'topology of existence proofs'? What else than horrendous "shifts" and "coverings" of meaning had to take place in order to arrive at a judgment that 'Debreu proofed Smith', as the Nobel committee said in 1983? What else could lead from the 17th-century British King to the 20th-century Swedish King? Let me give a first taste of the problematic relationship of economics and its history.

Needless to say, the low reputation of the history of economics within the profession has long been a complaint (Boulding 1971). It applies to most western countries (see the testimonies in Weintraub 2002). History of economics is not an essential part of an academic training. While some undergraduates do take a course in history, it disappears from graduate programs altogether (Gayer 2002). Most students graduate without ever having read any of the past economists in their original forms. "In the United States, the subdiscipline [of history of economics], as represented in PhD program, is in near free fall, with little on the horizon to provide a safe landing" (Weintraub 2002: 9). "Have you ever heard of the Cambridge Capital controversy?", Colander asked a graduate student. "Was that a *JEP* article? If it was, I didn't read the article. There was a survey of it earlier in some other journal" (2007: 158).

The intellectual practices of economists are far from historical reflections. Economists do not acquire their knowledge by the appropriation of a tradition. One can become a successful economist without knowledge in the history of economic thought, let alone economic history (which, anyway, is located in another department). We know the Stigler et al.'s who argue in one voice about the use of the past of economics that it is *past* and thus irrelevant: "The economics of 1800, like the weather forecast of 1800, is mostly out of date" (Stigler 1982: 108).

> One need not read in the history of economics – that is, past economics – to master present economics. This will not be news to the present generation of economists. (…) He [the young economist] will assume, just as mathematicians or chemists assume, that all that is useful and valid in earlier work is present – in purer or more elegant form – in the modern theory. The young economist will increasingly share the view of the more advanced formal sciences that the history of the discipline is best left to those under endowed for fully professional work at the modern level (Ibid.: 107).

Despite the official rank as second-rate scholars, historians are not diminishing. The *History of Economics Society*, since it was launched in 1973, flourished into a respectable community. Despite those who called back to the history of science department (Schabas 1992), historians of economics cannot be thought away in the ASSA shrine. Could this count as a sign of resistance against the scientification of economics? To what extent are historians critical for economists? What is their ethos?

Let me go through some typical intellectual attitudes among historians of economics with their respective proponents. In the case Stigler has described, history of economics is the history of economic theory, of thought, of ideas, of doctrines, etc. Here history mainly includes the texts that the present state of theory has made redundant to read, which in economics is roughly everything before the formalist revolution. Economists tend to think of this history as a history of errors that culminates in the present state of economics. Such Whig history, as it is called, is organized around an essence of economic theory that is arrived at by a teleological process. One of the first and path-breaking histories after WWII was of this kind – Schumpeter's *History of Economic Analysis* (1954). Although perhaps the richest history in terms of social and biographical material, including ancient and medieval economics, and including economic history and statistics in "analysis", Schumpeter believed that Walras's GET is that economic theory toward which all theoretical efforts prior to Walras were heading, and the standard by which all future efforts must be appraised. At the very beginning of the history of economic thought as a sub-discipline, it was presented as a mere supplement to science rather than an alternative reflection within science.

Such Whig history represents the history that came to be reproduced in textbooks, where, for example, Hicksean demand functions are inversions of Marshallian demand functions. Remember Thomas Kuhn's warning about textbooks. They create an image of progress by making invisible the revolutions that changed the terms of progress: "A concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of national culture drawn from a tourist brochure or a language text" (1970 [1962]: 1). History in textbooks is apologetic in that it justifies that no economist needs to read it. Here, for example, is *all* that Samuelson has to say about history in his *Foundations*:

> Beginning as it did in the writings of philosophers, theologicians, pamphleteers, special pleaders, and reformers, economics has always been concerned with problems of public policy and welfare. And at least from the time of the physiocrats and Adam Smith there has never been absent from the main body of economic literature the feeling that in some sense perfect competition represented and optimal solution. Of course, over time the exact form of this doctrine has undergone modifications (…) and there is considerable diversity in the attempted proofs (in the amazingly few places where rigorous proof was attempted (1961 [1947]: 203).

After 1000 years of preaching, there followed 200 years of chewing on arguments about the optimality of competition, which could now finally be proven rigorously. From Thomas Aquinas to Adam Smith straight to Gerard Debreu: the invisibility of revolutions.

To a great extent, the writing of the history of economics, even if more than Samuelsonian Whig history, is still oriented toward economic theory. One milestone in the writing of history that set new standards of rigidity (although it did not share Schumpeter's teleology) was Mark Blaug's *Economic Theory in Retrospect* that appeared only eight years after Schumpeter (1996 [1962]). Though the later Blaug does show a sense of Schumpeter's rich *Geistesgeschichte* (Blaug 2001), in his young years he largely assessed past theory in terms of a standard body of economics. "Criticism involves standards of judgment", Blaug opens his book, "and my standards are those of modern economic theory" (1996: 1). Even if there were merely humble numerical examples without curves in Ricardo, one can draw them and show what Ricardo actually meant with the decreasing rate of land rentals. In Blaug, there is hardly any mention of ancient and medieval economic writings, so that historians are trained to consider a standard narrative of the history of economics: the SMMS-narrative (Smith-Mill-Marshal-Samuelson).

The so-called "pre-Adamite" history thus needs to be done by other historians. Recently literary critics such as Mary Poovey have taken over that task (1998).

The great majority of historians today are able to carry out their business only internally, *as economists*. Yet they are not uncritical of its present state. The most common motivation for writing history is a sense of loss of the richness of economics, be it the richness of early neoclassicals such as Marshall, or the infinite richness of Adam Smith – to whom historians of the last two decades still dedicate more research than any other economist (according to the Econlit research of Marcuzzo 2008). These Smith-still-knew-what-wealth-is-historians show exegetically how much needs to be forgotten to subscribe to the progress from "Smith to Debreu". In their "nostalgia for the true humanist beginnings," as Ruccio and Amariglio put it (2003: 109), they do, however, tend to simply reverse the textbook narrative. The standard of present theory as *the* reference point is maintained in such a reversal. Such an attitude can go so far as to claim that one or the other scholar of the past was more advanced even by the standards of today! Such could be called reversed Whig History: applying the standards of today to the past not in order to show progress, but in order to show decay! One finds this attitude, for example, among neo-Ricardians. Some historians of economics want to be the better economists (for a further discussion, see Marcuzzo and Roselli, in Weintraub 2002).

There are also critical historians who are sufficiently historically minded to treat the history of economics as an actual historical event rather than the accumulation of an archive. Margaret Schabas, for example, plead for intellectual independence of historians and for a treatment of past economists "in their own right" (2002: 219). These historians tend to ally or provide material for science studies. The master of the writing of the social history of economics is certainly A.W. Coats (1924-2007). He is best known for his work on the British and American professionalization of the late 19th and early 20th centuries (1993). He also made significant contributions to the early modern history of economics, and also to the social history of economics after 1945 (1996). Though critically minded, Husserl would have most likely put his work on the "history of facts" shelf. I suppose that most economists perceive his work in the same manner as Husserl would, without sensing its critical potential. Just as there is a tendency toward naturalization in science studies, the risk in social history is high not to address economists in their historical ignorance. In order to write social history in such a way that it is critical *for the economist*, one needs to account for how the perception of that very social history is operational for the self-understanding of economists today. Otherwise, one runs the risk of actually reinforcing this perception. Economists could be proud that even such "contingent" things as their social history are worth being studied painstakingly.

This risk is perhaps best avoided in the challenging writings of Philip Mirowski. In painstaking archive work, he digs out the social motivations that shaped modern economics. He has traced the political conditions and consequences of the concrete and often symbolic interactions of epistemic imaginaries in economics and the natural sciences (1991), and computer sciences (2001). Mirowski mobilizes Foucault's imperative of the "archeology of knowledge" in that one digs out the underlying power structures that produce particular truth claims in economics – military power during WW II and the Cold War, and today increasingly market power. When talking about his motivation for spending years in archives, he evoked this Foucaultian notion of archeology (though he skipped it in the 2004 reprint).

> (W)hen I read a particular economist's advocacy of regarding children as consumer goods, or another insists that Third World countries should be dumping grounds for toxic industrial wastes since life is cheap there, or a third proclaims that no sound economist would oppose NAFTA, or a fourth asserts confidently that some price completely reflects all relevant underlying fundamentals in the market, I do not view this as an occasion to dispute the validity of the assumptions of their "models"; rather, for me, it is a clarion call to excavate the archeology of knowledge which allows such classes of statements to pass muster, as a prelude to understanding what moral presuppositions I must evidently hold dear, given that I find them deeply disturbing (1994: 29; see also 2004: 39).

Such frank lines are rare among historians. Yet does the strategy of an archeology of knowledge pay off? What are its effects on the profession? Does it *address* economists in being deceived about their history, or does it cast blame for feigning one thing (truth), but actually doing something else (exerting power)? Is the profession able to appreciate such an archeology as a liberating self-reflection rather than an estrangement of their professional dignity?

Both ways of writing the history of economics – history of thought and social history – face the difficulty of relating to economists. Too easily does one supplement or even reinforce the historical ignorance of economists. On the occasion of assessing "the future of the history of economics", Weintraub has put this dilemma in following terms.

> Suppose historians of economics were to take this advise [to critically assess the Whig history of economists, T.D]. I submit that the history of economics would soon stand in the same relation to economics as creationism does to evolutionary biology. (…) Suppose historians of economics were to (…) stop writing histories based on the 'presuppositions of ahistorical economist'. The audience then would become, I suspect, scholars writing in the history of science and science studies. And as a subdiscipline, the history of economics would likely become even less interesting to economists than it is now, if that is possible (2002: 6-7).

How could one avoid this dilemma? Note that in none of the mentioned approaches do we find a historical reflection on the very status of the history of economics for the practice of economics. Historians hardly engage in historical self-reflection. Such a history would be far more than merely a side-history. It concerns the very possibilities historians can seize upon. What needs to be written is the history of the *historicity* of economics, that is, the manner in which economics was able to acquire a history. History needs to be told as that history which enables economists "to have a task" – the history of the struggle of economists to understand themselves as economists. Such is the *history of sense* of economics that I trace in *Part 2*.

Recall, for example, that the difference between the history of facts and the history of ideas in economics as opposed above was a result of the so-called *Methodenstreit* between historians and theoreticians in the 1880s. Before economic science could separate from its history, economists first had to perceive a split between history and theory. Insofar as this perception was motivated, it itself has a history. Only since the *Methodenstreit* does history appear equal to a smorgasbord of facts, which indeed is the prevailing image of what the empirical soil of "the economy" consists of: a hodgepodge of data. If one asks an economist if he has ever heard something of "reality", he will, with a more or less uneasy feeling, ask himself when he has last look up data. And if one asks about "history", he will bring out a stochastic model of these data. Historians of economics are the children of that attitude, insofar as there is no question whether economic historians need a place at the economics department. They work at the history department. Economic history, if it is dealt with in economics departments, is nothing but *a* field among other fields of applied theory – such as in the case of *cliometrics* of Douglass North and Robert Fogel. The association of history with facts is only possible *after* the hierarchy of theory and history has already been established. A historicist position – that economic theory *is* economic history, and *all* epistemic concerns are historical – never existed in the history of economic thought apart from Marx's historical materialism.

Going back a little further, was not the very epistemic problem of early economic theory a historical question – namely, how economic *growth* happened? Although a perception of economic growth was essential for the very motive of practicing economics, the historical consciousness of economists never grew into a real conception of the history of "growth". To the contrary, just as there is a tendency in capitalism to forget that there was ever a time before (let alone to envision a time after), so it is in economics. If economists ever spoke of a time before capitalism, it was "conjectural history", the time of Robinson and Friday. Given that "the causes of growth" was one of the first questions of economic theory, as I will argue in *Part 2*, the very perception of this question is already based on the neutralization of time, and in particular the cyclical time that dominated economic life before capitalism.

As a last preliminary remark about the historicity of economics, consider how the tradition of economics has been handed over. What happened at the crucial junctures of the history of economic thought to the past of economics? How could economists generate their tradition so that it could culminate in the SMMS-narrative? History proceeds in economics mainly by means of *codifying* the body of economics. The main genre that made history in economics is that of *Principles*. Economists wrote *Principles* after *Principles* – as though it never really got started. Economists did never write *in* economics or *in* political economy, but they wrote *an* Economics or *a* Political Economy. Textbook culture dominated the discipline since the mid-18th century. Instead of arguing with or against the views of one's forerunners, one preferred to *summarize* them in such a way that the problematic context from which they were formed disappears. The disagreement economists had with their predecessors did not have to be argued through. They vanished in their re-codification. Economists did not acquire their tradition by means of contests, but by means of flattening differences. Economists since their earliest beginnings had the inclination to hide persistent conflicts. Contests – that is, what the rest of economic talk is mostly preoccupied with – were always poison for the scientific authority economists claimed. So better not to recall the past.

Hence the history of economic thought, to a great extent, can be told in terms of syntheses: First, Smith's *Wealth of Nations* (1976 [1776]) synthesized early modern liberalism and Scottish moral philosophy. He thereby detached economics from the political discourse of 17th-century England and opened the door to academic scholarship. Second, Mill's *Principles of Political Economy* (1994 [1848]) synthesized Ricardo's abstract liberalism with his utilitarian and soft socialist aspirations. He relieved the student from the burden of reading Ricardo, and in part from the suspicion of being biased towards *Laissez-Faire*. Third, Marshall's *Principles of Economics* (1938 [1890]) synthesized marginalism and classical economics. Since then "everything is in Marshall", and not only were the ideological differences of the marginalist revolutionaries smoothed down, but also the foundational issues regarding the analogy of "the economy" and nature (see e.g. Mirowski 2004: 335 ff). Fourth, and most problematic, Samuelson's *Economics* (1948) provided the ultimate synthesis of the science and (political) art of economics. It made the apparent contradiction of the 'engineering of liberty' rigorously invisible. This synthesis could only be topped by Mas-Colell et al., *Microeconomic Theory* (1996). It synthesized mathematical rigor and scientific authority. At each step, economic science advanced by means of forgetting the past it stemmed from – "dangerous shifts of meanings".

Let me close these preliminary remarks on my historiography with the vertical section of *Part 2*. How did economic science intervene in the social history of economic writings? Economic science, so goes the fundamental first insight, is part of modern history. In social terms of, economic science was given an opportunity to contribute to modern life because it promised the closure of the *modern triad* of *science*, *technology* and *growth*, in particular insofar as it promised to close the link between science and growth. Economists took peculiar alliances with modern policies that attempted to liberate mankind – on the right and on the left. Economists toggled largely between two clarion calls of liberation: In-the-Name-of-Science: *Laissez-faire*, and By-the-Means-of-Science: *Revolution*! The history of economic thought was the history of attempts to claim scientific authority in such tones. It begins with the creepy re-configuration of political life by epistemic concern for "the economy", and ends with the silence of the axiomatization of the conditions under which a GE holds.

If I adopt this broadest possible view on economists' intervention in modernity, economic science can be viewed as the attempt of a *structuralist turn* in economic talk and writings. This turn – which is one of my big claims – was never fully carried out. Economics could never establish a "paradigm". This attempted turn is that from the temporal order of the *oikonomia* to the structural order of "the economy". The exclusion of historical reflection, therefore, lies at the very bottom of the possibility of economics having an object at all (*3.1*). The concrete institution (*Urstiftung*) of an epistemic opportunity in economic talk occurred in the political discourse of late 17th century England. Science provided an opportunity to make an economic claim that was not a priori undermined by the economic suspicion that had dominated the intellectual culture of mercantilists. Nevertheless, scientific authority became associated with a particular position: the free-trade movement against the protection of merchants (*3.2*). After this "first wave" of scientification, scientific authority became politically contested during the century of high modernism in economics (1850-1950). Some began to claim "scientific socialism". The battle of ideologies worked as the main engine to push science beyond its political roots, which was not finalized until the formalist revolution (*3.3*). This, I will argue, was the end of the social history of the scientification of economics, since the contradictions of the scientific ethos of economists can no longer be productive. There is no reason to expect another wave of renewing scientific optimism, since there is a declining need for scientific authority due to its loss of contestability (*3.4*).

The nub of this narrative is that economists' scientific practices are entrapped. Scientification in economics made it impossible to reflect one's motives for doing economics. What at the beginning led to economic theorizing – to avoid economic suspicion – at its end backfired in the form of the complaint of the irrelevance of economics. At its end, there can be a theoretical interest only if it is not directed at anyone. Hence the scientification of economics revealed ever more clearly its inclination toward becoming a formal science compatible with all theoretical interests. Scientification of economics took place by means of formalization. In the following chapter I introduce this problem of "formalism" as the oblivion of the life-world.

(4) Formalism and the Oblivion of the Life-World

According to Husserl, the crisis of modern science has its root in its "objectivism". Objectivism and, closely related, positivism are heavily burdened notions in the philosophy of science. In some circles, they are often equated with "scientific", yet in two different meanings. 'Objective' can be opposed to 'subjective' in that 'subjective' amounts to the same as distorted by various contingencies of perception, interest, history, culture, personality, etc. Objectivity means in this case, negatively, the mere exclusion of the subjective. Such is the more everyday use of the word "objectivity". Objectivity, second, can also positively refer to an ontological vision of a totality of beings that are in principle fully determinable. This use of the word is hardly ever confronted in practical life. It matters mostly in the natural sciences, as discussed by philosophers of science under the title 'scientific realism'.

This difference is vital for the scientification of economics because, as I introduce in this chapter, objectivity in the second, positive sense never played a decisive role. The scientificity of economics was never achieved by means of things-being-out-there. There have never been any "discoveries" of objects that are of the same rank as atoms, hormones, neurotransmitters, or the like. Instead, scientificity was always achieved by means of not being *biased*. The scientification of economics, in other words, did not happen by means of the objectification of economic life, but by means of the formalization of a structure: "the economy". Since the phenomenological critique of science is traditionally oriented toward the natural sciences, most phenomenologists tackle the metaphysics of scientific realism. Instead, I need to make a major stipulation regarding the reason why economic science forgot the life-world: it happened because of formalism, not because of objectivism.

At first glance, the notion of the life-world presents in fact a different image of science than that associated with scientific realism. "The world (…) does not exist as an entity, as an object," which I have made central in Husserl's late phenomenology (Hua. VI: 146, E.: 144). World is horizon, and thus never totality. Husserl indeed was often opposed to the naive belief of science in a world in itself when speaking of a "constructive concept of a world which is true in itself" (Ibid.: 177, E.: 173).

In, say, folk-phenomenology, one commonly equates objectivism of science with reductionism: the reality of subjective experience ossified in the sum of properties of objects. The properties of objects only softly echo the experiences that constitute them, if they do not silence them entirely. Economists, however, have never gained great authority by reducing lived experiential reality to crude entities. To the contrary, they have always shown a sensibility for the intrinsic variety and irreducibility of individual realities of economic life. Economists gained a scientific ethos because they were able to point to the subjective realities of economic life, but with the same gesture also to steer away from it. If they would not point to it, they could not be identified as economists; if they would not divert from it, they could not be identified as scientists. Hence the theoretical perception of "the economy" was never meant to evoke the imaginaries of an object, but of a *structure*.

Historically speaking, the difference between formalism and objectivism accounts for the differences between William Petty and Thomas Mun, William Whewell and Richard Whately, Irving Fisher and Vilfredo Pareto, Oskar Lange and Ludwig von Mises, and ultimately between John von Neumann and Gerard Debreu. This argument is fundamental in order to arrive at the conclusions I have envisaged. It is also vital for understanding the distinctness and necessity of a phenomenology of economics as compared to other commentaries. For a preliminary understanding of the difference between formalism and objectivism, let me here introduce it with Husserl.

Objectivity and the Age of the World – Never Old Enough not to Shimmer with Reddish Shades

What, according to Husserl, is the problem of objectivism? He characterized objectivism as the "belief in being" (*Seinsglaube*) that the "scientific attitude" has in common with the "natural attitude". The phenomenon of the world, which is the object of phenomenological inquiry, renders in science and the rest of life a pre-given belief, a never questioned but continuously functioning *Urdoxa* (for example, Hua III: 257). In this sense of a *doxic* condition of science, Husserl speaks of the life-world as the horizon of *obviousness*. The existence of the world is never questioned but always presupposed for all practices of science. Science never gives an account of how something can appear to the scientist in the first place, nor how it arrived at the "belief in being".

That modern science forgot the life-world, at this point, means that it forgot that the belief in being in itself correlates with a *history of sense*. The belief in a world-in-itself is itself an accomplishment. Science forgets

> that even the apodictically persisting conviction of one and the same world, exhibiting itself subjectively in changing ways, is a conviction motivated purely within subjectivity, a conviction whose sense – the world itself, the actual existing world – never surpasses the subjectivity that brings it about (*zustandebringt*) (Hua VI: 271, E.: 337).

Before there was the belief in a world-in-itself, there was a history that motivated that belief. It just so happened that the world "grew old", so old that the scientist who accomplishes this belief and continues accomplishing it every moment when engaging in science, is inclined to believe that the world has been always there, and, moreover, that it even contains, hidden somewhere, all the answers to the questions this same world unceasingly poses to the scientist. The task of a phenomenology of science would amount here to catching hold of the world in a moment when it evokes its image of an infinite age – to catch hold of a past of science that has been always already forgotten.

Also in Heidegger, to mention another Husserl scholar, objectivism represents *the* disease of modern science, at least in his early work *Being and Time* (1962 [1927]). The distinction that made this book popular echoed precisely this worry about objectivism: the distinction of *Vorhandenheit* (being-present-at-hand) and *Zuhandenheit* (being-ready–at-hand). Heidegger's entire analysis of Dasein could be read as a way to show how the 'conviction of one and the same world' is motivated within our pre-theoretical dealing with the world. Concerning science, Heidegger mentioned (but did not elaborate) an "existential concept of science" as opposed to the "logical concept of science" (§69b). Within the logical concept, science is viewed with regard to its results, that is, "something established on the interconnection of true propositions". When considering the existential concept of science, instead, he asked, similar to Husserl, what were the "existentially necessary [conditions] for the possibility of Dasein's existing in the way of scientific research" (1962 [1927]: 408). Science is a "mode of Being-in-the-world", a "way of existence", as he puts it (see for an exegesis Gethman 1991).

In this existential genesis of science, Heidegger argues, "objectification" in science is preceded by "thematization". In terms of his jargon, "inner-worldly encounters" (*innerweltlich Begegnendes*) have to be there within a nexus of "in-order-to" which implies its own "circumspection" (*Umsicht*) before they can become "obtrusive" (*aufdringlich*), and can then be thematized in their obtruding. Then, by "bringing the object of concern close by interpreting it circumspectively", we can deal with this object of concern in "deliberation" (Ibid.: 410). Only in this way, can things be *singled out* and subjected to an epistemic question of objectivity. In terms of his famous example: The hammer is the hammer we use for something else; only if the hammer becomes "too heavy" can it be singled out independent of the place where it belongs. Only then can the hammer be thematized in its "objective weight" (Ibid.: 413). Husserl's attempt to catch hold of the world at a moment when it evokes an interest to consider it theoretically translates in Heidegger to 'which things have to break to make us objectify the world instead of simply dealing with it'?

As appealing as such tracing of scientific objectivity is, would it not be in the interest of science since it helps "making assumptions explicit"? Could this project not be potentially used in order to "filter out" even more of "the subjective"? Then one could even conceive of a phenomenological science that has as its "object" the horizon of the obviousness of the world, which would bring us back to the "regional ontology of the life-world" as discussed above. Indeed, Husserl's notion of the life-world as a horizon of obviousness is still embedded in his early, Cartesian work. It belongs to the Kantian paradigm of a reflexive, transcendental idealism as a critique of metaphysics (see for a defense Held 1991, for a critique Landgrebe 1961, and for a comparison Luft 2004). The main intellectual virtue of phenomenology would be here "apodicticity" and "presuppositionlessness", as hermeneutists mistakenly criticized Husserl.

The phenomenon of the world, however, does not lie "behind" the being of the world as a reflective presupposition of our attempts to comprehend its totality. The oblivion of the life-world is not the oblivion of a more fundamental level of the world principally still graspable within the horizon of reason. Life-world *is* the horizon of reason rather than a universal horizon as the totality of all partial horizons, which could be made an object of reason. With the life-world, and here I follow what the second generation of phenomenologists such as Ludwig Landgrebe emphasized (1961), Husserl aimed rather to overcome Cartesian and Kantian reflexive philosophy, and thus the project of a foundation of science. The notion of the life-world rather affects what it could possibly mean to reflect on presuppositions. The oblivion of the life-world cannot be avoided by the means of modern science: reflecting on presuppositions.

Apart from this philosophical reason, a genealogy of the scientific "belief in being" suits only a phenomenology of the natural sciences. For "nature" is just that world which is so old that we have forgotten the moment when we turned it into the prime locus of "objects". "Nature" is the object *par excellence*. But if, as I suggested, we think of science as the practice of claiming scientific authority, then science does not require a perception of nature. Thinking of the life-world as a horizon of obviousness is oriented toward the natural sciences, and does not help understanding the problematic relationship of economic science and the life-world. I venture that objectivism (as well as its methodological brother, positivism) cannot serve as a guide for a critique of economic science. The problem of economic science is not that it forgets to reflect on the genesis of its belief that *there is* world, the economic world respectively. Economists, I will show at length, did indeed forget the genesis of "the economy", but not regarding their belief that *there is* "the economy". The scientification of "nature" from Galileo to, say, Hilbert, therefore, cannot be told in the same terms as the scientification of "the economy" from, say, William Petty's *Political Arithmethick* to Gerard Debreu's *Theory of Value*.

Even Heidegger acknowledged with explicit reference to economic science that it is *not* necessary for science to objectify what is ready at hand. Economic science, said Heidegger, is the science that thematizes *Zuhandenes* as such:

> Even that which is ready-to-hand can be made a theme for scientific investigation and determination (…). The context of equipment that is ready-to-hand in an everyday manner, its historical emergence and utilization, and its factical role in Dasein – all these are objects for the science of economics. The ready-to-hand can become the 'Object' of science without having to lose its character as equipment. A modification of our understanding of Being does not seem to be necessarily constitutive for the genesis of the theoretical attitude 'towards Things' (Heidegger 1962: 413).

The scientificity of economics, along these lines, does not depend on the objectivity of its objects. There are economic things only as long as they remain in a pre-objective state of 'equipment'. Most economists knew that.

To be sure, objectivism was *a* mode by which economics became science, but it was not the driving force. To begin with, consider the official criticisms that positivism has earned in the last four or five decades in the philosophy of science (I mean the Popper-Lakatos-Quine tradition). Did it have any effect on how economists thought about their profession? Although philosophers of economics often proudly refer to this post-positivist tradition as *the* achievement of their work, having liberated, as it were, economics from positivism, economics itself was by and large not touched by these debates. In spite of the fall of the official doctrines of positivism, economists, in recent decades, increasingly use positivist methods, such as in econometrics or experimental economics. Economics gained its status as science independent of the received philosophy of science. The philosophical problem is rather that there is nothing in economics that suggests the need to take a position regarding the objectivity of its objects.

This theme of the secondariness of positivism and objectivism for the production of scientific authority in economics will be one of the running issues of my historical narrative. For a first taste, consider how the SMMS narrative of the history of economics had to be modified in order to grant full influence to positivist doctrines. Instead of "Smith", economic science had to start with William Petty (1623-1687). In the 19th century, the standard history had to include more economists, such as William Whewell (1794-1866), and also historicists such as Karl Knies (1821-1898) who liked statistical, descriptive, or other ontological determinations. Ricardo would be a minor character. Comte's sociological positivism could not have passed by so unnoticed as it did. Robbins's definition of economics could not have so easily ended up in the textbooks in spite of the critique of Hutchison. In the early 20th century, the makers of science should have been Hotelling, Coals, Lange, and others. None of these economists, however, contributed to the scientific authority of economics. To know things in detail, and to look painstakingly, to the contrary, always had the touch of indiscreetness. Bacon's eye was much too voyeuristic, in that it could help economists gain scientific authority. The ontological vision that *there is* a suitable object of economic science (scope) that can be *discovered* with particular techniques (methods) never made the economic scientist. At most, it functioned as window dressing for the hidden motivations of scientification. Positivism, now and then, may have shaped the belief in economic science. But the history of economic positivism is a history of a series of inhibitions.

Empirical interpretations of economic laws – be it the falling rate of profit or the diminishing marginal utility – never found a large audience. It was not the sophistication of techniques for discovering things that made economics, but the sophistication of techniques for "reformulating" and "generalizing" preceding theories. Economists tended philosophically toward an apriorism, deductivism, blackboard economics, that is, to *theory* – which has been long acknowledged by commentators. Economics advanced by means of *theory* rather than the discovery of empirical laws. Economic theory and economic science are, to a great extent, replaceable expressions. What did find a bigger audience were innovations such as the use of calculus, of geometric representation, of convexity analysis, and other sobering "reformulations" of preceding theories. If economics "discovered" things, then they were theoretical relations: integrability theorems, continuity axioms, multipliers, Slutsky equations, Arrow-Pratt measures, and the like. Economics is held together by theoretical structures, not by basic beliefs or empirical truths. Of course, economists still *can* believe in the objective world, even if focusing entirely on theoretical structures, as Mäki has taught for decades (2001). However, there is simply no need for doing so.

Economists, moreover, were always sensitive to the argument that basic economic phenomena are "subjective". Economists never wanted to reduce a reality to a principle, never reduced the Social to the Natural, or, for that matter, the Individual to the Social. The dominant strategy to deal with subjective realities was not to make tangible objects out of them, but to establish a level of reflection at which the subjective constitution of economic reality *becomes harmless for the economist*. Economic science did not proceed by means of filtering the subjective from the objective, but by means of allowing and at the same time disregarding the subjective. Even if attracted by the analogy of "the economy" and nature, there was no economist who did not believe that such is nevertheless an analogy. The so-called natural science-envy, which nourished much of the scientific optimism of 19th-century economists, was *not* decisive for gaining scientific authority. The natural sciences were merely instrumental for appearing to be beyond the subjective. No more!

The decisive engine of scientification was not the lack being objective, but the suspicion of being biased. That lack of objectivity and biased science are two different things is most apparent when considering the political connotations of positivism. For those who did stress the objectivity of economics even ran the risk of evoking more suspicion of hiding their subjective biases. Objects may convince Bacon's eye, but they do not convince a skeptical mind that sees nothing but the self-interests that lead to a claim. Those economists who looked closely with penetrating eyes, at least until the formalist revolution, have been those who stressed "scientific socialism". Positivism was reddish colored. In economics, to believe in a totality of being that entails all the answers one poses to it, is to believe in the ontological transparency, determinability, and thus possibility to design "the economy". If "the economy" is objective, a positive fact, then mankind is close to printing out the plan for economic heaven, abolishing all the miseries of capitalism. Many economists hoped so in the first half of the 20th century, as I am going to elaborate in the second part. To put it frankly, I venture to show that *imitating* scientific objectivity always sufficed economists' discursive need for authority, while carrying it out *harmed* it.

Those who wanted to avoid this association of positivism and socialism were those who did *not* look too closely – who remained *discreet*. The distance that the economic scientist adopts could not be that of the onlooker, who looks closely or counts carefully what people need, want, or desire. Fie! It is the distance of a discreet person that never asks too many questions and thus appears aloof from the quarrels others have – this granted scientific authority to economists.

Instead, the Discreetness of Formalism: Suggestive but Silencing – the Genetic Code of the Theoretical Experience in Economics

If the oblivion of the life-world is not due to the objectivism of science as the incontestable belief in the totality of being, what then makes the scientist forget the life-world? The belief in a world-in-itself, as I quoted Husserl above, is *motivated*. What needs to be motivated for science, I added, is not a positive vision of an independent reality, but first of all the *claim to scientific authority* – on whatever grounds that claim is made. Then the actual problem of objectivism is not that it is a particular epistemic presupposition, but that it hides to be a response to the need for epistemic authority. What must be motivated for science to settle, is, therefore, the exclusion of "the subjective" rather than the disclosure of the objective. Let me dwell upon this argument a little further.

Husserl refers repeatedly to the somewhat confusing notion of the *finitude* of science when talking about the oblivion of the life-world. The critical point of the finitude of science is not, as it may be conceived by scientists, that we do not have the cognitive capacities, or simply not enough time to enquire so long into objects that they really stand on their own. Finitude is not a problem of the limited cognitive economy of human knowledge. Instead, the finitude of science means simply that scientific knowledge is limited by its scientific interest. The claim to objectivity, respectively, is "infinite" insofar as it does not reflect the need for this claim. If *A* is *b* it is so whether or not someone asked it to be. At least there is nothing *within* the act of this judgment that could suggest that. Objectivism excludes the possibility that there is always already a non-thematized horizon onto which there can be a claim to objectivity.

As an illustration of the finitude of science, Husserl discussed in his famous §§ 8 and 9 of *The Crisis* one of the initiating rites of modern science – namely, the moment when pre-modern Euclidean mathematics was replaced with Galilean science. What happened when Copernicus, Kepler, and Galileo turned world into nature by mechanizing it? How did they relieve mathematics from its low esteem as part of engineering and grant it the height of the certainty of knowledge? While "Euclidean geometry, and ancient mathematics in general, knows only finite tasks" (Hua VI: 19, E.: 21), so Husserl states,

> the idea of nature as a really self-enclosed world of bodies first emerges with Galileo. (…) [It] soon brings about a complete transformation of the idea of the world in general. (…). The ancients had individual investigations and theories about bodies, but not a closed world of bodies as subject matter of a universal science of nature (Hua VI: 60, E.: 66).

What makes this new idea of 'the world in general' infinite, according to Husserl, was the perception of a unique *a priori* of science, through which *all scientific claims* had to be articulated. Problematic in the notion of an objective world is not its objectivity itself, but that it imposes an apriori. The novelty of modern science, in other words, lies in its monism (of which its psychic-physical dualism is only a modality, as Husserl continues to argue in § 10).

Husserl illustrates this monism of science with the "mathematization of nature". He focuses particularly on the construction of a geometric "ideal space" – that is, a homogenous space in which all points are interchangeable. Problematic about the ideal space is not that it *reduces* manifoldness to one idea, but that it *anticipates* everything that could possibly be a valid spatial phenomenon.

> What "exists" ideally in geometric space is univocally decided, in all its determinations, in advance. (...) What is new, unprecedented, is the conceiving of this idea of a rational infinite totality of being with a rational science systematically mastering it. An infinite world, here a world of idealities, is conceived, not as one whose objects become accessible to our knowledge singly, imperfectly, and as it were accidentally, but as one which is attained by a rational, systematically coherent method (Ibid.: 19, E.: 22).

The novelty of the mathematization of nature lies in the *anticipation* of what possibly can become a matter of reason. In Heidegger we can read similar lines:

> In the mathematical projection of Nature, moreover, what is decisive is not primarily the mathematical as such; what is decisive is that this projection discloses something that is a priori. Thus the paradigmatic character of mathematical natural science does not lie in its exactitude or in the fact that it is binding for 'Everyman'; it consists rather in the fact that the entities which it takes as its theme are discovered in it in the only way in which entities can be discovered – by the prior projection of their state of Being (1962: 414).

The problem of mathematization is not that it is an *abstraction* from qualities – this is *precisely* the virtue of mathematics – but that it *anticipates* all that could possibly count as a valid case to be reasoned.

The risk of this anticipation is, furthermore, not that it constrains what can be reasoned, but that it forgoes reasoning altogether. This risk lies in the *suggestive force* of mathematical experience, which let the scientist forget the reasons of the abstraction of the ideal space, and therefore also forget the preliminary character of the notion of ideal space. Mathematical practices are risky because they *cover* their own limitations. Husserl called this cover the "garb of ideas" (*Ideenkleid*) of formal expressions. It conceals the horizon from which such expressions were made. There is nothing *in* the mathematical experience that recalls what made it necessary. Even if mathematization begins as an abstraction it substitutes such reasoning "on the way". The actual reason why the abstraction of an ideal space, for example, came up in the first place is not *presentiated* in its mathematical form. 'Mathematical science' is in this phenomenological sense self-defeating.

> It is through the garb of ideas that we take for true being what is actually a method (…) It is because of the disguise of ideas that the true meaning of the method, the formulae, the 'theories,' remained unintelligible and in the naïve formation of the method, was never understood. Thus no one was ever made conscious of the radical problem of how this sort of naivety actually became possible and is still possible as a living historical fact; how a method, which is actually directed toward a goal (…) could ever grow up and be able to function usefully (Hua VI.: 52 E.: 51f.)

This "garb of ideas" is the genuine phenomenological locus of criticism of the oblivion of the life-world. For it does not concern a particular, adjustable epistemological position or belief, but it concerns the *experience* of modern science in that it makes, *willy-nilly*, forget. Whatever philosophy, whatever pragmatic objectives inform modern science, these possible sources of meaning bestowal are never confronted while practicing it. Modern science, in other words, is not expressive of its motives. More than that, in particular in economics, it tends to equate this inexpressiveness with its scientificaty, because it feels like being free from subjective distortions.

Philosophy apart, at the bottom of the phenomenological constitution of modern science is an *affective* identification of mathematics and scientificity. Although no philosopher of science has ever fully and explicitly made this identification, modern science is carried by the transcendental dream of a "mathematical science". Although mathematics has never made up the actual work of knowing, it always *felt* like that. Being explicitly asked, scientists may very well restrict their claims, and point to the need of further specifications. These warning, however, are only meaningful before the backdrop of the suggestive force of the experience of mathematics that says that mathematics is All There Is about reason. The countless contests about the *limited* role of mathematics in science show the very strength of this affective identification.

To be certain, nobody ever wanted or defended the oblivion of the life-world – for how, then, could it be an oblivion? Rather, the oblivion of the life-world can be called a *side effect* of the experiential nature of modern science. While being engaged in scientific practices, its horizon does not appear to be *needed*, and thus is easily forgotten. The forgetting has the character of an *unlearning* of a reflection in the course of engaging in the necessities of mathematical reason. The problem of modern science is the misunderstanding it suggests, the misunderstanding that the practice of reason can be separate from its motives. Modern science represents a *rupture* of intellectual life that cuts off the scientist from his or her past.

One may object that with a supplementary philosophical awareness this problem can be avoided. Certainly, but this is not more than question begging, as long as there is no motive to hold on one's philosophical awareness. Even if a scientist believes himself to be a "scientific realist", he or she may nevertheless be subjected to this misunderstanding since this belief is not confronted in scientific practices. This explains the gap between the institutions of philosophy of science and science itself, that is, in McCloskey's terms, the gap between the official und unofficial rhetoric of science (1998). I would even go so far as to claim that there is a philosophical issue about modern science only because of its phenomenological forgetfulness.

Economists were always aware of the inherent risk that "mathematical economics" may replace economics. Even John von Neumann showed this awareness when speaking of the "nature of intellectual efforts in mathematics". "[A]fter much 'abstract' inbreeding," he warned, "a mathematical subject is in danger of degeneration (1961 [1947]: 9). Economists today may still agree. But they may have also forgotten how to do anything other than stabilize formal relations. Recall once more, in this context, Hicks' poignant words when being asked to say something about what he considered relevant: "I'd like to if I could. But I think it is beyond what is left to my capacities" (1989: 180).

The inherent misunderstanding of a mathematical science has clearly nothing to do with objectivism or the philosophical belief in the being of the world. It is rather the neutral appearance of formal methods that suggests this misunderstanding – that is, in Edgeworth's words, the separation of "method and idea" (1889: 541), and in Koopmans's words, the "separation (…) of reasoning and recognition of facts" (1957: viii). The problem is the separation of form and content of science: *formalism*. It is this very separation that makes the scientist insensible to the need for explicating the meaning of formal relations – the need for making science intelligible.

Husserl's discussion of objectivism indeed comes down to the point that the claim to objectivity makes science *unintelligible*. Unintelligibility, in turn, cannot be the result of the belief in a totality of being, but is the result of formalism in that it makes the scientist forget to make understood. The actual problem of objectivism is not the belief in "things in themselves" – as though we had to solve a metaphysical quarrel about being – but that objects *interrupt* the history of sense – that is, they establish a structure of meaning independent of its history. To put it differently, the problem of objects is not their independent existence, but that they allow for *last words* in the discourses they govern. Claims of objectivity are problematic insofar as they function as a "conversation stopper", as McCloskey would say (1998). They represent the *closure* of scientific discourse. The problem of objects in modern science is that their objects are not *telling*. Modern science makes us silent rather than spurring us to think. Here is how Husserl made the link between objectivity and intelligibility:

> The point is not to secure objectivity but to understand it. One must finally achieve the insight that no objective science, no matter how exact, explains or ever can explain anything in a serious sense. To deduce is not to explain. To predict or to recognize objective forms (…) and to predict accordingly – all

> this explains nothing but is in need of explanation. The only true way to explain is to make transcendentally understandable. Everything objective demands to be understood (Hua. VI: 193, E.: 189).

And for precisely this demand, modern science lost its sensibility.

As long as the phenomenological problem of formalism is the rupture it represents in intellectual life, it applies equally to methods that are commonly opposed to mathematical economics, such as econometrics. Problematic in econometrics is not to ask: 'How Much? Is it really as big as we believe or perhaps even bigger?' Problematic is rather that quantitative methods are *too easily formalized*, which means here to standardize the procedure of counting in a, say, "mechanical" way. Problematic is thus not the counting itself but that, as method, it makes us forget to ask to ask: "How Big?" in the sense of: Is it "important"? Econometrics is formalist in precisely the sense McCloskey and Ziliak argued that "statistical significance" does not necessarily match with "economic significance" (2008). The point of the history of statistics in economics is not that it reduced economic life to numbers, but that it contributed to the diminishing need for asking what it means to conduct an economic life. And in this sense statistical methods are the offspring of, not a cure to, mathematical economics.

Apart from mathematical and statistical formalism, there are many other kinds of formalisms that phenomenologically all come down to the point of a loss of expressive abilities. Let me simply list some that are common, and commonly complained of in economics. There is, of course, logical formalism, that even early economists like Nassau W. Senior stressed, along their idea of the deductive method; all derivation – not saying any more than one had said at the beginning. There is computational formalism as used in operations research and complexity theory, putting algorithms on different structures while waiting for a new meaning to emerge. There is the moral formalism of utilitarianism, in which to all motives one cannot say more than "I like it" – as though one were embarrassed to speak about one's motives. There is naturalist formalism, cutting short reasoning by means of an analogy of "the economy" with a pendulum moved by gravitational forces – the analogy has never been spelled out whatsoever. There is the formalism of (national) accounting, saying that all there is about welfare must be somehow expressed in what we buy – as though there were not more to say about history than "1.2%". There is geometric formalism, saying that understanding the difference between monopoly and competition is knowing how to draw the Harberger-triangle. And there are mathematical formalisms of many kinds, at the top of which stands the axiomatic method as the separation of structure and meaning. With the axiomatic method, the latent conflict of all those formalisms can be illustrated in a most effective way.

This latent problem – I repeat because of its importance – is not a problem of a particular mode of expression, or of a particular philosophy that informs this mode of expression (Husserl thought Kant was formal). Formalism is a problem of "lowering one's tone", of becoming indifferent to the weight and demand of meaning. Not sensing the urge to explicate formal structures amounts to a *rupture* of intellectual life. Such an approach to formalism gives an answer to what most critiques of formalism in economics forget to ask: How could mathematical expressions and other forms of formalism possibly be so successful in the first place? There must have been something appealing in being aloof from meaning. Philosophy of science may re-justify or charge the limits of mathematical science. Science studies may give an explanation of its success in social terms. But do they make it understood?

Scientific authority in economics was gained by means of arriving at a level of reflection at which one no longer can impose particular motives or interests – that is, by means of becoming formal. The corresponding ethos of economists was that of the elevated, the discreet, or even absent participant in economic talk. The engine of scientification of economics was, as I will trace along various facets, the suspicion of being ideologically motivated. Only this motive can make understood why the aloofness from meaning was so appealing to economists. There should be no economist left today who has not heard: formal theories are politically irrelevant. Yes, for precisely this reason they were appealing to economists! Considering both aspects of formalism, being *attractive* and *risky* at the same time – attractive for appearing beyond political bias, and risky for ending up politically irrelevant – formalism is the genuine phenomenological locus of the *theoretical experience* of economics.

Given these preliminary considerations, I can anticipate the experiential genealogy of formalism in economics. In *nuce*, this genealogy runs as follows: First, scientific modesty is instituted as an opportunity to gain attention in a world where there is nothing but moral clamor. This modesty easily turns into aloofness and further into discreetness rather than scholarly respect for matters of concern. This discreetness, moreover, applies not only to the heated concerns of others, but increasingly to one's own concerns, which, after all, makes economists forget their own motives for engaging in science. As a result, irony and cynicism about the belief in the worth of one's effort are systematically induced. Such is the genetic code of the oblivion of the life-world.

In *Part 3*, I will flesh out such an experiential genealogy of formalism along the intellectual becoming and aging of perhaps *the* protagonist of the formalist revolution, Gerard Debreu. In Gerard Debreu the scientification of economics found its peak, all scientific optimism culminated, and yet it turned out to be a dead end. I will write the intellectual biography of Debreu as a *transcendental parable* of the oblivion of the life-world. It does not aim at revealing the truth about Gerard Debreu's life, but aims rather to help the economist to reflect on the motives of his or her intellectual life. Debreu's life is an apologue in order to transmit the question of intellectual ethos in an indirect but nevertheless concrete fashion.

This transcendental parable runs from "Bourbaki" to "Smith" – that is, from Debreu's youth at the Ecole Normale in Paris in the last years of WWII, where he was taught by "Bourbaki", to 1983, when he received the Bank of Sweden Prize in economics, roughly for 'having proven the invisible hand of Adam Smith'. When Debreu wanted to leave mathematics in the last years of the war, his fascination with mathematics constituted an existential dilemma. He has not learned how to deal with intellectual concerns. Rather than solving this dilemma, he entered economics without really knowing why – following a chance meeting with Maurice Allais. His further career at the Cowles commission until the mid 1970s was stamped by a rigorously *discreet* attitude. Debreu remained strictly Bourbakian and avoided making any economic claims. When in 1983 his work came to be celebrated with the Nobel Prize, the misunderstanding that kept economics going fell open at the feet of the Swedish King. The affective biography runs thus from Debreu's fascination with Bourbaki's mathematics, to his dilemma in his youth, to his discreetness as an economist, and culminates in the disaster of the Bank of Sweden Prize. Philosophically, this narrative runs from a position according to which mathematics excludes science, as in Bourbaki, to the position that mathematics and science are identified, as is necessary to claim that 'Debreu proofed Smith'.

Debreu's life thus consists of precisely the elements of a parable: a moral dilemma, a questionable decision, and the suffering of the consequences of this decision. Debreu's biography is a transcendental parable of the phenomenological end of the ethos of the economic scientist. Economists may face problems *in* economic science (mainly how to provide a real alternative to Arrow Debreu 1954), but they no longer face problems *of* economic science.

Concluding these remakrs on the oblivion of the life-world insofar as it is due to formalism, my critique aims at the difficulties understanding oneself when putting efforts into economics. The critical moment of The Phenomenology of Economics is the moment when there is silence, when there is *nothing to say* anymore as an economic scientist – a point, at which I will actually arrive with Gerard Debreu, who was celebrated for having nothing to say as an economist. I thus address a sense of inadequacy that accompanies scientific practices, regarding the feelings that keep the rest of economic talk moving. Husserl illustrated this, one may say, inherent estrangement of modern science with the image of the machine – as though he were a child of the industrial revolution: the product of man turning against himself.

> Are science and its method not like a machine, reliable in accomplishing obviously very useful things, a machine everyone can learn to operate correctly without the least understanding the inner possibility and necessity of this sort of accomplishment (Hua VI: 52, E.: 52).

The French phenomenologist Michel Henry has put this 'not understanding oneself in one's own accomplishments in somewhat harsher words. He called science a self-negation of life, which summarizes the phenomenological problem of the oblivion of the life-world – the clash of the reality "of" and *of* science.

> Science, on the one hand, is a mode of the life of absolute subjectivity, and belongs to it in and for itself. On the other, every accomplishment of scientific subjectivity is achieved as the bracketing of this subjectivity (...). Science thus presents to us a form of life, that turns against life. A life, that negates itself, the self-negation of life as such, is that decisive event which makes modern culture a scientific culture (Henry 1994: 204*).

(5) The Correlate of Theoretical Experience: The Invisible Hand

The invisible hand, according to the Nobel committee of 1983, is what Smith had in mind and Debreu proved. If we ask a historian about the invisible hand he or she could tell us that Adam Smith may have read the expression in Shakespeare (Macbeth, Act III, Scene ii), and that he used it for a somewhat non-Smithean argument. All later uses of this expression, the historian would add, go far beyond anything Smith could have had in mind. Debreu, most likely, never read Smith's *Wealth of Nations*, let alone the *Theory of Moral Sentiments* where the expression is mentioned, too. A philosophically inclined person, moreover, could tell us how Hegel's 'cunning of reason' shares Smith's intellectual flavor since both actually "discourage and deflect rational investigation" (Davis 1989: 65). Then, if we ask a 'generally educated person' about the invisible hand, he or she may perhaps say something to the effect of self-interest being good for "the economy", or, conversely, governments being bad for "the economy".

There is no meaning authority for the expression "invisible hand". Nor is there an authority that says what counts as a valid interpretation. If it denotes anything, then, I suppose, *economic theory*. The expression of the invisible hand has had considerable success in identifying economic theory in general economic talk. One could, as I do, also take it as an (empty) placeholder for large parts of the history of economic theory that ranges from Smith's first humble use of the expression to the tradition of general equilibrium theory (GET). In order to capture both the identifying role this expression plays in economic talk and the tradition of (mis)interpreting "the invisible hand" in economics, I shall rather restrain from narrowing its meaning in advance. Particularly, I do not take position regarding the relation of the invisible hand, liberal laissez-faire policies, the market mechanism, general equilibrium theory, and the very perception of "the economy". The ambiguities involved in the relation of these terms are rather constitutive for the establishment of an object of economic science. A prior definition would amount to nothing but question begging.

With the title of the invisible hand, phenomenologically speaking, I discuss the "intentional correlate" onto which scientific authority is claimed. To start with, let it be "the economy" – whatever that means. Although I do address what economists are concerned with – their field of expertise – I do not intervene in their concerns. The invisible hand does not add much to my account of economics. It is not a new topic since an intentional correlate, according to

Husserl 'parallelism', cannot show "more" than this act itself. Hence "the economy" is the extract of economists' intellectual discreetness. Just as formalism describes the degeneration of the act of economic theorizing, the invisible hand describes the degeneration of the correlate of this act – the actual theory representing something. While formalism is the title for adopting a low tone, the invisible hand is a title for saying less.

The metaphorical aspect of the expression points neatly to the phenomenological dilemma I associate with economic theory: One cannot exhibit the reality of "the economy" for it is "invisible", that is *non-intuitable,* and *insensible – anonymous*. Anonymity for Husserl meant the same as phenomenologically not being exhibited, but nevertheless functioning implicitly – as it were, "on the horizon". What is anonymous, for Husserl, demands to be explicated, demands to go through, and in this sense to be made "evident". Yet the anonymity of the object of economic theory goes further. "The economy" can never be made "evident", cannot be inquired, and cannot be exhibited. The invisible hand refers to the deficient phenomenality of "the economy" as an object of theory. It describes "the economy" in that it is not bestowed by the meaning of the experiences of economic life. "The economy" is senseless. For this reason "the economy" is evasive of a phenomenological analysis. And for the same reason, as I am going to introduce in the following, the tradition of economic theory results in *structuralism*, in particular the structuralism of the axiomatic method. In this chapter I thus set up my phenomenological conception of the history of economic theory.

The Search for a Generic Object of Economic Science as the Possibility of Claiming Scientific Authority

As is well known Schumpeter has deemed Walras, "so far as pure theory is concerned (…) the greatest of all economists. His system of equilibrium is the only work by an economist that will stand comparison with the achievements of theoretical physics" (1954: 827). Schumpeter believed that Walras' GET represents the analytic core of economic science, and all theoretical achievements until and since Walras ought to be judged in terms of this theory. For Schumpeter GET defines the very essence of economics. The proof of a uniquely determined equilibrium is vital for that essence:

> [F]rom the standpoint of any exact science the existence of a uniquely determined equilibrium is, of course, of the utmost importance, even if proof has to be purchased at the price of very restrictive assumptions; without any possibility of proving the existence of a uniquely determined equilibrium (…) at however high level of abstraction, a field of phenomenon is really a chaos that is not under analytic control (1954: 969).

Essentialism apart, I do agree with Schumpeter to the extent that it is indeed difficult *not* to put GET in the spotlight when speaking about the history of economic theory. Most innovations in both classical and neo-classical theory found a sediment in an "advance" of the constituents of this grand theory – one exception may be Keynesian economics that had its heydays between 1940-1970, though considering its incorporation in the discipline, it hardly differed from GET. Economists today, of course, deem themselves to be free from GET. Only a small minority still pursues it. The attempts to overcome its notorious "assumptions", however, still hold economic theories together, as I venture to show. In 1971, Arrow and Hahn were the last who could proudly announce in their *General Competitive Analysis*:

> [T]he notion that a social system moved by independent actions in pursuit of different values is consistent with a final coherent state of balance, (…) is surely the most important intellectual contribution that economic thought has made to the general understanding of social processes" (Arrow and Hahn 1971: 1).

GET, according to these lines, encompasses the whole of economic theory since the 18th century in its "most important intellectual contribution". What are the constituents of this theory according to Arrow and Hahn? The object of economic thought is the "social system", the units of analysis are the "independent actions in pursuit of different values", and the scientific problem is the "consistency with a state of balance". In this manner economics contributes to the "general understanding of social processes".

In these lines, we get a glimpse why one can hardly conceive of the history of economic theory without reference to GET. With this theory economists gain a generic object of study, which they do not share with other scientists – a generic economic object. Only economists hold a theoretical perception of "the economy". Economics could not have evolved as a separate discourse if it merely would have claimed what its discursive allies and opponents already believed (like 'corn prices are too low for our British landlords', or 'poverty laws may not help the poor'). There needed to be something beyond all the particular concerns of the people, but nevertheless determining it: an object *remote enough* to establish the distance necessary for a scientific ethos, but still *close enough* to claim authority in economic talk. In this sense, the tradition of theorizing "the economy" represents the locus of contests for the discursive identity of economic science. I thus depict the scientification of economics along the search for this generic object of "the economy". By means of cultivating the theoretical perception of this object, economists can claim epistemic authority.

Putting GET in the center of economic theory, I thus do not, unlike Schumpeter and others, count this piece of theorizing in the essence of economics. Instead, I conceive it as a guide through the contests about a generic object, domain, and thus identity of economics. Thee contests, broadly framed, concern the relation of "the economy", the market, and liberalism. Far from engaging in an actual argument in economic theory, my major claim concerning economic theory is this: Without the theoretical perception of "the economy" there is no opportunity for claiming scientific authority. Regarding all other topics in economic talk that are based on a notion of economic life there cannot be any motive, let alone the need for appealing to the authority of science.

What, then, are the historical ingredients of this tradition? What had to happen in order to arrive at a formulation of the core of economic theory as described by Arrow and Hahn? According to my image of the history of economic theory, there are at least seven steps to be taken. Even before there was the expression 'invisible hand', first, a theoretical perception of "the economy" arose within the rhetorical strategies to *avoid the imposition of self-interest* to the economic author (Thomas Mun 1664). Second, it came to denote the credo of early modern liberalism, namely the belief in *social beneficiary results of mercantile rivalry* (Adam Smith 1776). The third step in cultivating the perception of "the economy" was to sort out phenomena of *production and consumption of wealth* (John Stuart Mill 1848), which then, fourth, after the rise of the institutions of economic science, could lead to the doctrine of the *self-regulation of supply and demand* (Marshall 1890). Fifth, after marginalist reasoning became established, "the economy" was associated with *the allocation of resources given certain ends* (Robbins 1932), and, in particular, with the efficient allocation under the heading of the welfare theorems (Lange, Lerner). Beyond the political implications of these theorems, however, the core question of economic theory crystallized in the 1940s as, sixth, the *determination of the price system* (Samuelson, Hicks), that ended up, seventh, as the determination of the *conditions under which a general equilibrium exists* (Debreu 1959). In the 1970s this became the core of economic theory to which Arrow and Hahn referred as "the most important intellectual contribution" of economics. These steps represent the horizon within which I discuss the history of economic theory.

These steps leave open what is special about economic theory. Why should not people other than economists deal with the analysis of competitive markets which seemingly is the threat of this tradition? The nub is, however, that only along competitive markets could economists cultivate the theoretical perception of "the economy". The market is the "invisible" mechanism by means of which "the economy" is constituted. The decisive result of the "sense-coverings" that resulted from the history of economic theory – its Whig history – was the belief that there is an "economy" independent of the perceptions surrounding competitive markets. "If one goes back a few centuries", as Schabas agrees, "it is by no means clear that people, even the learned communities of Western Europe, perceived such an entity as the economy" (2006: 1). Without this belief in such an entity, I will argue, economic theory would have remained one among other political or social theories. For market theory differs from anything else social scientists could inquire since in markets there is "social order" *independent* of the nature of that which is ordered – the individual, its contingent needs, culture, history, morals, etc. Without that conception, there would be neither talk about "the economy", nor a separate economic science. To come up with a concept of social order without presupposing a social inclination is the distinct and outstanding accomplishment of economics as a science – justifying without presupposing.

Although "the economy" guarantees a generic object of scientific authority, economists have yet to say something about "how it goes with the economy" and what to do about it. By virtue of cultivating this issue economists virtually connect with economic talk. The market needs to be in some way or another *expressive* of economic life. At the height of its development, roughly between 1850 and 1950, the basic intuition of economic theory was indeed that the market represents the state of "the economy" as if it were a language of economic life. Such was perhaps *the* unifying image of the nature of economic knowledge. Knowing the market mechanism was to be able to assess how it goes with "the economy", which in turn was believed to be an expression of economic life. When I speak of the self-degenerating character of economic theorizing, it is because this intuition had to give space to scientific authority. Economists are increasingly less expressive of this notion of economic knowledge. Within the conditions under which a general equilibrium holds, there is nothing whatsoever through which "the economy" could count as a representation of economic life. In other words, only by means of the confusions between "the economy", the market mechanism, and liberal policies economists appeared to have something to say, and could exert scientific authority. Therefore, the social history of economic theory, as Frank Hahn was the first to acknowledge, is the history of confusions and misunderstandings.

To put it straight, the strong claim I attempt to make regarding economic theory is this: Without economic theory there was no talk about "the economy". "The economy" is a theoretical perception that arose from nothing but the need for scientific authority. Before economic science, there was no motive for such perception. Economic talk is about "the economy" (and how it goes with it) because of economic science. The contribution of economics to the modern economic talk is that "the economy" has become *the* locus of argument and authority. The rise of economic science and the perception of "the economy" mutually depend on, and reinforce each other. The more this perception dominated economic discourse, the more the social identity of economists and their academic institutions were legitimized. Saying so, I do not take position regarding the reality of "the economy" – in whatever of the seven meanings distinguished above. Such is the business of the philosophy of economics, and a different matter in each of the seven steps. Prior to the unhappy opposition of scientific realism and constructivism, I ask about the conditions of the possibility of the very perception of this elusive object, "the economy".

The key to this claim is that the formalization of economic theory did not happen by chance. The tendency to formalism says something about the very nature of economic theory. Some historians of economic thought have supported this association. Here, for example, Bruna Ingrao and Giorgio Israel in their classical study in the history of GET: "Our thesis is that the problem of mathematization is no secondary feature of general equilibrium theory but rather one of the basic reasons for its creation and development" (1990: x). Or here another historian, the often-quoted Christopher Bliss: "The near emptiness of general equilibrium theory is a theorem of the theory" (1993: 227). Rather than as a mere complaint, this historical insight provides a vital clue for the social history of economic theory.

If I take the association of economic theory and formalism serious, the "invisibility" of the invisible hand is not an epistemic deficit, but is inherent to economic theory and the very reason of its public success. The epistemic problem of "the economy" in economic theory is then not whether, by virtue of which laws, principles, causal chains, or underlying mechanisms the market determines economic life and vice versa. "The economy" constitutes an epistemic problem only as a title of intellectually reaching a point beyond economic life, in particular beyond its special interests. As soon as the invisible hand is spelled out in its social political meaning, I will suggest throughout the text, economic science can no longer hold up its discursive identity. As soon as one disentangles the relation of "the economy", the market, and liberalism, economic theory appears either politically irrelevant, or biased. The invisible hand is, therefore, the theory that allows for the disciplinary formation of economics and challenges at the same time the very possibility of intellectual responsivity.

There is another apparent obstacle in order to appreciate my claim – namely, that present-day economists deem beyond the "paradigm" of GET. For them, the preceding remarks must seem like economic metaphysics from yesterday. Is it not entirely outdated to put the invisible hand and GET in the spotlight of economic theory? It may be important for teaching economics, but it is irrelevant for most actual research. Economic theory today has moved beyond GET, and also beyond its accompanying exclusion of empirical methods. Granted!

Since the 1970s there has been a shift of the literary paradigm of economic theory from allocation of resources to, as one of the most energetic commentator of this switch has put it, "The New Modern Answer: The economic agent as a processor of information" (Mirowski 2001: 8). This is true not only for behavioral economics and complexity theory, but also for heterodox economics such as institutional and Austrian economics. Focusing on the *social* history of economics, however, we should come to acknowledge that economists' *ethos* is not affected by this paradigm. To the contrary, while the New Modern Answer may accurately describe *what* economists are concerned with today it does not describe what is *problematic* about it. Just as it is crucial for my phenomenological approach to take formalism instead of objectivism as a point of departure, it is crucial to depart from invisible-hand theorizing instead of theories of *economic behavior*. Against all talk about The Individual in economics, I will insist that the phenomenological problem of economic theory is not a particular conception of what *economic life* is about. Economic theory, today, just as half a century ago, and just as two centuries ago, is the diversion of the question: Who? What kind of people?

That the individual defines *the* locus and unit of analysis of economic theory, as in contemporary economic theory, is a *consequence* of, rather than an alternative to, the received tradition of economic theory. The New Modern Answer is preceded by the Old Modern Question of the conditions under which a general equilibrium holds. Present economic theory is inconceivable without being preceded by this issue. Otherwise I had to suppose that the only problem of economists is that they are so astonishingly bad anthropologists. But no. The new paradigm, and this is what makes it nonetheless a modern paradigm, does not correlate with a commitment to a particular anthropological conception of economic life. If contemporary economic theory puts the individual in the foreground one does not seriously ask: What Kind of People? Instead, one asks: How can we escape from the neo-Walrasian trap without losing our face as economic scientists? My answer is clear: No Way!

In part, an anthropological intuition of economic life was present in economic theory during its early and high modern period. Among the imaginaries that identify economic theory *homo oeconomicus* indeed ranks as prominent as the invisible hand. Until today, one hardly distinguishes between these two in general economic talk. The development of economic theory, however, was not accompanied by an advanced understanding of who *homo oeconomicus* is, but rather to divest this subject down to the so-called "atomistic individual", an element in a structure, $x \in X$. In economic theory one did *not* ask what are the implications for the order of "the economy" if the individual behaves such and such (as it appears in most present-day textbooks). The question was rather: What do we have to require from the individual in order to maintain our theoretical perception of "the economy"? The success of economic science was that it did not have to require *anything*. The individual is secondary for economic theory.

How far removed economic science is from an intuition of economic life, let alone an explicit anthropological notion of *homo oeconomicus*, is clear if one recalls those who seriously engaged in the what-kind-of-people question: All pre-modern writers have been preoccupied with it, above all the clergy who made a living from condemning some kind of people – mostly Jews for being merchants and vice versa. The same question dominated the intellectual milieu of merchants during 17th century against which economists had to gain identity. It was central to Adam Smith and his *Theory of Moral Sentiments* (1759), and was fatal for Marx's image of the capitalist. It was turned on its head with Schumpeter's entrepreneur (1934 [1911]), moved towards sociology with Max Weber's *Spirit of Capitalism* (2003 [1904]), and once again took an anti-Semitic shape with Sombart's *Modern Capitalism and the Jews* (1982 [1911]). How could we possibly think of The New Modern Answer as carrying on such writings? Today, those who do attempt to truly renew this literature, as recently McCloskey in her *Bourgeois Virtues* (2006), how could there even be the question that it could contribute to the body of economic theory? As soon as the question 'What Kind of People?' is posed, certain forces work against the possibility of claiming scientific authority. To exhibit these forces is one of the tasks of The Phenomenology of Economics.

Ignoring the individual while pretending to care about it, and ignoring science while pretending to claim it are associated. Mirowski and Sent agree in a poignant opening sentence:

> It is a commonplace observation that economists love the Individual; it is just real people that they cannot be bothered about. A wag once added that economists also prefer to love Science; it is just real scientists that make them nervous (2002: 1).

From the Temporal Order of the *Oikonomia* to the Structural Order of "the Economy"– From the Economic Suspicion of Mercenary Motives to the Topology of Existence Proofs

In order to flesh out the phenomenological approach to economic theory in more detail, let me sketch the historical arch from the logic of premodern economic discourses, to Smith's use of the invisible hand, down to the neo-Walrasian dead end of this tradition. The plausibility of my use of "the invisible hand" depends strongly on this historical perspective I adopt. Invisible-hand theorizing encompasses a history from Thomas Mun to Mas-Colell.

First, what was economic talk before we, Europeans, started speaking about "the economy"? Before that there was no theoretical perception of "the economy", but there was a different object of concern: the *oikonomia*. *Oikonomic* writings – that ranged from Xenophon 360 BC to roughly the housefather literature around 1800 – did not claim epistemic authority, let alone referential truth. They adopted either an instructive or a preaching tone. The oikonomia was a matter of *practical advice* and *moral imperative*. It was certainly not the title of a distinct object of study. Instead, it was the primordial manifestation of an encompassing order to be achieved. Oikonomic writings were concerned with economic life as a *task* for everyone. Referential truth matters to these writings in the same sense as ontological assumptions matter for a good cooking manual. The "acquisition of wealth", as Aristotle defined the purpose of the oikonomia in the same terms as later political economists, was a matter of learning how to do things and of being good. These concerns of economic life in some way or another were related with the experience of needing, the concrete material equipment associated with needs (tools), the efforts one puts into their acquisition (labor), as well as the social relationships one entertains with those who depend on one's economic life (family). Most of economic talk outside the shrine of science still refers back to these terms.

The becoming of economic science amounts to the attempt of a *structuralist* turn in economic talk that excludes the expression of a notion of economic life. Until the formalist revolution, I will show, this exclusion was not clearly perceived. Most modern economists believed that the social structure of "the economy" somewhat *represents* the finite reality of economic life in whatever mysterious way. They did not give up their intuition of economic life that stem from oikonomic writings. Smith certainly did not; neither did Marshall, who thought about the "ordinary business of life"; nor Edgeworth, who spoke about "trade" and "war" as the primordial domains of applicability for economic theory. And most economists today still know that "the market for children" and "the market for ideas" are *analogical* expressions. Most economists thus grant that "the economy" has its home ground in economic life. Perhaps, as I indeed suggest, there was never any economist free from such an intuition of economic life. That is except Gerard Debreu!

Debreu – standing at the top of the cultivation of the theoretical perception of "the economy" – however, did not aim at any economic claim and could not live up to a self-perception of an economist! This is the *skandalon* I will hang my narrative on. The difference between economists before and after Debreu is that even if one may still share the common sense that "the economy" represents and means something *for* economic life, this intuition is not expressed, neither worked on, nor in any sense at stake when doing economic theory. Structures being the object of science, the economist does not face, encounter, or could express a notion of economic life. There is no phenomenological reason why economists should retain-in-grasp the "intended applications", to use Stegmüller's term, when engaging in economic theory – which buttresses Hands early critique of the philosophical position of structuralism in economics (1985). And this holds true, as I venture to show (2.4), even more so as long as The Individual is allegedly economists' concern.

Let me relate this notion of the structuralist turn to Husserl. The shift from the *oikonomia* to "the economy" can be compared with the shift from "world" to "nature". In these terms, Husserl discussed the scientific revolution when natural philosophy was replaced by natural sciences. "Nature" in its depiction of ideal space and time initially meant a method to arrive at claims such as 'the distance of a falling object is equal to half of the gravity force times the squared time the object falls'. But as a result of the suggestive force of the exactness of this "method", as already discussed above, nature came to represent "the real world" – or better: reconfigured the conditions of what counts as real. This happened for the simple reason that one no longer asked: What was the purpose of developing the idea of ideal space and time? By means of the omission of a question, "world" became identified with "nature".

In which sense, then, was "the economy" initially method rather than object? In 17th and early 18th century Britain, an epistemic revolution took place in economic talk because of a *rhetorical need* to react to the pervasive economic suspicion to be guided by one's own interests when pretending to instruct the King. Talk about "the economy" was an effective way to move *beyond special interests*. This diversion from interests was the initiating motive of economic theory and will remain its engine of scientification. For this reason, the basic tenet of economic theory is that there cannot be any interest *for* "the economy", but only interest *within* "the economy" – in the words of Hayek's formula of the invisible hand: "The Results of Human Action but Not of Human Design" (1978), or, in Wall-Street jargon, 'nobody can beat the market'. The phenomenological problem of the market is its *epiphenomenality*. The market never presentiates (*vergegenwärtigt*) the experiences of economic life, but exclusively re-presents them. This epiphenomenality makes us believe that "the economy" can be an object of referential truth claims. It never has been, so my strong claim. More importantly, I show that this belief is self-degenerating regarding economists' ethos.

Moving on to the originator of the expression "invisible hand", Adam Smith, I like to give an impression of the richness of the moral connotations that later constituted the affective connotations against which the scientification of the invisible hand had to gain ground. The expression "invisible hand", for Smith, was rather contingent. Smith used it not more than three times – in his *Wealth of Nations*, *Theory of Moral Sentiments*, and *History of Astronomy*. In the last case, he spoke about the "invisible hand of Jupiter", referring to the superstitious belief in invisible forces (Macfie 1971). Smith did not use this expression out of conceptual rigor but rather out of a quandary in his argument for the sake of adding ironic wit to his text.

There is no need to go through the subtleties of Smithean exegesis (see for example Persky 1989, Rothschild 1994, Grampp 2000). None of these subtleties entered the history of economic theory. For how could economic theory possibly proceed by literary exegesis? Moreover, as Rothschild argued, the three times Smith used the expression were not even particularly representative for his work. The use of it referred to "a thoroughly un-Smithian idea" (Rothschild 1994: 320), for it expressed Smith's disrespect of those who do not use their capacity of reason properly. Against his belief in human reason, Smith ridicules "silly polytheists, rapacious proprietors, disingenuous merchants" (Ibid.).

Let me nevertheless consume some space to quote once more the respective lines of the *Wealth of Nations*. The expression was used, as all political economy in the decades around 1800 in the context of an argument in favor of free trade. But Smith also could have argued the same without this expression, because it did not represent the core of the argument, but rather a response to a counter objection (Grampp 2000). Smith expressed his doubts about those merchants who pretend to act in favor of the common interest.

> By preferring the support of domestick to that of foreign industry, he intends only his own security; and by directing that industry in such a manner as its produce may be of the greatest value, he intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. Nor is it always the worse for the society that it was no part of it. By pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it. I have never known much good done by those who affected to trade for the publick good. It is an affectation, indeed, not very common among merchants, and very few words need be employed in dissuading them from it (Smith 1976: 456).

In the second mentioning in the *Theory of Moral Sentiments* Smith used the expression when discussing the use and uselessness of poverty laws, which was, next to trade, another great issue for economics in the 17th and 18th century. The rich, though acting out of self-interest, ironically end up sharing their riches with the poor. The point is trivial. The poor are less poor as long as there are some rich who need the poor for their services.

> (…) in spite of their natural selfishness and rapacity…[the rich] divide with the poor the produce of all their improvements. They are led by an invisible hand to make nearly the same distribution of the necessaries of life, which would have been made, had the earth been divided into equal portions among

> all its inhabitants, and thus without intending it, without knowing it, advance the interest of the society, and afford means to the multiplication of the species (Smith 1976b: 184f.).

Rather than interpreting these lines, think of its intellectual wit. For definitions I would look in vain in Smith's text. The text is grounded in the political discourse of mercantile capitalism of Britain and of no other country (economic writings during this time in France and Germany were by far more sober, like book-keeping of administrators). Smith's theoretical interest is not apparent. The invisible hand is, rather, a literary expression in reaction to the contempt of merchants, which was common among intellectuals of these days. Smith is concerned with the political naivety to simply blame merchants, although he too shared this commonly held contempt. Smith moves beyond the belief that society is nothing but the manifestation of the moral commitments to society, but nevertheless remains within this moral sphere.

Apart from Smith's texts, consider the general connotations that the invisible hand can have in moral discourse. In nuce, the invisible hand represents a separation, a *distance*, a split, a distinction between economic order on the one hand, and economic motives on the other. This distance is vital for the possibility of a scientific ethos. But the same separation also has connotations other than epistemic. There are moral sub-tones. Separating economic order and motives can be said *sardonically* to be of good will, but cause bad results. This in turn can be said with a *gloating* tone against those who do want to be good. For the agents themselves such a situation is rather *tragic* in a very Greek sense of an imposed destiny that leads man to act against his own will – the moment of "what have I done?" Such ironic-tragic meaning can also be turned around when a bad result is *excused* with good motives – a rather Kantian position of as-long-as-we-want-the-good. This again can be an excuse for taking *social responsibility*. If this excuse does not convince, and the consequences gnaw at one's conscience, the separation can also correlate with a rising feeling of *guilt* for the unintended consequences of one's actions – the horror of nobody's-fault-but-everyone's-responsibility. Finally, it can be used, as most other economic discourses perceived the moral meaning of the invisible hand, as a free ticket for the pursuit of opportunism, fraud, and greed – of 'stabbing in someone's back', as in Macbeth.

Against the background of such manifold and contradictory moral connotations of the separation of economic motives and order, a scientific ethos of economists had to gain profile. Considering the two centuries of economic theory after Smith, what happened is, roughly, that this moral field of connotations lost its intellectual wit. Economists articulated a clear-cut structural vision of order that comes about "by itself" – *sponte* – without someone who orders, and without moral agents. But the path economic theory had to take crossed the one or the other mentioned moral connotations. From the point of view of economic theory these connotations describe the field of possible "misunderstandings". In the course of the two centuries of economic theorizing the main contests that surrounded economic theory became ever lighter in terms of moral weight. The theoretical debates that accompanied this moving beyond moral discourse constitute the *interpretive gulfs* of the invisible hand.

One of these gulfs was certainly the line between *economic growth* and *efficiency*. While growth and the causes of wealth was *the* question that made political economy "political", growth theory today is not more than a macroeconomic surrogate of GET. And nothing is farther removed from the classical notion of 'wealth' than the "neoclassical" notion of 'welfare'. Just think of the theorems of welfare economics. Who today would claim epistemic authority on what economic growth, or wealth really amounts to? Growth and the pursuit of wealth are overly burdened with Marxian connotations of "accumulation", or, since Galbraith, with connotations of "affluence". The question of 'the causes of growth' is hardly a befitting question in the ranks of economic theorists.

The great behavioral issue was certainly the difference of competition and rivalry. Rivalry referred to the aggressive behavior of a particular class, merchants. Rivalry was discussed as the engine of innovation and creativity (Schumpeter), or as the particular alertness of the entrepreneur (Kirzner). Competition, instead, refers to the structural condition of the *giveness of prices*, saying, that nobody can influence prices – the absence of market power. As a result, actual competitive behavior for "favorable locations, product innovations, advertising wars, quicker delivery, improved maintenance, and service guarantees" (Blaug 2003: 153), is ostracized from economics to business departments. Indeed, I remember a business economics class where students had to add prices as an argument *in* the utility function as though it was a property like "blue" or "delicious" – what an abuse! I thought.

There are many other dichotomies that shaped the struggle for an identity of modern economic science, such as that between allocation and distribution replacing that of production and consumption, or the difference between historical vs. mechanistic time, the orientation at the natural vs. the human sciences, the degradation of Benthamian utilitarianism from a social doctrine (Mill) to a naturalistic program (Edgeworth) down to a behaviorist devise (Samuelson), furthermore the difference of science and art of economics, and, of course, the system question *par excellence*: socialism vs. capitalism. Such and many more interpretive issues of economic theory circumscribe the field of expressive activities of economists – at least until the formalist revolution. At the same time, however, these interpretive issues made economic theory vulnerable to the moral misunderstandings of others.

The actual history of economic theory did not result in the resolution of any of these interpretative issues. Economists have never proven whether Kant or Bentham suits "the economy" better, whether rivalry does good to mankind, whether growth happened by means of becoming more efficient or more creative, and by no means did anyone ever show whether the market is what led us into the present disaster or what will lead us out of it. The history of economic theory is a history of becoming evermore indifferent to matters of meaning. The decisive question in 1945, for example, was not whether socialism or capitalism could guarantee that history will never repeat itself, but how to get topology – a field in mathematics – into the price system! From the imposition of mercenary motives to the topology of existence proofs leads one Big History of softening edges, lowering tones, oppressing what is at stake. At its end there remains one structuralist *Gestell*, the Arrow-Debreu model in which all interpretive gulfs of economic theory are covered by one mathematical mantle: $x \in X$.

Certainly, economists today believe that the Arrow-Debreu model really implies particular economic claims. There are substantive assumptions of agents (transitivity), of the commodities (convexity), and the allocation mechanism requires some sort of *tatonnement* process including an odd subject that sets the price system. For Arrow such "assumptions" were indeed crucial since they made him move beyond GET to other theories, such as social choice. For Debreu, however, these problems did not affect his theoretical interest. How could he possibly believe that "transitivity" describes or demands anything of the conduct of an economic life? The following lines taken from the first page of the Arrow Debreu paper of 1954 – the "centrepiece" of the formalist revolution (Blaug 2003: 146) – illustrates the self-degeneration of economic theory. They tear away the great interpretive marks in one move.

> The investigation of the existence of solutions is of interest both for descriptive and for normative economics. Descriptively, the view that the competitive model is a reasonably accurate description of reality, at least for certain purposes, presupposes that the equations describing the model are consistent with each other. Hence, the check on the empirical usefulness of the model is the prescription of the conditions under which the equations of competitive equilibrium have a solution (Arrow and Debreu 1954: 265)

Subtle lines. It seems as if Arrow and Debreu really have concern for "empirical usefulness" and "normative implications". But what these lines say is that their model gains scientific value by means of abstracting from both. Koopmans, the most outspoken defender of the axiomatic method, repeated: "The descriptive theory of competitive equilibrium (…) and the normative theory of the use of prices for the efficient allocation of resources appear as two sides of one coin" (1957: viii). What Debreu achieved (and Arrow made looking for other answers) was to gain control of the Walrasian framework *before* only the slightest issue of interpretation could arise – in particular regarding the nature of economic agents. At this juncture my narrative will differ markedly from that of Mirowski: "the computer was the instrument that permitted marvelous latitude in interpretation and flexibility in negotiations over that vexed imperative of 'rationality'" (2001: 311). For precisely this reason Debreu was *not* inspired by it. His work represents a real rupture and was not merely "little better than the nineteenth-century yearning for a perpetual motion machine" (Ibid.: 414, see also 9). Debreu represents a rupture since he liberated the economist from the burden of negotiating the meaning of theoretical principles. His achievement was to free economists from the *weight of meaning*. Here my Preliminaries return from where they started.

What remains after this liberation from the weight of meaning, is a *sensual* experience that is no longer of an epistemic but of a sensual kind – the sensation of an *aesthetic void* of a self-entailed system. Also Adam Smith spoke of the invisible hand, particularly in the context of the invisible hand of Jupiter, of beauty. "We take pleasure in beholding of so beautiful and grand a system" (in Rothschild 1994: 320). This is perhaps the only aspect of the invisible hand that actually remained the same throughout the history of economic theory. "Economic theory", Rothschild adds, "was in the 1750s, and is now, an aesthetic experience" (320). This experience that frees the economist from the dirt of the cultures of economic suspicion is the last source of affects that keep economic theory going. It appeared most clearly in the structuralism of Nicolas Bourbaki that informed Debreu's axiomatization of GET.

Let me repeat. I will, as my exposition should have made clear, hardly argue about economic theory itself. If there is anyone who should do so, it is the economist. Instead, I read economic theory, socially, as a reaction to the moral suspicion that guided all pre-modern economic discourses and, moreover, as a corner stone of all efforts put in the scientification of economics. The epistemic problem of "the economy" in economic theory is not a matter of referential truth. "The economy" constitutes an epistemic problem only as a title of theoretically reaching a point beyond special interests. For this reason the history of economic theory correlates with a tendency to formalism. The invisible hand describes, on the level of theory, the phenomenological problem of scientific practices between the search for a generic object and the risk not saying anything.

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So far The Phenomenology of Economics in its vertical section. Before I enter its exposition, let me remind that phenomenology serves less as a conceptual scheme or even method imposed on the body of economics. Most of the understanding depends on the so-called "phenomenological reduction" that I tried to introduce rather informally in the preceding pages. The Phenomenology of Economics does not deal with what economists are interested in, but mainly with this interest itself – the subjective constitution of economics. Philosophy of science being concerned with properties of scientific theories, science studies with the reality of the theorist, the correlation was never tackled.

Since I am skeptical about the possibility of a phenomenological science of economics, I cannot call this reduction a "method", as Husserl did. Phenomenology is not a method for apart the essence of economics. If at some point the notions presented in the preceding Preliminaries will no longer be conceptually recognizable, it nevertheless informs every line of it (it would, for example, take another book to conceptualize fully the notion of *affective history*). Phenomenology merely determines the tone, and the attitude of the analysis. Phenomenology is not a *method* in the sense of a tool, but it is method in the sense of ηξεισ – attitude towards the other. And this attitude is inviting instead of teaching, evoking instead of delimiting, provoking instead of calming, but above all addressing, and at last motivating for a reflection on the motives for claiming scientific authority in economic talk. In this sense my exposition is a "way into transcendental phenomenology", for the purpose of which Husserl once has designed the notion of the life-world.

For the phenomenologist who is interested in the social sciences, there is sufficient news in terms of the material I present. For great parts of the text, I do not suppose training in economics. Even the most exclusive spheres of economic science, GET, I discuss, thanks to the phenomenological reduction, without the technical issues involved. For the economist, instead, the material I present entails no great news (apart from some fine points on Bourbaki as well as some details about the person of Gerard Debreu). The argument lies more in the way it is presented and the associations it evokes. The novelty and challenge of The Phenomenology of Economics is that it claims (rather than represents) economics in its historical whole, reflects on the horizon that makes it a human practice, and thus goes beyond the scientistic limitation not to reflect on the significance of science.

In this project I combine both, the historically broadest view on economists' intervention in modern history (*Part 2*), and a piece meal approach to the intellectual becoming of the most inconspicuous mathematical economist, Gerard Debreu (*Part 3*). For the historian of economics it is vital to keep in mind that I am not arguing as a historian, but in favor of the historian. I do not turn to history as an object of representation. History is the locus of intellectual tasks. As soon as history degrades to the history of facts, the historian gave up treating history historically. If I cannot and do not pretend to meet the standards of the history of facts, then it is mainly for the reason that I have doubts about the historical genesis of these standards. After the following narrative, economists will perhaps be more interested in the *Wealth of Nations* than that Gerard Debreu proved it.

And so the character of my argument is not to convince the economist of something. The text does not argue for a particular truth, but wants to *appeal* to an instance that somewhat became stunted in the three or four years of becoming an economist, in the three or four decades of gaining an academic ethos as an economist, in the last three or four decades in which economists have tried to regain significance, in the last three or four centuries scientific authority has intervened in economic talk. I neither aim at a judge of today's situation in economics, nor at a judge of any economist. This is up to the economist him- or herself! I rather want to take economists in charge and invite them to respond to their current situation, and have them take social responsibility for their profession. It is this social responsibility of economists that The Phenomenology of Economics calls for. The following exercise can be read as a long invitation to a self-reflection on the motives of doing economic science.

Visible economists 2002:
Princeton Economics Department chairman Ben Barneke
takes over the FED Board from Alan Greenspan,
co-author of Ayn Rand's *Capitalism: The Unknown Ideal* (1967).

Part 1
Discourse

Invisible economists 1946:
Friedrich Hayek launches the Mont Pellerin Society.
Other members are Ludwig Ehrhardt, Walter Eucken, Milton Friedman,
Luigi Enaudi, Karl Popper, Frank Knight, Fritz Machlup, Georg Stigler...

Part 1
Discourse
The Public, Professional, and Pedagogical Ethos of Economists

A phenomenological analysis – as I suppose economists, too, associate with this tradition – begins with a certain naivety. Phenomenology is associated with a "mere" description of things "just" as they appear to us, that is to say, without intending to judge them, without questioning their existence, without trying to explain them, etc. When withdrawing from a judgment (*εποχη*), the difference between a richly described character in fiction and a hard fact in an analysis vanishes. They are, prior to that difference, *given* to us. They can be described in such way that the difference between fact and fiction is the result of the "modes of giveness", as Husserl liked to say. A phenomenological *εποχη* is like the democratization of meaning. Everything is given the occasion to appear in the mode proper to itself.

In this part, I invest this favor for naivety in a description of the world of economists "as it appears to us". This description is a first heuristic step. It is written in the naïve attitude that economists' practices can be described just as any other human practice. Such description is pre-critical. The limits of this exercise will introduce the phenomenological problem of economists' world. In this way I grant voice to the lamentations about economics in its present state as they pervade most skeptical critiques of economics.

What is the nature of this phenomenological naivety and how does it relate to my guiding notion of the life-world? The early Husserl framed this naivety of "mere description" in one of his major works, the *Ideas I*, in a rather pontifical and Kantian fashion as "the principle of all principles":

> No conceivable theory can make us err with respect to *the principle of all principles*: *that every originary presentive intuition is a legitimizing source of cognition*, that *everything originarily* (so to speak, in its 'personal' actuality) *offered* to us *in 'intuition' is to be accepted simply as what is presented as being*, but also *only within the limits in which it is presented there* (Hua III/1: 43, E: 44, emphasis Husserl)

Because of such lines, phenomenology came to be known as a description of original, immediate experience. In a Cartesian fashion, Husserl believed the 'originary presentive intuition' to be capable of a foundation of knowledge. Pure experience represents a 'source of cognition' in that it presents to us the stuff of meaning before it becomes meaningful.

The naivety of this principle of naivety is apparent. How could this layer of pure experience be an object of description as though there were not only an originary presentive intuition, but also an originary presentive language? Hermeneutists critiqued Husserl on such grounds. They deemed it necessary to move beyond his Cartesianism, and to take meaning as an entirely discursive rather than experiential title.

With the notion of the life-world, however, the late Husserl corrected his Cartesianism. Figuratively speaking, the life-world grants some 'thickness' to the originary presentiation of meaning, to such extent that the life-world inheres in the language that articulates it rather than being opposed to it. With the life-world, the unfathomability of meaning became mundane. Not the purity of experience, but its pervasiveness and its being prior to cognition moved into the foreground. Instead of a realm of pure experience, there is already a habitus of the world, in which we "accept simply as what is presented as being".

This mundanization of transcendental analysis was indeed revolutionary since it upgraded the 'natural conception of the world' (*natürlicher Weltbegriff*) to an epistemic instance of first rank. With just this move, the young Heidegger became famous when he spoke of the "everydayness" (*Alltäglichkeit*) as a primordial layer of phenomenological (or existential) analysis. Thanks to Heidegger the life-world came to be known as the everyday world, the world as it appears "first of all and for the most part" (*zunächst und zumeist*) in our daily practices. In a similar fashion as Heidegger, Alfred Schütz later utilized the life-world as the everyday-world, and that of common sense for the social sciences (Steven Shapin, among others, quotes phenomenology as one of his sources on that point, 1994: xxv, 29). What Heidegger, Schütz, and many others have in common is that they thought of the life-world as an *original order*. The life-world reveals a primordial practical and epistemic *structure*. Heidegger, for example, spoke about the world as the 'nexus of meaning' (*Bedeutungszusammenhang*, 1962: 83 ff.), and Schütz as a 'relevance structure' (1982, Schütz and Luckmann 1980).

In the Preliminaries, I have introduced this original order as the hermeneutic notion of the life-world. It serves as the background for this first part. It is present in various hermeneutic aprioris of order (not only in Heidegger's *pre-understanding*, Gadamer's *pre-conceptions*, Schütz' *ideal types*, but also in Bourdieau's *habitus*, Kuhn's *paradigms*, down to Hacking's *styles of reasoning*, and perhaps even Foucault's *episteme*). Without denying their different emphases, life-world refers in these cases to a habitus on which we rely, to the obvious and unquestioned world that grants consistency to a domain or phase of life, to a historical period, to a discipline, to cultural spheres, to social milieus, and of course, to discourse. A characteristic feature of the hermeneutic notion of the life-world is a certain dialectic of what is obvious and what is problematic, of the habitus and the potential, of tradition and project, of author and reader, of the present and the absent, the concrete and the abstract, etc. This I call the *hermeneutic play of meaning*. In which hermeneutic play of meaning, I ask in this part, are economists enmeshed?

Going back to Husserl, the hermeneutic notion of the life-world cannot be more than a first heuristic step in a phenomenological analysis. Husserl, too, considered such hermeneutic idea of a pre-structured world, but he did not call it life-world. He called it "the world for everyone" (*Jedermanns Welt*), on the one hand, and the special world (*Sonderwelt*), on the other. We are living in different worlds, but nevertheless share one world. This constitutes what one might call the right to understanding, which is claimed by the naivety of a description. Life-world here is a title of *intelligibility*. In precisely this sense of a world in and of which everyone has an equal right to claim understanding, Husserl spoke of an "objective" world.

> The world, in natural attitude, is experienced with the sense of a world for everyone, which thus everyone can experience and reflect upon as the same world, and, by experiencing in such an approving way, can positively determine as the same: correlatively, it is in itself (Hua XXIX: 86*).

In this part, I will employ the hermeneutic notion of the life-world as a first heuristic step in a phenomenological critique of economic science. With its help, I can informally introduce economic science in its concrete appearance *for us*, that is, for you-and-me-and-our-fellows. To say that economics presupposes the life-world means here that, on the one hand, it "has" its own world, and, on the other, participates in the world-for-everyone. We can demand the right to understand economic science in the same terms as any other human practice. "Practices" indeed became the watchword for most commentary on science that probes beyond the philosophical issue of justification – which is "bracketed" and "falls under the epoche", as Husserl would have said.

The questions that I attempt to answer are thus: What is the relevance of economics, in the same terms, as we would speak of the relevance of the cobbler? How can the special world of economists be described so that it is intelligible within the world-for-everyone? On the basis of which intellectual interest can economists provide knowledge? Such an exercise helps me to introduce economic science without, at least tentatively, taking a position within economics – which falls under the epoche, too.

The special world of the economist, in a hermeneutic image, cannot be other than that of a discursive order. Economists participate in the world-for-everyone by means of *speaking up* in a particular fashion. Economists participate in economic talk as those who make epistemic claims. Economists' practices are determined by a particular way of relating to other economic discourses. The manner in which a speaker relates to his or her audience can be said to reflect one's *ethos*. Gaining an ethos is to acquire a special world within the world-for-everyone. In order to describe the special world of economists, I ask for their ethos as introduced above.

Gaining ethos in front of one's audience is not to gain ethos in front of another audience, as rhetoricians and hermeneutists know. Between the world-for-everyone and the special world of the economist, there are degrees of discursive *vicinity*. There is a continuum between audience and speaker in accordance with degrees of shared interests. The loosest relation economists have with those who are not directly addressed: the general public, or "laymen". Economists do now and then address "everyone". They have a *public ethos* (for a largely western public, I should add). Facing the layman, economists gain a public ethos as specialists on particular issues. How does this happen? Why do laymen believe in the economic specialist, and what makes them interested in following their talk? What is the economist as a "worldly man"? (1) A second, closer way of identifying economists is by their *professional ethos*. Husserl's special worlds and also Heidegger's being-in-the-world often referred to professional practices

(*Berufswelt*). A professional ethos demands the exclusion of skills and the inclusion of services, which form the pragmatic character economists' ethos. What do economists produce? What are they good for? (2). The third relationship concerns the most intimate audience: the descendants of economists who need to internalize economists' interests in order to themselves become economists. What is it like to become an economist? (3).

In this fashion I attempt to descriptively determine the discursive ethos of economists as they appear to you-and-me-and-our-fellows. What are economists, insofar as they are perceived as particular kind of people, as people who live in their own special world *in* the world-for-everyone? As Plato tried to nail down the Sophist in a particular class, the impossibility of which let the actual problem of the sophist emerge, I ask by what type of practices economists gain discursive identity. This part reads as an introduction to the problem of economics. It begins with the naive demand that economists should occupy a clear-cut place within the order of the life-world, and results in the critical lament that they cannot. The punch line is thus the indeterminacy of the economists' ethos. With that I will arrive at the complaints that is found in much of the commentary of economics: that economists are by far less relevant than you-and-me-and-our-fellows would expect.

For a first hint at where this ambiguity of the economists' ethos is located, think of the use of the word "economist" in everyday language. As compared with social characters such as the medical doctor, the engineer, the scholar, the artist, the politician, the lawyer, the worker, etc., there is no clear perception of the academic economist (Reay 2008). Just as there is confusion between "the economy" as a field of reality and "economics" as a discipline of research, there is confusion between academic economists and, loosely speaking, those-money-kind-of-people including businesspeople, bookkeepers, bankers, administrators, and other number crunchers and decision makers. They are all broadly perceived as economists. Against such a backdrop academic economists have to establish their discursive ethos. How?

(1) The Public Ethos

Economists, now and then, address us – people like you and me, the unwashed. They make statements and assertions, comment on changes and surprises, make predictions for years and decades to come, warn of risks, but mostly calm down worries, only occasionally cast blame, and are never too personal. They do so on various issues from governmental deficits to organ donations through various channels of public discourse, mainly via news channels, a range of magazines, and too many talk shows. What happens when people's faces are captioned "economist" – be it "Princeton economist", "Chief Economist General Motors", or "World Bank Economic Speaker"? By what means do economists appear to be worth listening to and moreover, credible? What kind of interest makes us listen to their claims? What is the *public ethos* of economists and how do they gain it?

Experts in Economic Talk – Aloof, but Exposed. Exposed to "Everyday Economics"?

To begin with, not everybody under whose name is written "economist" relies on actual epistemic authority. In public, almost anyone can make economic claims without clear grounds. Some do so because of the business they have done in new markets or in other economically remote areas, others because they lost a wad of other people's money on Wall Street. Among the various people who participate in public economic talk, academic economists do not have a strong profile. Only a handful of them are known. Thinking of the U.S., the *New York Times* columnist, Princeton economist, and now also Nobelist Paul Krugman may come to mind to some. Perhaps also the World Bank and Nobel economist Joseph Stiglitz, or the Harvard economist Robert Barro, who consults the public on nearly all economic issues. Other "economists" in public are actually businessmen, economic speakers of firms, of parties, of think tanks, etc. Some have are trained in economics, others not – and yet come off as more convincing. In a blurry field of economic talk, academic economists who claim scientific authority have to cultivate a public profile. How do they do that?

The most straightforward way of showing one's epistemic authority could be to avoid messy economic talk and gain status in the public science genre. But economists hardly appear in the world between *Science News* and *National Geographic* – though some economists do make their living in science parks. In public economic talk, there is in general not much discussion about science. Economists are not the makers of science. One merely relies on scientific authority when applying phrases such as "a recent Princeton study", or "latest research has shown…" and the like. What that means, what kind of methods are used, and on what grounds these claims are made, I have hardly seen discussed in public – at least not as often as in sciences like medicine, astronomy, or physics. The public is better informed about new methods and techniques in biomedicine than about recent innovations in computer-simulated market laboratories. Economic scientists do not grow in the safe cocoon of science parks, but are exposed to the "transgressive" character of its discursive environment, 'threatened by its overflows', as Amariglio and Ruccio have put this basic hermeneutic situation of economists (2003: 280). Science talk is a different genre than economic talk.

Instead, economists appear in the politics section – more on a national than a local level, but even more at home on a global scale. There they have to use their epistemic authority as specialists, experts, and advisors. Economists participate in politics by either speaking up themselves or informing journalists, politicians, and other decision makers. This is their service to the public. As a *New York Times* editor praises this service to the journalist:

> Journalists have ready access to smart, fast-talking, and politically savvy economic analysts on nearly every issue. Dozens of high-quality think tanks and policy groups, liberal and conservative, produce mountains of readable reports that reach decisive policy conclusions with firm evidence, but little distracting details (Weinstein 1992: 73).

As advisors, economists take political positions. Yet they are distanced and remote from politicians. When economists claim epistemic authority, they claim to know better than politicians, but do not achieve the status of doctors and engineers. Economists do not rule political talk like doctors rule the operating room. Economists know better than day-to-day politicians, yet they do not make politics – aloof, but exposed.

Being experts in economic talk, what identifies their field of expertise? Economists, as the namesake implies, are specialists on "the economy". What is "the economy" in public? A bunch of catchwords come to mind: unemployment, growth, poverty, governmental spending and deficit, development, financial markets, etc. Klamer has identified more systematically four typical sub-genres of economic talk (2007: 162 f.). First, a Keynesian perspective that focuses on government spending and taxation; second, a Wall Street perspective that focuses on supply-side entrepreneurial activities of CEOs and their fellows; third, a monetarist perspective of financial markets including the magic art of Greenspans; and last, the Aristotelian perspective, which is perhaps the most natural and most ancient style of economic talk, modernized by Marx and handed over down to unionists such as José Bové: moralizing.

Donald Lamm, the head of the publishing house W.W. Norton & Company, uses roughly the same categories when splitting up the sub-genres of economic books published for sale (1989: 97). In the books market, economics competes with other nonfiction books such as Sociology and "Customs, Etiquette, and Folklore". Each sub-genre in economics has its bestsellers. One classic of the Keynesian perspective is surely Galbraith's work in the 1970s (such as *Economics and the Public Purpose*, 1973). But the business literature of successful CEOs holds hitherto the greatest market share in economic literature. It ranges from General Electric CEO Jack Welch (*Jack*, 2001) to Koch's popular adaptation of Pareto (the *80/20 Principle*, 1999). Financial market literature is big, too – be it Greenspan's diagnosis in *The Age of*

Turbulence (2007) or George Soros' revelations in *The Credit Crisis of 2008 and What it Means* (2008). The moral genre is the most secure option, for there seems to be an infinite demand for books such as John Perkin's *Confessions of an Economic Hit Man* (2005), a firsthand exploitation report; or more moderately, Stiglitz's *Globalization and its Discontent* (2003) – let alone all the Black Books of Capitalism that flood the shelves of bookshops. If moralizing describes one basic tone of these various genres of economic literature, the other side is clearly *instruction*. Grabbing one of these books from the shelf, the general reader does not want to know, but wants to be told what to do or shown who is to be blamed. Such are *not* epistemic concerns.

When classifying economic talk in the way Klamer and Lamm do, the risk is therefore to foist theory on it. In the little commentary that exists about public economic talk in relation to the academic conversation, such is hardly acknowledged. There it is common to speak about "everyday economics", and even more problematic, about "ersatz economics", as McCloskey has coined the term with pejorative undertones (see Amariglio and Ruccio 2004: 252 ff., McCloskey 1999). These terms suggest that people do hold, even if implicitly, actual economic theories. "No one", Klamer states grandly, "can live without beliefs about the economy" (2007: 160). If that is so, one could, for example, embark on a study of *The Child's Construction of Economics*, as Berti and Bombi did (1988). They asked children about their theory of prize determination, income differentials, interest rates, etc., and showed where they persist to recognize contradictions. Children "argued", for example, that workers earn little because the plastic bags they produce are so cheap (148). Do such replies reveal implicit economic theory? Are there two types of theories, academic and non-academic? (Klamer 1991)

When approaching economic talk in this way, the argument usually results in the acknowledgement of the incommensurability and irreducibility of everyday and academic economics because of the so-called theory-ladenness of economic reality. This approach aims thus at relativizing economic knowledge, and at acknowledging or blurring the boundaries between academic and public economic talk. Ruccio and Amariglio, for example, want to "challenge the terms in and through which academic economists have sought to subvert the discursive standing of everyday economics" (2003: 281). Though I am sympathetic to their, say, democratic intention, they do not question how the boundary between academic and non-academic comes about in the first place. The emphasis on discursive differences may lead to more tolerance, yes. But it also risks reinforcing the notion that academic economics can only be understood from within, rather than from the world of you-and-me-and-our-fellows.

Prior to acknowledging "everyday economics", we need to ask what a *theoretical interest* in economic talk can be. Phenomenologically speaking, the first thing to acknowledge is not that there is a plurality of theories, but that in economic talk a theoretical interest first needs to be accomplished. It is all but obvious that the explicit theory of the economist and the so-called implicit theory of the layman are related. Just that link, to the contrary, is most problematic when considering the economists' place in the world for everyone. It is not economic talk as seen from the point of view of economics that needs to be understood, but rather how economists gain discursive authority on the basis of a theoretical interest in the first place. For this reason the question of the public ethos of economists is how economists are perceived by you-and-me-and-our-fellows. I do not attempt a reform of discursive boundaries. I ask how they come about. By which means can a theoretical interest in economic talk be established? And at what cost?

Why the Layman Watching the News Knows he is a Layman: Complexity

If there is a general concern of economists by which they are identified in public, *economic performance* certainly ranks high. "The economy" in public does well or poorly – which is one, some say *the* corner stone of the logic of contemporary political discourse. Is there any law that is *not* made in support of or despite its effects on economic performance? Economists are the judges of economic performance, but not its guards. Economists are not politicians. They deliver research reports with which politicians can decide and act. Economists, as all scientists, need to adopt a distancing attitude to "the economy" to such an extent that the stuff of economic policy appears like facts. Only for facts are specialists needed. How then is a perception of the facts of economic performance motivated?

Economic performance is not trivial. Laymen have no natural sense for it. Before one can judge how it goes with "the economy", one has to count. The material the economist deals with are *measures* of economic performance: growth rates, unemployment rates, inflation rates, stock-indices, consumer prizes, and, of course, the GDP. To assess the overall performance, to predict future performance, and to advise political decision makers of society about possible strategies are all supposed to fall under the economists' field of expertise. The typical questions economists answer are, for example, whether stagnation in the EU is due to the U.S. economy, the global economy, or a lack of domestic innovation; whether the rise of interest rate has indeed affected the governmental deficit as the opposition says; why the Enron crash or the 2008 financial crisis did not after all affect the "real" economy; whether the NAFTA agreement increased poverty in Mexico; or whether landowners are really better off if one demands tariffs on the imported corn from Poland, which was hotly debated throughout the first decades of the 19th century in the British parliament. There are also less typical cases in which the public has become accustomed to consulting economists, such as in the debate over whether one should "invest in social capital" or allow for a "market of organs". Economists take epistemic positions *in* politics. From such knowledge laymen who do not consider themselves economists are excluded. They accept the authority of economists to some extent. Why?

Specialists are needed for things one has to examine very closely with eyes that are trained for a long time. The measures of economic performance are not immediately comprehensible, intuitable, or visualizable. As we cannot see the forest for all the trees, we cannot see whether "the economy" goes good or bad. You-and-me-and-our-fellows are too busy with all the things that have to be done, bought, and sold day by day. Who knows how much is an annual GDP of 12.623.113.000.000 dollars of the European Union in 2005 or defense expenses of 320.000.00.000 dollars of the U.S. in 2006? And who knows why one debates and studies for months whether economic growth will be 1.6% or 1.7%? Isn't this below any significance? Whether the layman and his fellows had a good or a bad year, lost their job or added little to their wealth the bank handles without their knowledge, does not help understanding that difference of 0.1%. Just as the German minister of finance once explained the governmental deficit by comparing the height of bank notes with the height of the highest dome, the public knows that economic measures can just mean everything. Climbing up the dome, how could it help in understanding the governmental deficit?

Only the economist can say something sensible about the meaning of measures of economic performance. For economic performance is not a simple matter, but *complex*: the meaning of one measurement depends on that of all the others. Whether 1.6% or 12.623.113.000.000 dollars is big or small depends on all the other measures – non-intuitable things multiplied by non-intuitable things. What seems minor on one side of the globe can have severe effects on the other. In "the economy", *everything is connected with everything* – dizzying. Who else could be the authority of meaning if not those who are able not to lose, in Heidegger's word, the "circumspection" in all these inter-dependencies and trade-offs? Is it not fascinating and appealing to know how All That hinges together? What lets economists appear as specialists and worth listening to, is that their knowledge seems to cope with *complexity*.

Economists like to refer to such complexity when they start to settle the world of their specialty – not only since the academic complexity-hype of the last decade. "[T]he economic system is a whole all parts of which hold together and react upon one another," Cournot made a principle of his economics of 1838 (146). Later, Kenneth Arrow, talking to a general public in his Nobel Prize lecture, illustrates his field of expertise with the consequences of oil discoveries – liquid global power:

> The price of oil became very low in the 1930's because of discoveries in Texas and the Persian Gulf area. Homeowners shifted in great numbers from coal to oil for home heating, thereby decreasing the demand from coal and employment in the coalmines. Refineries expanded, so more workers were employed there. There was as well a demand for refinery equipment, a complicated example of chemical processes. This in turn induced demands for skilled chemical engineers and for more steel. Gasoline was cheaper, so that more automobiles were bought and used. Tourists areas accessible by road but not by railroad began to flourish, while railroads (…) (Arrow 1972: 51).

(…) ad infinitum. "The economy" is a *complex system*, held together by everything and nothing, circling on its own.

Such is the dominant manner by which economists gain epistemic authority. Only economists can make sense of the composition of markets, the logic of which goes beyond mere 'supply up, price down'; only economists know how market forces subtly slide into the numeric world of "the economy" that fills the journals and news; only economists can come up with an almost miraculous account of the world like, 'because of oil discoveries in the Persian Golf, your favorite bakery around the rail station closed down'. It is this dizzying impression that is necessary for economists to claim an ethos as a specialist. They need to evoke the wooziness of "the economy" again and again in order to claim authority as specialists and keep us listening. If the public acknowledges "the economy" to be complex, the economist is granted a free ticket for epistemic authority and intellectual unintelligibility, as well as years of research in order to go through "all that".

Only insofar as the world appears as a complex system in which whatsoever is connected with whatsoever, economists can raise their voices with epistemic authority. Because the market connects worlds that have nothing to do with each other, people who never have heard of each other, because we are alien and not familiar with each other, because "the economy" goes beyond the *oikos* – that is, because "the economy" is beyond all negotiations of meaning that constitute it: the economist can strike roots.

Why the Layman Nonetheless Does not Accept Being a Layman: Anonymity

For just the same reason of complexity, however, the credibility the public grants to economists is limited. As easily as laymen may accept economists' authority in the abstract, so hard is it to hold on the belief in the concrete when it comes to their own participation in *the economy* – the world of economic life. Then, most, like you-and-me-and-our-fellows, are reluctant in following economists' truths. Only a few dash to their bank after economists claim that the interest rate may fall – perhaps those who think about the "structure of their portfolio", about the difference of 1.6% and 1.7%, thus these-money-kind-of-people, but certainly not you-and-me-and-our-fellows. Who feels addressed when listening to statements like 'the clammed U.S. consumer temperament of the last months caused panic sells at the European stock markets', even if the bag we carried home from the grocery was slightly lighter than usual? Do we start to panic? Is it a reason to even shrug shoulders?

For most of us, I suppose, not. The attitude we adopt when listening to economists is not one that could move us to act. The curiosity that makes us follow economists' talk – for some it may rather be out of boredom – does not grow into an actual practical interest. The relation of economic knowledge that economists claim, and economic behavior that you-and-me-and-our-fellows reveal is not trivial. As important as "the economy" seems as long as we are kept under the impression of a woozy "all that", as irrelevant it is for everything we deal with from morning to evening. The interest rate we receive from the bank is simply not the same interest rate as the interest rate economists talk about. We may believe they are the same, but we may not know what that means. After we switch channels, turn pages to the culture section, but at the latest when we take up to work again, there is hardly anything that reminds us to what economists have said.

Laymen, or, as some economists have called their epistemic counterpart, "practical man" do not live in a complex world. They live in a world that passes for better or for worse from morning to evening. Here there is no "economy", but there are people conducting an *economic life*. In this life, the next morning never waits too long to call for work; we pinch pennies at the end of the month, but not at all on holidays; we go once or twice a week shopping, getting nothing but in one another's way; children turn out more expensive than expected, though we do not regret; we can feel safe or cannot stop worrying about the future and the assets hoarded in the last decades… . Conducting an economic life – only vaguely confined, sometimes too intrusive, but after all never pervading all the rest of life – there is nothing that connects our practical interest with the casual interest in listening to economists. If we want to know how "the economy" goes for us, we are negligent in granting too much weight to what economists say. "The economy", for most parts of our life, has nothing to do with *the economy*.

In their empirical study of the general public's perception of "the economy", Robert Blendon and his team at Harvard, for example, arrived at the following result:

> Asked to choose from a list of nine possible sources, the two indicators they [the U.S. public asked by telephone] think give them the best indication of how the economy is doing, only 32 percent of the public mentions news reports on government unemployment and cost of living statistics. Nearly as many Americans (28 percent) cite as a key indicator the amount of buying activities they see in the stores. More than half (55 percent) rely on the personal experiences of family, friends and coworkers (Blendon et al. 1997: 115)

If I deny the accuracy of such lay judgments, why is it not the same as denying the accuracy of a patient's judgment who takes how he or she feels as an indicator of the success of the doctor?

The answer is clear: It is not the people who *are* "the economy". The forces economists speak of do not touch us in person. They are *anonymous*. Market forces are exerted by everybody and nobody. Economists do not speak about this or that concrete person, about you or me or our fellows, but about "consumers", "investors", "producers", "entrepreneurs", and a handful of other characters that are defined by the relations they hold to each other. For the economist, not the people living *the economy*, but the relations of these people make "the economy". These relationships are not those among associates – mother and daughter, master and slave, boys and girls. Instead, in a complex system relationships are relations with features on their own – which is perhaps *the* abstractive move of all modern science. "The economy" is a structure, a system, a complex, but not a world we possibly could live. Economists can only claim authority when speaking about anonymous forces *structurally induced*.

The public ethos of economists may be that of specialists. But they are specialists of a not very engaging sort. If academic economists are urged to translate their findings about "the economy" in terms of economic life, they often appear trivial if not easily ridiculed.

> A highly embarrassing moment usually follows a new Nobel Prize-winning economist being asked, "What for?" I remember Franco Modigliani on the morning news (…) When asked what he had invented, he said something like: "Well ... uhm ... I had this idea that people save for the future." James Tobin told the reporter of public radio that *his* great idea was that people, when investing their money, "do not put all their eggs in one basket." You could hear the reporter's gasp. "Is that it?" "Yes, more or less, that's it," Tobin responded. When James Buchanan explained that he got the prize for the insight that politicians pursue their own interests just like everyone else, Mike Royko, a columnist, claimed half the prize because it had been his insight, too (Klamer 2007: 158).

Certainly, laymen do not know the complex implications such trivialities can have for economic theory, as Klamer continues excusing this embarrassment. However, if complexity needs to be evoked in order to speak of facts of "the economy", and if this requires anonymous relations between agents, it is not only not surprising, but *necessary* that the economists' bonds with the rest of economic talk are weak. Complexity allows for the appearance of the economist, but at the same time makes it difficult for the economist to address the people in their personal situation. One accepts economist' authority on the one hand, while everyone is still allowed to hold his or her private opinion on what is really going on the other. Precisely as the economist is able to associate with "the economy" worlds that have nothing to do with each other, you-and-me-and-our-fellows are able to associate with *the economy* a world that has nothing to do with the economists' world.

Economic talk beyond the control of epistemic authorities follows different rules. When laymen consider matters of how "everything is connected with everything" as personal matter economic science is hardly of interest, let alone of authority. The image of complexity economists evoke justifies a belief in science only regarding those things in life that do not move us from our chair. Laymen reply to the wooziness of complexity with a very different, personalized account. If everything is connected with everything, everybody can be blamed for everything – in particular the disliked people. Again the example of oil prices:

> We also asked about gasoline prices in our survey, at a time when they were rising. Nearly three-fourths of the public believed that the increase in gasoline prices was due more to the oil companies trying to increase profits than to supply and demand, while 85 percent of economists said the price increase was due to supply and demand (Blendon et al. 1997: 116).

What is apparent to Arrow and his fellows – that oil discoveries let gasoline prices fall – the public successfully ignores. Klamer and Meehan, too, have put much emphasis on the gap between the economists' and the public perception of "the economy":

> Everyday economists are most likely to personalize the economy; they think in terms of people doing things, of right and wrong, of victories and defeats, of special interests, and of identities. Whereas academicians choose to think about the economy as a (general equilibrium) system of markets, everyday economists prefer to think in anthropomorphic or biological terms. That is to say, the economy is populated by people who are emotionally driven, and functions like an organism as a whole with functionally interdependent parts. The academic narrative is minimal whereas the everyday economists prefer to dramatize the economy, endowing it with villains (big corporations, unions, foreigners, or presidents of the other part) and heroes (entrepreneurs, small business, unions, presidents of one's own party) (Klamer and Meehan 1999: 69 f.).

In economic talk there is only one question that moves the mind: *Who*? And: *What kind of person is that*? Partaking in *the economy* always bears names – the names of these idlers who make a living from evading taxes, the names of these wannabes who drive expensive cars, the names of these greedy CEO's of the low-cost airline that controls employment in our region, and the names of these politicians who pretend to represent values while seeking economic power. When laymen come to consider their own partaking in *the economy*, the knowledge of the economist serves at most as a *symbolic* factor in their attempts to make sense of their economic situation. Economists' claims can be a mere vindication of our bad luck and failures. What unpleasant situation or unfortunate course of actions cannot be chalked up to "the economy"?

I repeat, the question here is not to upgrade the personal attitude of "everyday economics" as an irreducible modality of knowledge, but to question the conditions by which this boundary can be established. The gap between the academic and the everyday perception of "the economy" become unbridgeable if they do not 'see the same with different eyes', but the latter sees through its own eyes, and the former through nobody's eyes. The difference between the anonymity of relations and the personality of agents is not a theoretical difference. It is prior to a theoretical interest. If the anonymity of relations is a condition of the very difference of *episteme* and *doxa*, economists as such cannot relate to actual persons.

The Culture of Economic Suspicion: Some Instances

Laymen never fully internalize that the forces that rule *the economy* are anonymous. Behind these forces there are, or better, there *must* be some fellows who choose among "systems" – preferably politicians, but also other liked and disliked groups of people. Economic talk today, as centuries and millennia before, prior to any theoretical interest, is ruled by *economic suspicion*. There are present governments speaking of freedom while thinking of oil, there are 17th century traders speaking of honor while ruining the treasure of the British Kingdom, and there are those who gained power at the expense of those excluded from *the economy*, as it happened for millennia in Europe to the Jews. The economic suspicion says, among other things, that behind all complex systems, the motives of those who profit persist. To say that everyday economics is more "declarative" understates the indiscreetness of this suspicion (Amariglio and Ruccio 2003: 268). The economic suspicion is the great force to see persons where others claim necessities, to see intentions where other claim causes. *There are* global players, those who meet invisibly but nevertheless *really*, *annually*, *there* in snowy St. Davos, people making GATTs, and NAFTAs, sitting in WTOs and World Banks. And this suspicion can befall the economist: For there also must be some people who do profit from the *belief* in anonymous structures!

Economic talk, in all its genres, moves within the discursive cultures of economic suspicion. Marxists have always done so when blaming capitalists and other potential exploiters. Sociologists do so when speaking of class, groups, and roles – the science of what-kind-of-people. In business literature everybody does so anyway – is it not the job of businesspeople to know what kind of people they deal with? All anti-globalizers do it anyway – it's Bush fault, isn't it? Authors of sale books on the ultimate capitalist plots do so – or was it Bill Gates? Other emphatic apologies and cruel historical score-settings of capitalism do so – perhaps it all started with Roosevelt's New Deal? Religious economics on bearing or abolishing poverty do so, too – from Jesus to Therese, were they not economic heroes? Esoteric economics on the latest ego-technologies that show how to liberate yourself from the spirit of mammon contribute to the culture of economic suspicion in their way. And so on. Beyond the control of epistemic authorities, there is a considerable amount of economic discourses speaking against the anonymity of *the economy*.

Such are the sources, easily accessible and comprehensible, that help people understand themselves in their present-day economic life. They provide instructive advice with one hand and moral support with the other. If any academic discourse informs them, it is hardly economics proper, but other disciplines such as political science, anthropology, psychology, sociology, and philosophy. If there is any reference to economic science, then it is at most to the invisible hand – mentioned either enthusiastically with Smithean connotations of liberty or pejoratively with Marxian associations of selling exploitation as just that.

Economic suspicion rules economic talk. The study by Klamer and Meehan on the political process that, at the beginning of the 1990s, led to the NAFTA vote, illustrates a case of this suspicion. At the beginning of the debate, the U.S. International Trade Committee considered extensive academic research about the effects of NAFTA – mostly computable general equilibrium models. When the decision came closer, however, it turned out to be a matter of special interests, national pride, and personal credibility rather than a matter of Truth.

The pro-NAFTA turn in public opinion took place when Vice-President Al Gore and NAFTA opponent Ross Perot debated on television.

> In front of a national television audience Al Gore and Ross Perot personified the two sides; they became the issue themselves. The debate became a contest in character. [Al Gore] challenged Perot by pointing out contradictions in earlier made statements and interest of the Perot family in the continuation of trade barriers between Mexico and the United States. While Perot tried to make an emotional plea against the NAFTA treaty, alluding to losses in the United States and likely exploitations of unprotected Mexicans by US corporations, Gore tried to deflate the emotional plea by calling the character of the spokesman himself into doubt. (…) The public response to the debate suggested that Gore had been successful in making the opposition suspect, by making the individual who personified that position, suspect (Klamer and Meehan 1999: 77f).

The influence of academic economists on the passing of NAFTA, too, happened by virtue of their character. 300 economists, including all American Nobelists, wrote a letter in support of NAFTA that was continuously quoted by the pro-NAFTA camp – whatever the Nobelists actual arguments.

The study of Blendon makes another case for the culture of economic suspicion. 69% of the public, as compared to 12% of economists, believes that "the economy" is not doing better than it is because top executives are paid too much. You can see by the numbers the poise with which the public says, "Yes, because of *these-kind-of-people*!" And you equally see the affects in the economists' reply: "No, it's more complex!" Neither wants to appear naïve.

Views of Reasons Why the Economy Is Not Doing Better Than It Is

	General Public	Views of Economists
The federal deficit is too big	77%	32%
Too many people are on welfare	70%	11%
Foreign aid spending is too high	66%	1%
Taxes are too high	61%	18%
People place too little value on hard work	59%	18%
Too many tax breaks for business	48%	5%

Source: Washington Post/Kaiser/Harvard (1996), quoted in Blendon et al. 1997: 113

For another instance of the culture of suspicion, consider how the assessment of "the economy" functions in the political discourse. Although one grants economists their specialty in making such judgments, to believe those who claim "the economy" is doing well, does it not amount to the same as saying 'I am so naïve as to believe what the government says before the elections'? Saying that "the economy" performs poorly does it not amount to the same as saying that one is politically critical-minded? Why should we even conceive of ourselves as political beings if we believed "the economy" was doing just fine?

The economic suspicion does not spare the economist. Appearing remote, trivial, or boring at first, if the economist actually makes a claim, he or she is subjected to suspicion just as anyone else. Although economists can come up with an account that handles the utmost complexity, there is also the common knowledge that they, in principle, could have come up with an alternative account resulting in the opposite claim. Although there is some acknowledgement of economists being scientists and well seated at Princeton, Harvard, and

Chicago, there is also the widespread belief that this science is as flexible as the specter of political interests at Princeton, Harvard, *or* Chicago. Even our grandmothers may have heard of President Truman's saying about the lack of one-handed economists. Accordingly, when hearing one-handed claims – such as 'Princeton economists have shown that a minimum wage has little to no effect on employment', or 'the economic council assured that last year's unemployment was caused by the lacking reforms of the tax system' – being wary is appropriate. Who are You, Dear Economist, Arguing This!

And so, the *New York Times* editor's praise of economists in the media quoted above has its pessimistic backside:

> Will they (economists) fuel the cynical presumption, deeply embedded within journalists, that expert testimony reflects little more than the self-interest of a client? (…) Where once scholars served as reliable authorities, they now serve as advocates (…) Rather than to clarify public debates, economists are too often trapped by it (Weinstein 1992: 77).

Thus, if one has a practical interest in a particular claim, there will always be an economist with the suitable political inclination to support it. If everything is connected with everything else, everything can explain everything else – in particular what suits my interest!

Under this rule of public economic talk, economists' judgments on "the economy" are easily undermined. Measures of economic performance, so the rule has gone for centuries, are expressions of special interests. This could be called the *methodological Ur-suspicion* of modern economic talk.

> Asked to judge government reports on how well the national economy is doing, including statistics on the rates of unemployment and inflation, 26 percent say they think these reports are not too accurate, and an additional 13 percent say they are not accurate at all. (Fifty-three percent said they thought these reports were fairly accurate, while only 7 percent said they were very accurate.) (Blendon et al. 1997: 116)

Roughly fifty-fifty – enough to govern. Choose your party, and you know how "the economy" performs. This methodological Ur-suspicion is both the great engine and, at the same time, the main stumbling block of the scientification of economics.

Within the culture of economic suspicion, the woozy perception of everything-is-connected-with-everything can play out in a different way than epistemic authority. Compare, for example, Arrow's illustration of the market system quoted above with the following description of a cultural critique. Wondering who could be blamed for the misery of farmers in Asia, he travels through the market's complex system, asking everyone he finds: Who should be blamed?

> My trip led me first to the caoutchouc plantations of Southern Thailand. The small planters referred to the Chinese intermediaries as the reason for their miserable destiny. The Chinese traders, instead, would explain later that it's not their fault. It's the fault of the multinationals that push prices down. The agricultural minister of Thailand could confirm that. He himself was powerless. So I drove to Akron, the seat of Goodyear, in order to ask the president, a pleasant Egypt called Sam Gibara. He emphasized that the "market" is responsible for everything. 'Effectively', he added, 'we have to say that the annuity funds became the main actors in the market' (Toledo 2005: 51*).

The layman is thus able to come up with an equally miraculous claim that we all – only thanks to holding a bank account and thus supplying annuity funds – can be blamed for the misery of the caoutchouc farmers in Southern Thailand. If everything is connected with everything, then everyone is responsible for everything – in particular for all the misery of the world. Terrifying.

Everyone – so why not turning the moral discourse on its head? Mirowski illustrates with the following fictive reply to a beggar asking for change.

> Look, my man, if I give you a dollar, your income will go up, so average spending will rise without any offsetting rise in production. That will push inflation up, devaluing our currency after worsening our trade deficit, not to mention shifting the tax burden into the more productive sectors of society. The dollar becomes worthless, and more people are thrown out of work. So I'd like to help ya, guy, but don't you think things are bad enough already? (2004: 379).

Such one may call the moral end of the theoretical abstraction of "the economy" in which everything is connected with everything.

As a last, peculiar, but highly virulent example of the culture of economic suspicion, consider a remark by Hans Sinn, head of the Institute for Economic Research (IFO), on the recent debate about the 2008 financial crisis. He warned the public not to blame the managers, since in the crash of 1929 Jews were blamed in the same way. The public's clamor was great, obviously. Sinn touched on the respectable German taboo of comparing anti-Semitism (read: Holocaust) with any other event in history. Historically, however, there could be no more truth than that: suspicion–violence–liberation by anonymity forms the key-chain for understanding the epistemic culture in Western economic talk.

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The public ethos of economists, to sum up, is double: On the one hand, in order to claim scientific authority, "the economy" has to appear as a *complex system*. Only by a complex system can the economist establish systematic knowledge. Yet, for the same reason, economists have difficulty addressing the general public, since market forces in complex systems are presented as *anonymous*. Moreover, just because of this anonymity, if there is a political claim made with the authority of science, the economic suspicion rules; other than anonymous forces must lead to such claim. Measuring "the economy" does not help since numbers are the first objects of will, as you-and-me-and-our-fellows know better than governments and their economic advisors before elections. Only when kept under the dizzying impression of a non-intuitable complex system is the economists' knowledge suggestively important. Only a moment later, when facing our concrete world, it is of no interest whatsoever.

The perception of "the economy" is decisive for this gap of interests. The perception that "everything is connected with everything" can institute a theoretical interest to come to grips with "all those" interwoven facts. Regarding such interest, we grant economists' knowledge specialty, unintelligibility, and, moreover, formality. Yet the same perception can also correlate with an excuse for one's bad luck, a reason to blame whomever one wants, or the burden of the guilt of the entire world. The perception of "the economy" can reflect a theoretical notion of a complex anonymous system, but it can also reflect the entire moral spectrum of our economic life. These two attitudes draw a dividing line in economic talk: here we are concerned, there we want to know. The former attitude we adopt when it comes to our own participation, the latter when it comes to everybody's and nobody's participation.

This problem of economists' public ethos runs deep. If I am right that the perception of a complex system is *constitutive* of, rather than a flaw in economists' ethos, we cannot avoid the skeptical question: Is it *a priori* impossible for economists' knowledge to be relevant for anyone else? Is it *a priori* impossible that economists' knowledge explicates or intervenes critically into the public's perception of economic life? How can economists be influential at all if the very condition of their discourse is their inability to address anyone in particular? Could we still find people claiming epistemic authority in economic talk if economists became more personal? Thus, the least I can say is that the problem of the relation of the economist to public economic talk is not only a matter of discursive incommensurability, of the lack of pluralism in the epistemic business, which leaves the inherent solidity of economics intact. But the public ethos becomes a problem *for economists themselves* since there is no apparent reason why one should engage in supporting epistemic culture of economic talk?

From this point of view it is questionable whether the problem of economic science, as Blendon et al. conclude, is "that economists need to do a better job educating the public about economic matters" (1997: 117). Of course the general public does not "know" much about "the economy" – only a third of American adults knows that the Federal Reserve sets monetary policy (Ibid.: 116). But will people believe more in "demand and supply" if they knew? What does public economic education accomplish? How can the *National Council of Economic Education*, for example, seriously promote the belief in science by lamenting that the lack of profound knowledge of economics makes people "more likely to have money problems, career problems, and credit problems, and less likely to make informed decisions as citizens and voters" (quoted in Ruccio, Amariglio 2003: 264). Public economic education serves different purposes, as I suggested in this chapter, and as a closer look at the *Test of Economic Literacy* confirms. One question of this test is the following:

> In a market economy, the social purpose of profits is to (a) get business to follow government regulations; (b) get business to provide what consumers demand; (c) provide funds to pay workers better wages; (d) transfer income from the poor to the rich (in Nelson, Sheffrin 1991: 159).

Answer (b) counts as most "literate". But does such literacy help reduce "money problems"?

Did I not present sufficient reason to doubt that the different perceptions of "the economy" are a mere matter of education, of knowledge and ignorance, of *doxa* and *episteme*? Perhaps this gap is, to the contrary, the condition of the very identity of economists' as specialists? Perhaps the difference between doxa and episteme comes about simply by ignoring economic talk?

The theoretical perception of "the economy" represents the gulf between economic science and the rest of economic talk. Large parts of the following phenomenological remarks about economic science will trace this theoretical perception of into various facets: its historical genesis, its manifestations in economic theory, and its existential effects on the character of economists.

In the philosophical commentary on economic science, the gap between public and academic discourse is discussed and partially celebrated as the distinct achievement of economics using invisible-hand-explanations. Reference goes usually to Hayek's formula of 'the results of human action but not of human design'. Invisible hand explanations, a classic commentator said, need to be "non-natural", "counter-intuitive" and "implausible" (Ullmann-Margalit 1978). If science says what the rest of the people anyway say, she argues, it is not worth having a science. Complexity as a condition of such explanations is granted, too. "Only when the social pattern or institution to be explained has a structure beyond a certain degree of complexity the invisible hand explanation of it has a point" (Ibid: 267). She furthermore speaks of a "constructive character of invisible hand explanations", while non-economists reveal an "artificer bias" (268). The value of invisible hand explanation, she argues in line with Hayek, lies in the *surprise* that there is actually nobody who is responsible for market allocations. Is it, however, perhaps the other way around that the conspiratorial thinking (artificer bias) is even reinforced through the abstinence of economists from the responsibility question? What then are the costs of using invisible hand explanations? And what are the costs to defend them?

But first let me ask: Who is the economist? Since in public there is hardly any reflection on the scientific grounds of epistemic claims, the identity of economists is rendered rather vague – hidden somewhere behind politicians, without great entries in the science section. This fuzzy image does not lead us to the actual practice of claiming epistemic authority. Let me thus move closer to economists' actual institutions and social identity by asking: What kind of profession is it? How do economists gain *professional ethos*?

(2) The Professional Ethos

The 'who' question is the preeminent concern of most economic discourses. It was, to anticipate a historical remark I will come back to in the next part, also the exclusive concern of European pre-modern economic writings before there was economic science. The who-question was posed mainly as that of *professions*, around which economic institutions have been organized. As Plato would have been content if he were able to determine the exact profession of the sophist, days and years in the middle ages passed smoothly as long as the order of the professional guilds gave meaning to all stages of economic life. The great challenge of these pre-modern writings was the classification of particular persons: traders. What kind of profession is trade? What do they produce? What do they contribute to society? The answer was far from trivial. Traders only bring things from here to there but do not bring things about. And the usurer, the money-trader, does not even do that! So how could they survive? Most economic writings before economic science concluded they must be thieves!

Did I not make a similar charge in the previous chapter? If economists have difficulty addressing the general public, what does the economist actually accomplish? If economists do not address or relate to anyone, but only to the relations we entertain in "the economy", then what are economists good for? What is it to be a professional economist? What do economists produce? What do they contribute to society? Stigler explains:

> A world full of mistakes, and capable of producing new mistakes quite as rapidly as the economists can correct the old mistakes! Such well-meaning, incompetent societies need their economic efficiency experts, and we are their self-chosen saviors (Stigler 1982: 8).

The saviors of incompetent societies? Many objected – among others, the young Colander:

> [S]ay that all garbagemen got together and went on strike. What would the effect on society be? The Answer is clear: Society would be a mess. Now say that all economists got together and went on strike. What would the effect on society be? Most people's answer would be, 'None. Things would be just about the same with or without economists.' Hence the question: Why aren't economists as important as garbagemen (Colander 1991: 19).

To pose the question of professional service in such a blunt way is somewhat forced. Academic life is usually granted freedom from the need for an "immediate" outcome for society. Academia is worth having because it provides criticism of society at large, because the conduct of an intellectual life is regarded as something virtuous, or because it is part and parcel of our cultural heritage. Academic practices, then, are not associated with proficiency, but with scholarship. If economists make a living from scholarship, there should be no reason to complain that they are not like doctors, lawyers, engineers, or managers. But why do we expect economists to be like them anyway?

Be it a professional or scholarly community, I can ask how the discourse of economists plays out in society at large, how their worth is justified for those paying their salaries, and through which channels their efforts are utilized. Economists, I attempt to show in this chapter, are torn between the two. Neither did they accomplish a professional ethos by providing a distinct service to society, nor did they accomplish recognition as scholars beyond their immediate usefulness. Pragmatic justification is expected from economists, but at the same time threatens their identity. Economists are not like engineers, doctors, or lawyers, nor like philosophers – even worldly ones.

What is interesting when speaking of professions is that a profession requires a balancing act between *social exclusion* and *inclusion* – a standard distinction in the sociology of professions (see Coats 1993: 395 ff.). In order to appear as professional, one needs to adopt a sovereign yet sensitive voice. A professional ethos, on the one hand, requires recognition and esteem from you-and-me-and-our-fellows. The product needs to be included in the world-for-everyone. On the other hand, a profession has to exclude others from their activity in order to claim authority, authorship, expertise, and responsibility on its products. A profession thus needs closure, needs to exclude others from its special world. These two conditions make professions the instance *par excellence* of the hermeneutic notion of the life-world, between the world-for-everyone and special worlds. How do economists manage this double requirement?

Note on the literature

Approaching economics as a profession is an established part of today's commentary of economics. For an inspiring, though Austrian-biased, volume of how eminent economists themselves think about the services they offer to society, including Hayek, Kirzner, Coase, and Tullock, see Klein (1999). Stigler's essay on "Do economists matter?" needs to be added to that volume (in Stigler 1982). The self-understanding of economists as professionals of less known (academic and non-academic) economists is reported in Raey 2008. Institutional data on the profession are gathered and interpreted favourably by Siegfried (e.g. 1998, 1999). For an early survey of the field in the 1960s under the auspices of the *Nationai Academy of Science*, see Ruggles 1970. Colander's work has disclosed much of present-day attention to the profession (Colander and Coats 1989, for an early survey Colander 1989). Standard topics are the ranking systems of journals, universities, and economists, analysis of bibliographic data, etc. The growing field of economics of science addresses issues such as the organization of science (efficiency of funding and production), the resulting research patterns, wage-differentials, and also race and gender discrimination in an evermore market-dominated science (for an early survey see Sent 1999, more recent Diamond 2008). For a historically image of the economic organization of science that goes far beyond the static image I present here see Mirowski and Sent (2002). For an economistic discussion of the production function of economists, demand and supply constituents, see Frey (2003). The heterodoxy has a natural interest in changing and thus first describing the rules of the profession of economists (see e.g. Lee 2004). The classic historical work is that of the social historian of economics A.W. Coats on the professionalization of economics (1993). For another historical work on the professional ethos of British economists in the 20th century, see Middleton (1989).

Let me thus open the black box of "the economist". Who are those people making their money with economic claims of epistemic authority? The broadest spectrum of people I can conceive of ranges from those who have some training in economics but are employed outside of academia, to those who are employed in economics departments. What I call the profession of economics must include all the in-between cases: from academic economists who do full-time teaching, to applied economists, to economists in departments other than economics, to researchers in think tanks, to economic advisers in governmental and international organizations, and private companies. This excludes "economists" who work as accountants, statisticians, and managers, insofar as they are not informed by economic theory, nor utilize its authority, nor got their job because of a training in economics. Having observed above a gap between economic science and the rest of economic talk, we here find a rich institutional continuum between science and its paying audience. In which sense, then, can we speak of each instance of "economist" as a profession?

The Producers of Economic Theory Associated in the AEA – Arcane as Artists, but Rigid as Taylor Workers: Social Responsibility?

This range of academic economists is considerably broader than what in the literature is referred to as the academic profession of economics. There one often includes members of the *American Economic Association* (AEA), who hold a PhD and are employed full-time at a university or college (Siegfried 1998). The AEA had 17.143 members in 2007, having declined in recent years. It first reached this figure in the 1960s, when membership doubled from 10.000 – itself up from 5000 in the 1950s. Approximately 13.000 economists in the U.S. are employed by around 400 colleges and universities, with a median salary of 72.780 dollar per year. Their overall research costs tally at 30.5 billion dollar – about 0.0006 % of the world income, as van Dalen and Klamer guess (2005). About 1000 economics PhDs graduate per year, half of which are not from the U.S. – though 80% white and male! Around 30.000 undergraduates choose economics as their major year after year (see Siegfried 1998, 1999, Klamer 2007). In order to receive an entry in *Who's Who in Economics*, more narrowly, one must have published and have been quoted by others in one of the ranked journals. Today, 1168 economists, half of them alive, meet these criteria, which is twice the number in 1983 (see Blaug, Vane 2003).

The AEA, founded in 1885, has gained the rank of *the* institution of the profession of economists. Noteworthy in the present context is that at its very beginning it was not yet clear whether it would be a professional or academic association (Coats 1993: 205 ff.). Many of the early members were not academics. Can we think of the AEA today as an institution that functions like a professional union or guild? The AEA always played an active role in unifying economics by setting the standards of research, and, moreover, by standardizing undergraduate and graduate teaching (Siegfried and Hinshaw 1991). Yet it has never handed out a certificate for economic services such as policy advice or forecasting. It neither has ever written a statement of professional ethics. The AEA is not held together by the services it provides. Instead, what holds it together is economic theory. If one has no training in economic theory, one will not excel much in that society. *The* professional skill of economists, as represented by the AEA, is teaching economic theory, on the one hand, and using it to structure data sets on the other. What kind of proficiency is that?

The skills needed to produce economic theory include, roughly speaking, some technical abilities like real analysis and dynamic programming, combined with the capacity to come up with a narrative that "explicates" the technical language in whatever context in which the economist wants to claim authority. Optional, but today ever more fashionable, are the technical skills of dealing with data, running tests, using econometric software such as Ox, Stata, or E-views, etc. Using these skills, economists' actual products are journal articles – the more regular, frequent, and quoted, the better. Other writings, such as book-length treatises, textbooks for students, and books for a general audience do not contribute to the professional status of an economist. Contrary to the view that even children hold economic theories, economists are in fact the only people who engage in such arcane effort, as the following economist describes.

> [A]cademics are trying to solve the following problem: show that starting from a set of not completely implausible assumptions can lead one to an interesting (i.e., novel or counterintuitive) result. No one else (i.e., private sector economists, government economists, policymakers, economics undergraduates) has that as an objective. Thus, unless and until they adopt this objective as their own, the people listed in brackets above find much of academic economics misdirected, irrelevant, or esoteric. This leads to some understandable frustration (in Colander 2003: 161).

Indeed, nobody outside the profession ever reads the theoretical work of economists. Most economic theorists never communicate their work to any other person than economists. Moreover, only a small part of all articles are read and quoted by others. In this respect, economists seem hardly like a profession, but more like artists, from whom only a few stars are noticed at all. As opposed to artists, however, the literary appearance of economic theory – including style, methods, technical vocabulary, and structure of articles – is highly rigid. Economists do not have to make up on their own what kind of text they are supposed to produce. This again suggests a professional rather than scholarly activity. Let me thus have a closer look at a typical journal article.

The depiction of the motivation of the enterprise entails common-sense notions that have some intuitive footing in one of the topics an outsider could identify as an economic issue. In the last AER issue, for example, titles include notions such as "large pay-off", "global imbalances", "auctions", "centralization", "public spending", "electricity markets", etc. The actual problem, however, is defined in relation to already-established theories. Without knowing them, the article cannot be of interest: "large pay-off game shows", "an equilibrium model of global imbalances", "asymmetric auctions", "a dynamic theory of public spending". The actual audience is addressed in the literature survey, which consists of other articles to be refuted, qualified, generalized, applied, etc. They usually do not extend back more than ten years. The model follows. Models are assemblages of well-defined relations of technical terms, based on particular theoretical principles. There are basically two (four) theoretical principles that identify an economic article: (ir)rationality and (dis)equilibrium. A model typically consists of mathematical equations. However, other models, such as technically richer computer simulations, are coming into use too (see for example Kim, Morse, Zingales 2006).

Then an "empirical" section follows. The model is specified in such way that it allows for a quantitative testing procedure – econometric testing. This testing, too, McCloskey has long protested, is highly standardized. What truth possibly means is not up to the economist.

Around the 1970s, when the fashion I describe began to dominate economists' academic writings, theory had priority. But in recent years we can observe a turn to more empirical work. In 1970, 70% of papers were theoretical, and 13% included empirical data. In 2000, only 11,4% were solely theoretical, while 60% included data (Kim, Morse, Zingales 2006: 203). Some speak of an "empirical turn", to which I will return at several points below.

The conclusions of a typical article do entail moderate political claims, but never without reference to further work. In order to offer a taste, here are the concluding lines of an article on public spending, taxation, and debt. Note how the author gains policy relevance *and* epistemic authority by moving back and forth between technical and political notions.

> The result [of combining tax smoothing and pork-barrel spending] is a tractable dynamic general equilibrium model that yields a rich set of predictions concerning the dynamics of fiscal policy and permits a rigorous analysis of the normative properties of equilibrium policies. There are numerous ways the theory might usefully be extended. A particularly interesting extension would be to introduce cyclical fluctuations in tax revenues due to the business cycle. This could be achieved by specifying a stochastic process (with persistence) for the private sector wage (…). It would be interesting to know what the type of theory developed here predicts (Battaglini, Coate 2008: 223).

These extensions may be interesting for both the author and the politician who uses these predictions – but, as the reader may imagine, for rather different reasons. In the appendix of the article, the formal proofs of the model's Lemmas and Propositions follow. In the quoted case, these proofs take up about a third of the space of the entire article, and perhaps most of the time spent on it. That not being enough, the author needs to refer to an additional Web appendix with yet more proofs.

Such rigidity represents the black hole into which the efforts of most economists fall. "Close to a thousand manuscripts a year" – reviewed by the former editor of the AER, Robert Clower – "and I swear that the profession would be better off if most of them hadn't been written, and certainly if most of them hadn't been published" (Clower 1989: 23). Better they not, as long as proficiency is gained at the cost of the expressive strength of intellectual efforts, as, among others, Frey argued (2003). Academic publishing, he complains, goes at the cost of originality, creativity, and other scholarly virtues. Frey called the process of getting an article published in one of the ranked and peer-reviewed journals as "intellectual prostitution":

> The system of journal editing existing in our fields at the present time virtually forces academics to become prostituted: they sell themselves for money (and a good living). (…) [A]cademics sell their soul to confirm to the will of others, the referees and editors, in order to gain one advantage, namely publication. Most person refusing to prostitute themselves (…) are not academics: they cannot enter, or have to leave, academia because they fail to publish. Their integrity survives, but the persons disappear as academics (Frey 2003: 206).

Frey's suggestion is to treat economists more like artists. But, then, could economists still claim epistemic authority?

The dominant control apparatus that reinforces this intellectual culture is the *ranking system*. Rankings contribute a great deal to the appearance of professional closure. They are the "measure" of intellectual accomplishment (Husserl would turn over in its grave). As aloof as the practice of economists may seem, rankings make their accomplishments tangible. They provide structure to most of the professional life of economists: which conference to go to, what kinds of topics to address, which references to make, which contacts to pursue, which department politics to support, whose tenure to grant, which curriculum to enforce, etc. All professional activities can be viewed in light of such rankings (rather than, as in scholarly disciplines, being oriented by, say, intellectual communities).

There are about two handfuls of high-ranked journals that set the profession's field of attention, the oldest of which are the *Journal of Political Economy* (1892, Chicago), *Quarterly Journal of Economics* (1886, MIT), *American Economic Review* (1911, AEA), *Econometrica* (1933, Econometric Society), and the *Review of Economic Studies* (1933, LSE) (see e.g. Kalaitzidakis et al. 2003). The majority of the middle-ranked journals have been founded between the 1960s and the 1980s. EconLit, the relevant research database, lists around 600 economic journals. There are notably no non-English-speaking journals in any ranking. Rankings exist not only for journals, but also for economics departments and universities. The highest-ranked institutions for decades have been departments such as Harvard, Chicago, MIT, Yale, Princeton, Stanford, etc., trailed by LSE and two or three European and Asian Universities in the double-digit rankings. Even single economists are ranked (see Coupé 2003). Not only the top ten, but a list of more than a thousand economists can be found at IDEAS. I could fit 216 on the following page. Which notion of scholarship can such a list possibly convey?

Although rankings dominate the everyday life of economists, I suppose no economist would *not* agree with Van Dalen and Klamer, who reminded us that "not being cited is not necessarily a sign of waste, just as receiving many citations is not necessarily a sign of scientific breakthrough" (2005: 407, see also Gillies 2006). Though nobody may have ever had doubts about that, the pile of rankings grows and grows. Ironic commentary presents itself readily. Rankings, says Colander, play different roles. They play

> (…) political (show them to the dean to support your budget increase request), psychological, and sociological (show them to your friends and to yourself to make them feel worse and you feel better) roles. More rankings increase the probability that one's school will have done well on one of them; cognitive dissonance takes care of the rest (Colander 1989: 142).

Is the number of rankings not a clear sign of insecurity about the palpability of intellectual accomplishment, lack of an inherent criterion as to what an intellectual accomplishment amounts, and the lack of social feedback from outside the profession? Assuming that economists, anyhow, at least secretly dislike such measures, are they not a bad surrogate for a discursive judge? What else than an ironic attitude about the profession is induced if professional closure is achieved by rankings nobody believes in, but everyone orients themselves by?

When viewing the profession along such rankings, professional closure comes at the costs of excluding professional services. Economics is inward-oriented. If publications in ranked journals are the main occupation of the profession, one is inclined to believe that economics is beyond both proficiency (for providing no service) and scholarship (for showing no inherent discursive judge). Many have embraced that image, and sung lamentations similar to that of Mark Blaug: "modern economics is sick. Economics has increasingly become an intellectual

1 Joseph E. Stiglitz
2 Robert J. Barro
3 Andrei Shleifer
4 James Heckman
5 Robert Lucas Jr.
6 Peter Phillips
7 Jean Tirole
8 Olivier Blanchard
9 Martin Feldstein
10 Edward Prescott
11Daron Acemoglu
12 John Campbell
13 Mark Gertler
14 Lawrence Summers
15 Christopher Baum
16 Thomas J. Sargent
17 Maurice Obstfeld
18 Lars Svensson
19 Alberto Alesina
20 Stephen Turnovsky
21 Gregory Mankiw
22 Nicholas Cox
23 James H. Stock
24 Robert G. King
25 Alan B. Krueger
26 Michael Woodford
27 James Poterba
28 Ross Levine
29 Barry Eichengreen
30 Elhanan Helpman
31 Robert J. Gordon
32 Jordi Gali
33 Ben S. Bernanke
34 Pablo Fernandez
35 Kenneth S Rogoff
36 Gary S. Becker
37 David E. Card
38 Martin Eichenbaum
39 Jeffrey Frankel
40 Edward Glaeser
41 Paul R. Krugman
42 Bennett McCallum
43 Sebastian Edwards
44 Richard Freeman
45 Jean Laffont †
46 M Hashem Pesaran
47 John B. Taylor
48 Eugene Fama Sr.
49 Robert F. Engle
50 Paul Romer
51 Christopher Sims
52 Peter A. Diamond
53 Dani Rodrik
54 Donald Andrews
55 Alan Auerbach
56 George A. Akerlof
57 L. Christiano
58 Lawrence F. Katz
59 Raghuram Rajan
60 Joshua Aizenman
61 Angus S. Deaton
62 Lars Peter Hansen
63 Paul Milgrom
64 Andrew Kenan Rose
65 Oliver D. Hart
66 Boyan Jovanovic
67 Clive W. J. Granger
68 Rudiger Dornbusch †
69 Robert C. Merton
70 Zvi Griliches †
71 Gene Grossman
72 Patrick Kehoe
73 Avinash Dixit
74 Florencio Silanes
75 Richard Blundell
76 Martin Ravallion
77 Frederic Mishkin
78 Bruno S. Frey
79 Timothy J. Besley
80 Robert J. Shiller
81 George Borjas
82 Eric S. Maskin
83 Stephen John Nickell
84 Robert Ernest Hall
85 Kevin M. Murphy
86 Sergio T Rebelo
87 Tim Bollerslev
88 Kenneth R. French
89 Ricardo J. Caballero
90 Rafael La Porta
91 Bruce D. Smith †
92 Guido Tabellini
93 David Romer
94 Drew Fudenberg
95 Joshua D Angrist
96 Francis X. Diebold
97 Peter Nijkamp
98 Daniel Hamermesh
99 M. Carmen Guisan
100 Paul A. Samuelson
101 John H. Cochrane
102 Julio Rotemberg
103 Sherwin Rosen †
104 Allen N. Berger
105 Finn E. Kydland
106 Edward Lazear
107 Willem Buiter
108 Xavier Martin
109 Martin Shubik
110 Martin Weitzman
111 Alan S. Blinder
112 Richard H. Thaler
113 Jeremy Stein
114 Robert Feenstra
115 Laurence Kotlikoff
116 Douglas Bernheim
117 James Hamilton
118 Kenneth West
119 Andrew Abel
120 William Easterly
121 Mark Taylor
122 David Neumark
123 James Markusen
124 Pierre Perron
125 René Stulz
126 John Haltiwanger
127 David Cutler
128 Torsten Persson
129 John Whalley
130 Sanford Grossman
131 Andrew Oswald
132 Halbert White
133 Robert Hubbard
134 Carmen Reinhart
135 Charles Engel
136 Steven Levitt
137 Chris. Pissarides
138 Larry G. Epstein
139 Jonathan Eaton
140 Asli Kunt
141 Richard Rogerson
142 Anthony Venables
143 Ernst Fehr
144 Jeremy Greenwood
145 Charles Jones
146 David Hendry
147 Randall Wright
148 Amartya Sen
149 Roland Benabou
150 Glenn Rudebusch
151 Ray C. Fair
152 Douglas Diamond
153 Jonathan Gruber
154 Shang-Jin Wei
155 Alvin E. Roth
156 Soren Johansen
157 Ellen McGrattan
158 Douglas Gale
159 Jose Scheinkman
160 Geert Bekaert
161 Michael C. Jensen
162 David N. Weil
163 John Moore
164 James Tobin †
165 Charles F. Manski
166 Jess Benhabib
167 Christopher Carroll
168 Robert Moffitt
169 John List
170 Robert M. Townsend
171 Orley Ashenfelter
172 Oded Galor
173 Athanas. Orphanides
174 Stephen Morris
175 Varadarajan Chari
176 Richard J. Zeckhauser
177 Danny Quah
178 Campbell Harvey
179 Vernon L. Smith
180 Matthew O. Jackson
181 Assar Lindbeck
182 Jacques Thisse
183 Dale T. Mortensen
184 Carl Shapiro
185 W Kip Viscusi
186 Anjan V. Thakor
187 Anil K Kashyap
188 Gilles Saint-Paul
189 Adrian Rodney Pagan
190 Michael David Bordo
191 Alex Cukierman
192 Joseph G. Altonji
193 Daniel Kahneman
194 Andrew Hallett
195 G. William Schwert
196 Steven N. Durlauf
197 Janet Currie
198 Martin Browning
199 Stephen P. Jenkins
200 Eric A. Hanushek
201 Richard R. Nelson
202 Manuel Arellano
203 Bruce E. Hansen
204 Thomas F. Cooley
205 Gordon Hanson
206 Kenneth A. Froot
207 Michael B. Devereux
208 Steven Shavell
209 Charles L. Evans
210 Glenn Ellison
211 Naray. Kocherlakota
212 Philip Lane
213 Peter Howitt
214 Walter Erwin Diewert
215 David M Newbery
216 Enrique Yacuzzi

game played for its own sake and not for its practical consequences for understanding the economic world." (2002: 36) We read another rendition of this song in Colander:

> By the late 1960s, the formal techniques necessary to undertake the noncontextual arguments [theory] had become so great that younger economists were no longer being trained to know about real-world institutions, a requirement if one is going to talk seriously about policy. Unless an economist has inherent or independently acquired abilities in communicating ideas to the outside world, becoming an economist is like join a priesthood sworn to communicate only among its members (1991: 22).

The riddle that such complaints leave open, however, is how it was possible that economic theory, in spite of the closure of its production, could nevertheless leave considerable traces in the economic talk of the last decades. There is an astonishing gap between the inward orientation of economic theorists and the scope of its impact on its environment. This impact, in other words, takes place *indirectly*, which is to say that there is nobody who actually mediates between economists and the rest of economic talk (see Colander 1991: 19 ff, Frey 2003). The impact of economic ideas can be a total mismatch with the intentions of their originator. Those who utilize, or are in any sense influenced by, economic theory are not the same people who design it. To be sure, this is always the case in scholarly discourses. However, the closure of economic theory makes us wonder if the absence of economists from the production of meaning in economic talk is perhaps *constitutive* of its impact. This represents the problem of *social responsibility* of theoretical economists, to which I will return again and again.

Rather than Civil Servants, the Social Engineers of Freedom – All around the Globe, Even in Totalitarian Regimes: The Epistemic Dialectic of Neoliberalism

Part of this puzzling success of economics is its ongoing internationalization. Academic economics, no doubt, has its home in the U.S. The greatest majority of all ranked journals, universities, and economists, including those who have earned the greatest of all accolades in economics – the *Bank of Sweden Prize in Memory of Alfred Nobel* – are based in the U.S. This is not surprising, to the extent that the criteria of success in other countries are not exactly the same. Since WWII, but primarily since the 1970s, these cultural differences have faded. Economics has internationalised – or, better, Americanized (see in full detail Coats 1996).

European economics departments, for example, traditionally have close ties with their local institutional and political environment, but are on the way to becoming equivalent to second-ranked American departments (Frey and Eichenberger 1993). Latin American departments have traditionally been more rooted in political economy and socialist traditions, but now they too obey to the North-South hierarchy (Montecison, in Coats 1996). This is true since the 1950s at the latest, when Friedman send Harberger to Chile, after which "half of the economists trained at leading foreign institutions studied at Chicago" (Harberger, in Coats 1996: 302); Korean and also Japanese departments are traditionally socialist, yet were greatly influenced by the U.S. through their homecoming graduates (Choi, in Coats 1996, and Bernstein 1999). Samuelson comments:

> Again and again I have seen in recent decades the tremendous stimulus that top postdoctoral scholars from abroad have received from a year's soujourn in the States. They go back home fired up to change the old world (in Breit and Spencer 1995: 62).

Samuelson did contribute his share to fire up the rest of the world. For the greatest force of the Americanisation of economics are certainly textbooks. Ever since his *Economics*, they tend to be the same all over the world. This is reinforced, for example, by the "charity" of institutions like the University of North Carolina, which in 1990 donated economics textbooks to universities in Czechoslovakia, Hungary, Poland, and ex-Yugoslavia (Bernstein 1999: 112).

According to the inward orientation stated above, this international success is a mystery. What makes economics departments all over the world stand in queue to get into the U.S. rankings? Is it part of the general success of markets in shaping political discourses? Is it part of a shift in the bias of science from socialist to liberal? No, not at its surface. Economics departments, when adopting to the U.S. institutions, rather, change the perception of the profession they represent: from that of the *civil servant* to that of *social technocrat* – apparent in the case of the countries formerly under the cultural influence of France. Economics in France is traditionally embedded in the education of public administrators, who are trained in order to function in particular public institutions and required to be knowledgeable in legal and historical matters (for similar observations regarding Korea, see Choi, in Coats 1996: 106 ff.).

So it happened, for example, in Iran in the 1960s. "Higher education was revamped and expanded in response to the growing demand for technocrats" (Behdad 1995: 195). It was John Hicks who helped carry out the Samuelsonian reform of the economics department in Teheran. The great promise of U.S. economics was not to infiltrate western politics in academia, but, to the contrary, to find a way to remove economic discourse from its socialist *and* liberal bias. For this reason, economics survived the so-called Cultural Revolution in 1978.

> There has been little 'Islamization' in the structure of the economics program in post-revolutionary Iran (…) It is ironic that the Islamic rejuvenation of higher education and the ideological cleansing of economics, one of the most seriously contested disciplines of learning by the Islamic Republic and its ideologues, have only strengthened the process of Americanisation of the economics discipline (Ibid:.212).

Such is one of the riddles of present-day economics: it grows from the most advanced market society (the U.S.), but easily merges with the educational politics of totalitarian regimes (and increasingly with the quasi-democratic politics in the U.S. itself). The most well known case is Friedman, who, in the 1970s, made Pinochet's Chile his "laboratory" of economic liberalization (for a participant account see Harberger (in Coats 1996), for an early critic see Letelier 1976, for a philosophical assessment Schliesser 2007). Economic freedom and political violence can easily coexist, while the former, as the left insists, legitimizes the latter.

It is the spirit of *social engineers* that stamped the pragmatic conception of the economic profession most intrusively. Despite the rigidity of economic theory, some eminent economists have managed to go beyond teaching and publishing as sources of income and make direct epistemic interventions in the political sphere. At least this is the case for *the* economists, the Samuelson-Summers-Sachs's. Paul Samuelson can be held most responsible for the public image of economists as social engineers. He was able to push both economics as a science and its political utilization at the same time. Having merged Keynesian aspirations, neoclassical ambitions, and econometric sophistication, economists can thank him for the fact that they are constantly held to the standard of political relevance. It is worth quoting his advising activities, for it reflects the institutional scope of the demand for epistemic authority in economic talk.

> Professor Samuelson has served widely as a consultant. He worked for the National Resources Planning Board from 1941-1943 (in charge of war-time planning for continuing full employment); the War Production Board and Office of War Mobilization and Reconstruction in 1945 (economic and general planning program); the United States Treasury, 1945-1952; the Bureau of the Budget in 1952; the Research Advisory Panel to the President's National Goals Commission from 1959-1960; the Research Advisory Board Committee for Economic Development in 1960. He was a member of the National Task Force on Economic Education from 1960-1961 and has been a consultant to the Rand Corporation since 1949. He is an informal consultant for the United States Treasury and the Council of Economic Advisors. He is also a consultant to the Federal Reserve Bank. He was Economic Advisor to Senator, candidate, and President-elect Kennedy and was the author of the January 5, 1961 "Samuelson Report on the State of the American Economy to President-elect Kennedy" (nobelprize.org).

The list only includes activities until the mid 1960s. Samuelson is still alive!

But Samuelsons are rare. He became an economist because he could not understand the fuss about such a trivial discipline (1992: 236). For most economists, to get one article published per year is full-time occupation, so that theoretical and political engagements do not interact. They exclude one another because they require different skills. Is Samuelson a successful advisor *because* he is a successful theoretician? Even though those who demand his authority may believe so, there are different skills at work. As Coats concludes from his study of economists active in the contexts of governments:

> Though professional skill (that is, knowledge of an ability to use economic ideas and techniques) is a necessary condition for success in an official bureaucracy, (…) it is by no means sufficient. The list of desirable qualities required by an ideal government economist is indeed daunting. It includes tact; patience; adaptability; the ability to communicate with non-specialists in a variety of circumstances and at different levels of audience comprehension; skill in the arts of persuasion; recognition of the limits of one's professional expertise (…) (Coats 1989: 117f.).

Etc. One acquires none of these skills by trying to publish one article a year. To the contrary, exceptional of economists like Samuelson is that they are able to maintain their intellectual wit despite their theoretical ambitions.

If the skills necessary to produce theory do not match the skills necessary to produce economic advice, then more is going on than "applying theory". I consider the very word "application" as misleading for describing economists' practices. Economic theory and political advice do not relate like the general and the particular. Applying means to be more *careful*, but not *creative*. An application is supposed to add nothing new in principle, only in the concrete. If one can derive contrary results from the same theory in the same situation, as is the case, more is going on than mere application. Economics and politics do not relate like positive theory and normative applications. They are mediated by something else.

The influence of economic theory on its discursive environment, Samuelsons apart, does not occur by means of the economist, but indirectly. And this indirect effect – so it seems since the 1970s – tends in one direction: in support of pro-market policies. Is economic science today not the science of the NAFTAs and GATTs, of privatization of public services, of deregulation of energy markets, road pricing, auctions for IT property rights, etc.? The riddle is this: By which murky path does the aloofness of economic theory from economic policy lead to an advocacy of market policies?

> In the economics community the academic ideal, namely that of pure research truth-seeker, the detached non-partisan experts, outweighs any more pragmatic conception of professionalism or public service; and when this ideal is transported into the non-academic realm it often takes the form of partisan advocacy of efficiency and market methods (Coats 1993: 398).

Epistemic authority of "non-partisan experts" and partisan market policies are in a secret alliance. In this mutual support, they both reveal their inner ambiguity: theory being aloof from economic policy *as* a truncate policy, and market policies being theoretically founded *as* being aloof from politics. In its appearance as science, when coming in touch with the political sphere, economic theory supports a particular politics, supports economically-oriented politics, supports the anti-politics of neoliberalism. Doing theory for science's sake, economic claims are led as if by an invisible hand into the political sphere. How can we impel the economist to take responsibility for such effects if the very motive of science is to be beyond politics? As indirect are the influences of economists, so indirect is the suspicion that falls back on them: Is the very pursuit of an economic science politically tinctured?

Such is the epistemic character of economic talk in a post-1945 neoliberal world in which free markets count as a reply to the failure of politics in that they substitute politics. This dialectic will be a repeating theme in the coming considerations, and I leave the reader with this irritation about the pragmatic image of economists. Here I merely want to maintain that due to this dialectic, economists defy a trivial conception of professional pragmatism.

There has been one economist who has acknowledged the full consequences of the non-trivial character of economic knowledge in a neoliberal world – Friedrich von Hayek. Arguing forcefully against social engineering, he was the only one who defended scholarship at the cost of professionalism. When addressing his LSE students in 1944, he plead that "Being an Economist", as he titled his speech, *requires* being irrelevant, because the knowledge of economists is ineffective if it not realized by the society on its own. Economists' influence can only be indirect, for a direct influence is against the ideology that allows for this knowledge in the first place: free markets. Although the economist may know what is better for society, he or she cannot truly claim it. If you really believe in economics, you need to keep it for yourself!

> In this economics differs from other disciplines. We do not, as the other sciences do, train practitioners who are called in when an economic problem arises (…) The reason why I think that too deliberate striving for immediate usefulness is so likely to corrupt the intellectual integrity of the economist is that immediate usefulness depends almost entirely on influence, and influence is gained mostly easily by concessions to popular prejudice an adherence to existing political groups (Hayek, in Klein 1999: 143).

When Hayek spoke about "popular prejudices" in 1944, he meant socialism. At the end of WW II, Hayek saw the university insinuated by social engineering. "[T]he attraction of a planned and directed economic system is now as strong among the American intellectuals as it ever was among their German or English fellows." (Hayek 1949: 371) Intellectual integrity, according to Hayek, thus demands to withdraw from immediate utilization of economics, and to take influence via general scholarship. For this reason he launched an alternative institution, which I discuss below. Rejecting the idea of a political party, he suggested the following:

> I would join with other in forming a scholarly research organisation to supply intellectuals in universities, schools, journalism and broadcasting with authoritative studies of the economic theory of markets and its application to practical affairs (quoted in Backhouse 2005: 368).

The perversion of such a political scholarship happens when it falls back on the institutions of economic science. The best instance is – who else – Stigler, who speaks about "professional integrity" with the following words:

> One evidence of professional integrity of the economist is the fact that it is *not* possible to enlist good economists to defend protectionist programs or minimum wage laws. The groups who seek such legislation accordingly must seek elsewhere for their spokesmen and theorists (Stigler 1982 [1976]: 60).

Applied Economics – the Intellectual Acrobatics of Maintaining one's Interest in a Domain, and the Unifying Rejection of Economic Imperialism

The field where the greatest numbers of economists make their living is not economic science, but the economic *sciences*, that is to say all the countless "applied" fields in economics. For an impression of what the profession consists of in this sense, have a look at the list of associations on the next page that, centred in the AEA, have "allied" at the *Allied Social Science Conference* in 2007 and 2008. To this list I should add other economists working in other disciplines at other departments at which economic theory is part of the curriculum, such as in business economics, public finance, public administration, and a number of recently invented studies such as "innovation design and management", "information architecture", etc. I should also add the interdisciplinary specializations such as economics and law, economic sociology and social economics, economic geography and geographic economics, economic psychology, etc. The diversity of the realm of the economic sciences – as is also apparent in the list – is not older than some decades. Many of these associations were founded after 1970. This is not surprising if we believe Mirowski and Sent that the very distinction of pure and applied is "an artefact of the Cold War regime" (2002: 22). I will return to that below.

What differs between applied and theoretical economists must be their perception of a distinct domain of application. This perception serves as the source of relevance and motivation for the economist. What is important for health economics, how could it be the same as what is important for public finance? Proficiency here comes down to specialized knowledge of a field that includes institutional, historical, and technical knowledge: 'How much agriculture will be feasible in Greenland in a century?' 'Who benefits from tax cuts on private care if the demographic situation will have changed in a couple of years?'

If so, what makes applied economists ally in economics? Do they share an encompassing perception of a unified domain? Is there an Ur-domain of economic theory that encompasses all special worlds where it is applied? Hardly. It is rather the kind of question that applied economists pose, which often can be reduced to: market, yes or no? The main question, for example, in environmental economics is 'Should we restrict pollution by law, or sell rights for

Allied Social Science Associations 2007-2008

1885: American Economic Association
1907: National Tax Association
1919: American Agricultural Economics Association
1930: Econometric Society
1939: American Finance Association
1940: Economic History Association
1941: Association for Social Economics
1946: Transportation and Public Utilities Group
1947: Labor and Employment Relations Association
1949: National Council on Economic Education
1959: National Association for Business Economics
1963: Omicron Delta Epsilon
1963: Peace Science Society International
1964: American Real Estate and Urban Economics Association
1967: Association for Evolutionary Economics
1968: Association for the Study of the Grants Economy
1968: Union for Radical Political Economists
1969: National Economic Association
1970: Society of Government Economists
1974: History of Economics Society
1975: Association of Indian Economic Studies
1977: International Association for Energy
1977: International Society for New Institutional Economics
1978: Middle East Economic Association
1979: Association of Environmental and Resource Economists
1980: National Association of Economic Educators
1982: American Committee on Asian Economic Studies
1982: Association of Christian Economics
1982: International Society for Inventory Research
1983: Cliometric Society
1983: Association of Financial Economists
1984: The Korea-America Economic Association
1986: Association for Economic and Development Studies on Bangladesh
1988: African Finance & Economics Association
1988: International Trade and Finance Association
1988: National Association of Forensic Economics
1989: Chinese Economic Association in North America
1990: Association for the Study of the Cuban Economy
1990: North American Economics and Finance Association
1990: International Association For Feminist Economics
1990: Society for Economic Dynamics
1993: Society for Policy Modelling
1994: International Network for Economic Methodology
1995: Society for Computational Economics
1996: Economic Science Association
1998: International Economics and Finance Society
1998: International Health Economics Association
2000: Association for Comparative Economic Studies
2001: Health Economics Research Programme
2003: Industrial Organization Society
2006: Economists for Peace and Security

pollution?' In health economics: 'Are we 'healthier' if we pay our doctors directly or via the state?' In international and developmental economics: 'Did foreign aids prevent the people in central Africa from specialization in labor-intensive products?' And in cultural economics: 'Should we subsidize the art although hardly anyone is willing to pay for it?' Certainly these questions are of public concern, but they do not stem from how the public perceives the problems of each domain. They stem from economic theory being the theory of markets, whatever domain is at stake. Theoretical rigidity prevails also in applied economics:

> In the 1970s many fields that had previously been open to less technical work followed suit. Industrial economics, development economics, and even international trade and macroeconomics began to require understanding of mathematical techniques not required a decade before (Backhouse 2005: 382).

Economic theory does not unify applied economics by means of being more general in terms of scope of applicability. Instead, it unifies by means of pre-determining the kinds of questions that can be posed, and by means of narrowing the conceptual borders of economic talk. The perception of a particular domain hardly functions as a guide for applied economics. The philosopher's concern for the scope of economic theory does not enter the consciousness of practicing economics. If this is the case, an interesting question is again which skills are needed in order to apply economic theory? Does mastering the theory amount to the same as knowing how to apply?

Consider, for example, the division of labor between economic sociology (done in sociology departments) and social economics (a heterodox approach), between industrial economics (including theory of the firm) and parts of business economics, between political economics (say, social choice theory) and political theory at political science departments, between economic geography and geographical economics, etc. They do share the same domain. But how can we believe that they will ever merge? Does this not suggest that the skills here and there compete with each other? Doing applied economics, does it prevent the economist from gaining proficiency in other discourses about the same domain? The more one engages in economic theory, the less one is guided by one's perception of a concrete domain? And if so, what is it that leads to claims in applied economics?

If we believe the joke our grandmothers know (about the one-handed economist), applying economic theory leads instead to a range of all *possible* economic claims, but not yet to an actual economic claim. Is the effect of economic theory in all those fields not to *structure* the discussion, and to provide the form of argument without actually contributing? Think for example of the capacity of economists to engage in interdisciplinary relationships: What does economic theory contribute to other disciplines? What happens when market theory is applied to "pollution", "health", "art", "education", etc.? What kind of intellectual labor is it to specify MaxU as MaxU^P for P= "pollution"? What is it to view the world in light of economic theory? Do economists not face the sheer impossibility of the intellectual acrobatics of holding onto a conceptual frame that hardly interacts with the intuitions oneself and others have about the field – continuously confronted with the inappropriateness of this conceptual framework? Are not the best economists those who are able to keep their intuitions apart, pretending never to have made any contact with the field, and really claim, "Yes, art, children, pollution – all that *is* utility, *is* a commodity, *is* an asset etc." – *whatever* that means!

The origin of this problem is the well-known idea of Becker (1976) that economics is indeed not a discipline among others, but a method of the social sciences. Then there is not a discipline called economics applied to various domains, but a discipline that gains discursive power by taking over others: "economic imperialism". For this Becker has received the Nobel Prize in 1992, and the Presidential Medal for Freedom in 2007.

> Indeed, I have come to the position that the economic approach is a comprehensive one that is applicable to all human behavior, be it behavior involving money prices or imputed shadow prices, repeated or infrequent decisions, large or minor decisions, emotional or mechanical ends, rich or poor persons, men or women, adults or children, brilliant or stupid persons, patients or therapists, businessmen or politicians, teachers or students (…) Subsequently, I applied the economics approach to fertility, education, the uses of time, crime, marriage, social interactions, and other "sociological," "legal," and "political" problems (1976: 8).

I suppose that there is no single applied economist today who embraces Becker's attitude, and conceives rationality and equilibrium as principles of all human behaviour. No applied economist, I suppose, believes that economic life is not a "compartment" of life – the point to which Becker brings his approach home (Ibid.: 14). No applied economist does not believe that some things of life should concern the economist more than others. And yet, despite this disbelief, discursive boundaries between economic sociology and social economics, between cultural economics and cultural studies etc. hold strongly. Perhaps precisely because of this disbelief? Perhaps what makes applied economics ally under the ASSA umbrella is neither a shared interest in particular phenomena, nor a particular theory or method, but the *rejection* of theoretical principles? From finance to cultural economics, is what differentiates the economist from others not that the economist believes that rationality is *not* a sufficient principle to speak about the economic domain?

Economists in Think Tanks – Non-Partisan Neoliberalism and the Treasonous Division of Labor between Doing and Utilizing Economics

For now, I leave applied economics with these questions open, and move on to the close environment of academia. There, economists work in so-called 'think tanks' (Heidegger would turn over in his grave). Think tanks fund research, both theoretical and applied, both at universities and at their own institutions, and infuse it into the political discourse or directly into governments. In think tanks, economists do policy research of an academic sort, but in a non-academic context. There, they produce both policy reports and academic publications. Thinks tanks cultivate epistemic authority in politics. Think tanks are the institutions that mediate between the culture of academic research and policy making (for a general assessment see Stone 1996, regarding economics e.g. Smith, in Colander and Coats 1989).

Let me list some of the more famous think tanks that have flourished since the 1970s. In the U.S., worth mentioning are the neo-conservative *American Enterprise Institute* (1943), the libertarian *Cato Institute* (1977), and the *Heritage Foundation* that supported the Reagondom of the 1980s (1973). In Great Britain, most important is the *Institute of Economic Affairs* (1955), launched with the help of Hayek, later followed by the *Adam Smith Institute* (1977), and the

Centre for Policy Studies (1974), which was founded by Margaret Thatcher in order to disseminate free market ideas and to protect the independence of Britain. Recently established is the *Globalisation Institute* (2005) affiliated with British Conservatives, which likewise fosters the perception of benefits of global markets. One of the first, biggest, and most influential think tanks is the RAND Corporation, which was founded in 1945 by the U.S. Air Force and proliferated in the 1970s. Many Princeton-Chicago-Stanford economists work there in close association with the U.S. and many other governments all over the world. It includes the *RAND Journal for Economics* (1970) as well as a graduate school. One of their core values is "objectivity". The father-organization of think tanks is *Atlas* (1981), the mission of which is to "bring freedom to the world by helping develop and strengthen a network of market-oriented think tanks that spans the globe" (quoted in Backhouse 2005: 370). One World, One Market, One Science?

Rather exceptional are research centers that are in close affiliation with a particular school of thought *and* a particular political position, such as the *Ludwig Mises Institute* (1982) that promotes libertarian Austrian economics. Another candidate that is often mentioned as being responsible for the spread of neoliberal ideas is the *Public Choice Society* developed by James Buchanan, Gordon Tullock and Mancur Olson, located at the same place as the Austrian center, at George Mason University. It supported the spread of the idea that the market governs politics rather than politics governing the market (see Amadae 2003: 133 ff.). This research group, as opposed to the old and new Austrian school, found inroads in high-ranked journals, and can therefore be called a direct epistemic backdrop for neoliberalism: proving the inefficiency of state activities. The mother think tank of all think tanks, where academic economists, namely *the* economists from both Austrian *and* Chicago schools – both new and old – initiated the epistemic culture of neoliberalism, is the *Mont Pellerin Society,* founded in the aftermath of 1945 (see next page).

Taking one step back in the chain of funding research, I should mention foundations. They direct science into specific political channels, yet seem to patronize science out of sheer philanthropy (see Goodwin 1989, Balakrishnan and Grown 1999). Apart from the National Science Foundation (its political leanings are disputable, too – see Newlon, in Colander and Coats 1989), much academic research at U.S. universities as well as think tanks is funded by private foundations such as the *Ford Foundation* (1936) that supported many of the above-mentioned think tanks, the *Koch Foundation*, the *William Volker Fund* (1932-1965), and the *Earhart Foundation* (1929). The last two, at least, were expressly market-partisan. Earhart fellowships were granted to Becker, Buchanan, Stigler, Vernon Smith, Hayek – which happen to be just the line-up of the *Mont Pellerin Society*. In order to close the circle of academic economists and the-money-kind-of-people, one may find beneath foundations certain industrialists, such as Joseph Coors of the eponymous brewery. He pays. Cheers to the market.

These and countless other thinks tanks, foundations, and research centers are the most apparent manifestations and clearest pushers of epistemic culture in economic talk since WWII. Commentators, who are sensitive to conspiratorial thinking, and want to spell out the murky link between economic science and the master narratives of neoliberalism, need to go into the archives of Mont Pellerin in Switzerland, or ask the fellows of Antony Fisher what it was like talking to Thatcher and creating the *Atlas* network. One may certainly find links to

One science, one party? The Mont Pèlerin Society (1947)

This image shows Ludwig van Mises with Karl Popper at the opening meeting of the Mont Pèlerin Society in 1947. Friedrich von Hayek was its central maker. The society aimed, in Hayek's words, at the "professional secondhand dealers in ideas" (1949: 417), in order to exert a long-term influence via "journalists, teachers, ministers, lecturers, publicists, radio commentators, writers of fiction, cartoonists, and artists" (Hayek 1949: 372). The Society fostered the belief in the "competitive market; for without the diffused power and initiative associated with these institutions it is difficult to imagine a society in which freedom may be effectively preserved" (Statement of Aims). Was this the moment when monist scientism and neoliberal hegemony allied for the first time – the moment when "scientific liberalism" came to earth?

Considering the list of founding members, one is inclined to reply in the affirmative: Maurice Allais, Milton Friedman, Georg Stigler, Frank Knight, Fritz Machlup, Walter Eucken. Later economists like Ronald Coase, Gary Becker, James Buchanan, and Vernon Smith were part of the shrine, too – Austrians, Old and New Chicago united, and all Nobelists, of course. Other members were government officials, most famously the makers of the European "social market", such as Ludwig Ehrhard and Luigi Enaudi. This society can be called the mother of all think tanks. Today an entire empire of think tanks is associated with this society, from the *London Institute of Economic Affair* that has advised Thatcher, down to *Atlas*, which became the father of all think tanks (see Plehwe and Walpen 1999, and for a more extensive archival material, Walpen 2004).

Crucial for the understanding of this intellectual birth of neoliberalism is the experience of WWII. The group believed itself to be on the good side of mankind because politics *as such* has failed. The market appeared as the only viable alternative. In the name of all the deaths before 1945, Hayek took over the "fate of liberalism". In the Statement of Aims, we read:

> The central values of civilization are in danger. Over large stretches of the earth's surface the essential conditions of human dignity and freedom have already disappeared (…). The position of the individual and the voluntary group are progressively undermined by extensions of arbitrary power. Even that most precious possession of Western Man, freedom of thought and expression, is threatened by the spread of creeds which, claiming the privilege of tolerance when in the position of a minority, seek only to establish a position of power in which they can suppress and obliterate all views but their own.

Thus, one science, one party? No. Although there was from the beginning an ally between the anti-political politics of neoliberalism and the political neutrality of science, Hayek, at least, did not push science. The Society was neither a party nor an academic corpus. Hayek has always associated scientific authority in economics with the planning of society. His emphasis on, for example, uncertainty has to be understood as a critique of scientific determination. Hayek was clear about his ambiguous position between science and politics: "what is essentially an ideological movement must be met by intellectual argument" (Statement of Aim). 'Ideological intellectualism' meant for Hayek not to rely too much on data, nor on math, but rather on human nature, cultural evolution, etc. May be the same could be said about Friedman, but certainly not about Stigler or Becker, or later Presidents of the society. As soon as scientism went hand-in-hand with neoliberalism, and the association of science and socialism lost its historical ground, the members again relied fully on academic institutions. This reflects the situation today, in that scientific engineering sneaks into neoliberal politics: a boom in market liberty combined with an ever-increasing apparatus of state control of the market. The question is less: "One Science one Party?" But rather: "Which misunderstandings make science and politics ally with each other?"

both economic theory (such as public choice theory) and to economic institutions (such as the IMF that gives grants to developing countries under the condition of accepting policy advice from U.S. think tanks). In think tanks we find a non-inhibited coalition of economic theory with hard science on the one hand (using high-powered technologies, operations research, mechanism design, etc.) and explicit dedication to partisan values of neoliberalism on the other (privatization, reduction of the welfare state, tax cuts, deregulation, auctions for media and pollution rights, etc.) Here, for example, is the mission statement of the *American Enterprise Institute*:

> The American Enterprise Institute for Public Policy Research is a private, nonpartisan, not-for-profit institution (…) AEI's purposes are to defend the principles and improve the institutions of American freedom and democratic capitalism – limited government, private enterprise, individual liberty and responsibility, vigilant and effective defense and foreign policies, political accountability, and open debate. Its work is addressed to government officials and legislators, teachers and students, business executives, professionals, journalists, and all citizens interested in a serious understanding of government policy, the economy, and important social and political developments (www.aei.org).

Such "serious, non-partisan neoliberalism" – how could it be written on the flags waving in the Berkeley-Chicago-Yale community? If economics since the postwar period, but at the latest since the 1970s, became associated with a neoliberal ideology, it was mainly the accomplishment of such think tanks and the economists they hired. People like John Williamson of the *Institute for International Economics* were those who proclaimed that the Washington consensus

> embod[ies] the common core of wisdom embraced by all serious economists, whose implementation provides the minimum conditions that will give a developing country the chance to start down the road to the sort of prosperity enjoyed by the industrialized countries (quoted in Middleton 1998: 344).

Rather than to "serious economists", Williamson could better refer to intellectual movements such as that of Ayn Rand (1905-1982). She was one of the few who believed that objectivist epistemology and market capitalism amounts to just the same "philosophy" (1967). But a movement as obscure as that of Any Rand would clearly undermine the scholarly ethos of academic economists. Hardly any "serious economist", I suppose, would subscribe to Williamson's statement without reservations.

If economics represents the hegemony of economic talk, it was thanks to the work of people like Williamson. If there are some who made us believe that the market governs politics, that there is a market for law just as a market for health and education, it was the work of think tanks. There we enter the dirty world of economic researchers educating, for example, television writers, who later drop the subject in daily soaps: "Darling, excuse me, I need to do some paperwork for my private pension fund". I later learned of one of my economic instructors doing just that in service to the *Initiative New Social Market-Economy* that wanted to replace the public pension system in Germany. It was replaced, but people still do not hold private pension funds.

Historically speaking, this scary presence of economists has flourished since the 1970s, and does not go further back than 1945. The few existing equivalents in the prewar period hardly engaged in day-to-day policies, but rather mid-term developments (e.g. the *Fabian Society*, 1884, *Brookings Institute*, 1916, see Backhouse 2005: 370). In Keynesean times between the 1930s and 1950s, economists did not need separate institutions to make their theories politically relevant. Economic theory was "soft" enough to lean spontaneously with the institutions of its time.

Rather than entering the archives, I invite the reader to consider what the very existence of "think tanks" tells us about the epistemic culture of economic science. Why is there the need for non-academic institutions in order to produce policy advice from roughly *the same* economic theories that in academia lead to nothing but moderation about epistemic claims? Should we not expect that the political interpretation of economic theories is a contested matter *within* academia? And why is the role of think tanks so little discussed within academia? Is there a division of intellectual labor between *doing* and *utilizing* economic theory? Do think tanks utilize economic theory in a political discourse, or do they show the hidden political interest of economic theory? Does the existence of think tanks reveal the actual destination of economic theory that in academia is inhibited for all-too-obvious reasons? Is the status of science jeopardized when politics are too openly contested within academia? Which understanding of science allows for such a separation? And again the question of Coats: Why is it that most think tanks tend to promote pro-market policies? Why is it difficult to imagine Marxist think tanks – or, at least, why do I hesitate in calling them "think tanks"? Does the existence of think tanks show the secret alliance between scientific monism and neoliberal hegemony?

To be sure, there is no reason to take to the streets because of the ideologies of think tanks. It is their job to do politics. But the institutional separation of theory and political utilization shows at least this: in order to arrive at a political claim from economic theory, another practice is needed that is not or cannot be part of the official ethos of an economic scientist. In order to make a career as an economist in one of the Cato-Centre-Institutes, other skills are needed. The existence of think tanks shows the ambiguities in the relationship between economic theory and the political sphere. From this point of view, it is no longer surprising that academic economics to some seems politically irrelevant. So it must in order to maintain its academic status. No wonder that "economists' specialized knowledge is used to support already-decided-upon positions, is ignored, or is used in a 'cover your backside' role", as Colander complained (1991: 20). It cannot be any other way, for in politics, epistemic authority is instrumental for power. It did not take Foucault to tell us that.

Why People Employ Economists and Why Others Would Do as Well – Occupational Discrimination and Specificity of Skills

At the other end of the spectrum of the profession are graduate economists who got a job outside of academia without ever having done academic research. Their economics degrees paid off in one of the countless McKenzies, Deutsche Banks, local governments, trade unions and commissions, public offices, or, if lucky, in one of the IMF or World Bank offices, or even in the Council of Economic Advisors. The certificates of these economists make them appear skilled, either for administrative, advisory, bureaucratic, or managerial activities, or for a job as a number cruncher in General Accounting Offices. These economists are not the enactors of scientific authority, but they profit from it. They may have received their job because Micro-Macro is written on their diplomas, but the on-job training they receive provides them with most of the skills actually necessary. Their job activities are usually far removed from economic theory. If they use economic models, they provide a rather symbolic input in a process that is dominated by those who persuade rather than "know" – lawyers.

What happens when trained economists enter the market and try to sell their expertise? What kind of intellectual changes do they undergo? Does their economic training contribute to their professional ethos? Barely. The moment of graduation as an economist is for most the moment they realize that that university does not provide a professional education. An economic degree serves as a signaling device of thinking in economistic terms, not as document of approved skills (see e.g. Choi, in Coats 1996: 110). Rather than the skill of economic theory (being able to solve the equations and coming up with a narrative about them), other interests one acquired elsewhere are decisive in order to succeed in the interview – mostly those related to the institutional knowledge of a certain branch. Students are advised to forget the curriculum as soon as possible rather than asked to bring new theoretical insights into practice. Most students already become aware of that during their studies, oriented toward other institutions, doing internships and the like. There is a clear gap between the skills needed to pass one's exams and the skills required to be a successful economist outside academia. "Application, communication, and instruction are much more important for success in their jobs than for success in graduate school, while the opposite is true for mathematics" (Stock and Hansen 2004: 270, see also Coats 1993: 618 ff).

This insight seems trivial. But for students who enter economics it may not. A quick look how the Mission Statements of some average-ranked universities try to recruit students for their education suffices to show how the image of economists as professionals is exploited. (Harvard-Chicago-Stanford, of course, does not need Mission statements; they list their Nobel Prize winners.) According to the mission statement of the University of Georgia, economics

> endeavors to prepare the university community and the state for full participation in the global society of the twenty first century (…) The department strives to (…) provide students with an excellent education in economics as a foundation for general understanding of the world, successful undertaking of business endeavors, and advanced studies and research (www.uga.edu).

Going through economics is an important contribution to society, and moreover is well paid, the student may believe. Thus, does it pay off? What difference does it make to be a trained economist or trained in any other science? In monetary terms, it does pay off, at least if one graduates quickly (Siegfried and Stock 2001). "Many of those employed in business and industry, however, were not satisfied with their jobs despite receiving significant salary premium relative to academics" (Siegfried, Stock 2004: 272, see also 1999). Why?

The skills economics students acquire are not specific. Whether one has a degree in economics, sociology, statistics, or any other discipline in the social or even human sciences does not make a great deal of difference.

> Even now, when the level of technical sophistication of PhD students from so-called elite graduate departments has reached heights that to older professionals seem dizzyingly abstract, there is often still no clear occupational discrimination in the non-academic world between (a) those with advanced as

> well as undergraduate degrees, (b) those with joint or mixed rather than single honours undergraduate degrees, and (c) those in higher management or civil service positions with no formal training whatsoever in economics (Coats 1993: 399):

No wonder that in recent years some Wall Street firms hire fewer and fewer economists for jobs that decades ago were exclusively reserved for economists (Cassidy 1996). "Practitioners at the major international and foreign-aid agencies complain that recruiting is much harder than it used to be", the welfare economist Arnold Harberger reported (1993: 3). "Candidates for job are less in tune with policy analysis, and less able to answer relatively straightforward questions about it, than they were 15 or 20 years ago" (Ibid.). Economists in non-academic institutions are not able "to think on their feet" (Ibid.). The officials of the AEA, such as here Hansen, admit that "new PhDs are well prepared technically but limited in their ability to function effectively as professional economists particularly in the non-academic sector" (quoted in Coats 1992: 346). Economists learn professional skills "on job" because their academic knowledge does not "suffice as a foundation for claims to expertise in non-academic settings" (Coats 1993: 399). Thus, a degree in economics is for most a "similar intellectual requirement as the skill of writing beautiful poems among applicants for public servants' careers in China" (Frey 2003*).

Although there is a distinct education for economists quite different from other social sciences, market forces apparently annihilate economists' specificity. Economists are either employed as number crunchers next to statisticians, mathematicians, operations researchers, or other engineers, or as piecemeal researchers and advisers next to political scientists, sociologists, and lawyers – who dominate most of the actual process of economic and political decision making. In both cases it is difficult to see what exactly the comparative advantage of an economic training is. If there is income discrimination between economists and other graduates, then it is because economists may be less afraid of numbers, or simply expect a higher income – since, after all, they more easily accept being one of these-money-kind-of-people.

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In this chapter, I opened the black box of "the economist" and listed some of the institutions they rule, they dominate, they sneak into through the back door, and from which they would better keep distance. I did not present a unified image of the profession. The utilization of epistemic authority is unstable, in that it results in neither a clear scholarly nor a clear professional identity of economists. The least I can conclude from the multifaceted image I have drawn is that the social inclusion of economic science in economic service is not as trivial as 'economists apply positive theories to economic domains'.

A common feature I have observed is the separation of the making and the utilization of economics. The use of economics seems to exclude the economist. Be it that the proficiency of economic theory does not stem from an economic context but from other, mainly formal sciences, be it, as in applied economics, that economic theory undermines the perception of a distinct domain of application, be it that economics needs separate institutions in think tanks to utilize its policy research, be it the strict hierarchical organization of the institutions centred around MIT-Stanford-Chicago – social inclusion does not happen by means of economists themselves, but rather by means of others who know how to utilize economics. If economists appear like professionals or scholars, then it is not because of something specific to economists. The skills necessary for the utilization of economists' service are largely independent of the skills necessary for producing economic theory. Thus, the perhaps most important question of this chapter: How, then, can economists take *social responsibility* for their services if their utilization *necessarily* excludes their participation? Do the institutions of economics not stand on one foot?

(3) The Pedagogical Ethos

The profession of an economist requires social inclusion in economic services, which was the theme of the last chapter. It also requires the social exclusion of others from providing these services. This happens by means of education resulting in certificates. Given the not-so-trivial case of the service of the profession, the making of an economist becomes interesting. How do economists *re*produce? Why do young people study economics? And what happens to these motives during their study? How are students taught? And why do they graduate? Thus, what is it to become an economist?

Students of economics represent the vast majority of those who encounter economic theory – in a quite intimidating way. There are many more students who pass through the industry of economic teaching than those who will profit from their Micro-Macro certificates in jobs. Around a million U.S. students, as Klamer and Colander estimate, pass undergraduate courses in economics each year; 30.000 choose it as their undergraduate major; and around 1000 PhDs graduate, the majority of whom enter again into the chain of reproduction (1990: 7 ff.). Given the unclear prospects for becoming a professional, one may ask skeptically about their reasons for pursuing careers as economists.

> Tireless young economists trying to get into the profession write papers that will not be published or, if published, not read, spend money traveling to conferences where they will not be heard and sacrifice home and family to do it all (…). Some may ask why, why be an economist when the returns are so hard to detect (Klamer 2007: 13).

The critique of becoming economists deserves extra attention – all the more of if comes from the students themselves. This happened in 2000 at the *Ecole Normale* in France. Economics students wrote a petition for a teaching reform and triggered a movement that received some publicity: the *post-autistic economics* movement. Listen to the tone they adopted:

> Open letter from economic students to professors and others responsible for the teaching of this discipline: We economics students of the universities of France, declare ourselves to be generally dissatisfied with the teaching that we receive. This is so for the following reasons: (…) we no longer want to have this autistic science imposed on us. We do not ask for the impossible, but only that good sense may prevail. We hope, therefore, to be heard soon (in Fullbrook 2003: 13).

To such a desperate appeal to 'good sense', the community of economists was to some extent sensitive. The reason is apparent. As opposed to the layman's critique, or the critique of those complaining that economists paid by Cato support the Liberals, the student touches the *existential* interest of the economic profession. Without students, no offspring – without students, less funding. But since the students are *not yet* economists, one can easily downplay their critique by paying lip service to the demand for reform.

Students of economics occupy a critical point within the phenomenological coordinates of economic science. Since they have to pass from the world-for-everyone to the special world of economists, they are the actual critical character of the economists' world. Students stand amid the biographical aging of economists (teachers, too, have once been students), the history of economic science (the economics of tomorrow, if at all, will be written by the students of today), and the socio-historical environment of economists (the motivation of students to enter economics is rooted there). Students stand at the threshold of economics and other economic talk which triggered their first interest in economic issues. An actual experience of a period in which one's intellectual life gains shape describes the hermeneutic play of meaning between economics and the rest of the world – an experience that swings between excitement and disappointment, hope and doubt, effort and evasion. The disciplinary identity of economic science correlates here with the intellectual acrobatics the student is asked to perform, and which the French students described as the imposition of autism. Because of this critical status of students, it is worth devoting this chapter to the teaching of economics.

Notes on Literature

Since Klamer and Colander's study (1990) at the latest, academic teaching culture in economics has justly become a critical topic of research. In this landmark of skepticism, Klamer and Colander interviewed graduate students in the triad of Chicago-MIT-Stanford on the self-image they adopt while becoming economists. This study caused bigger waves than other similar critiques. The AEA appointed a *Commission on Graduate Education in Economics* (COGEE) that included Arrow, Blanchard, Lucas, Stiglitz and others who had assessed the need for reform in graduate teaching (Krueger et al. 1991, as a reaction see Colander 1998). Colander – only Colander – recently updated the survey to match his new views on the empirical turn, allowing for little more optimism (2000, 2005). Ever since this study, teaching is on the agenda in official commentary. In the orthodoxy, the empirical work of Siegfried and Stock needs to be mentioned (2001, 2004, 2007); and in the heterodoxy, the vivacious discussions surrounding the post-autistic movement (Fullbrook 2003: 45-107). One finds more research in the *Journal for Economic Education*, though less politically heated. Yet teaching culture was an active topic since the foundation of the AEA (Siegfried and Hinshaw 1991, Bowen 1953, Coats 1992). Since the AEA was launched, it has always concerned itself with teaching standards – be it in high school, undergraduate, or graduate teaching. As many knew before, teaching the youth is an effective way of building walls around worlds.

Students as the Critical Character between the Layman and the Professional – Understanding Generation or Authoritative Indoctrination of Economists

First year students decide on economics and against other sciences for reasons which are to a great extent beyond the control of the profession (except the promotional activities of economics departments, recruitment days and the like). Part of this motivation is naturally some kind of interest in economic affairs, which students know from their previous milieu, from public economic discourse, or from their first contacts with professionals. At this time students usually do not know whether they want to continue with academia or find a job in other public or private institutions.

A student's life can thus be described as the transformation of the interests, motives, and values that gave rise to the decision to study economics into those interests, motives and values that are necessary either to contribute to economics or to be able to expose oneself to one's fellows as an economist. The question 'What am I actually up to? – which describes, according to Husserl, the act of critique – is unavoidable and omnipresent in this transmission. Students are in this sense the actual critical character in the discipline: on the one hand, they have to accept the authority of their teachers, since teachers know what they themselves do not know. On the other, they also have to make sense of what is presented to them regarding their former ideas, expectations, interests, wishes, political and social identity, and so forth. Only so long as one is able to perceive some kind of integrity in these motives are students able to personally internalize the curriculum and then smoothly pass the point of deciding whether to continue with graduate school or go into other institutions. Otherwise, studying is like gradually building a conflict that will end in a state in which the student's past does not translate into a future.

In accordance with this critical situation, I can think of two stylized modes of becoming an economist drawn along the student-teacher relation. The first may be called 'understanding', the other 'authoritarian' – drawing in black and white my experience as a philosophy and economics student. An understanding teacher, first, tries to share his or her own interest in doing science, and thereby giving the students the chance to develop and articulate their own motivations in their own terms within the tradition of the discipline. Learning, hermeneutists would describe, amounts here to a continuous renewal of the interest that gave rise to the study of economics in light of the present course – a play of pre-understanding and project. The position of the teacher in the discipline, his convictions and research interests, are crucial for the student's learning process. Only within a close relation of teaching and research can there be a continuous history of economics from generation to generation. The "rules of the game" are not given, but are open to change for each new generation. Even if this process occurs by means of an "attack" "with ever-increasing violence" against prior conceptions about economics, as McCloskey puts it in her early textbook (1982: 3), students learn by being confronted with their prior conceptions rather than ignoring them. To become an economist, then, is to develop the ability to come up with, elaborate, develop, and defend an economic claim of one's own, and so to hand over the tradition by challenging it in light of present social life. Becoming an economist is to become an apprentice, to become well read and learned, and thus to become a scholar. Becoming an economist is to *grow into* economics. Practizing economics is in this case the *generation of economics*.

Alternatively, the teacher can rely on the students' ignorance and willingness to accept the authority of the teacher. The student is taken to be incapable of relating to the values of economic science for being not yet an economist. Becoming an economist, then, is to acquire the tools of research and to learn the rules of the game. Studying economics is preliminary to research. The acquired knowledge does not allow for making a claim, but for applying tools to a field, and for enlarging science. Because tools are general and independent from actual economic problems, the teacher's research is immaterial to the process of learning. Teaching and research are separate institutions, like tools are separate from their ends. Furthermore, there is a definite "core" of the curriculum, for which there are particular "pre-requisites" to meet, to use the words of the COGEE report (Krueger et al. 1991: 1052). The image of science as a 'body of knowledge' follows naturally. Practicing economics is then the *specialization* rather than generation of knowledge. The rigidity, and the costs associated with education produces a "deep hold" on students' mind, which keeps them on the reproductive track. Studying economics is less to grow into it, but rather to be *indoctrinated* into economics.

Hardly contestable, the prevailing treatment of students in western economic departments is of the latter kind. It may seem trivial, yet is by far obvious that

> based on their pedagogy, economists appear to believe that one studies core subjects to become socialized as an economist, then one masters several fields of specialization for teaching purposes, and finally one selects a problem in one of these fields in which to become a specialist (Weintraub 2002: 1).

In order to produce this image of economic knowledge, economic teaching culture is fully authoritarian – except some U.S. departments that lean their teaching on neighboring disciplines or on more heterodox traditions, such as UC Riverside, Notre Dame, and, of course, the New School (see for a survey Lee 2004). But the greater part of graduate students would still hurrah the following statement made by one of them 20 years ago:

> The first year seem to shape the rest of our career as an economist. It is really disturbing. We are moving into something but nobody really knows what it is (…) It's like being brainwashed. You are deprived of sleep. You are subjected to extreme stress, bombarded with contradictory notions, and you end up accepting anything (Klamer and Colander 1990: 28).

Becoming an economist is to forget what has motivated it. The technical efforts are so all-consuming that one looses one's breath. There is simply no more energy for the task of internalizing economics. Once one has passed the first obstacles, the costs will have been so high that some may become economists *in spite of* knowing better.

Let me list some evidence regarding the ruptures between higher education, graduate teaching, and professional life. When entering higher education, in the so-called "science of choice", as it is called on the Harvard homepage, there is usually not much choice. The first thing a student notices is a highly standardized curriculum. Courses are obligatory, topics are assigned, lectures are fully structured, and course objectives so narrowly defined that undergraduate teaching simply waits to be automatized (Sheflin 2008, Noble 2002). In the "pre-requisite courses" of mathematics and econometrics, students hardly find a material link to their discipline. Introductory economics courses leave no doubt that there is an uncompromising, analytic *core*: market theory, which is as free from its contextual meaning in a capitalist society as Euclidean truths are independent of the Greek state (are they?). This core relates in one way or another to the question of price determination, entails the principles of rationality and equilibrium, and produces the theoretical conception of "the economy".

Given the difficulty of finding one's personal access, it is hardly surprising that the general appearance of economics students on campus is marked by negligence and reluctance, which the COGEE report stated as a "a lack of creativity on the part of students" (Krueger et al. 1991: 1037). Regarding the relation of teaching to the initial motivation of the students, Amariglio and Ruccio doubt that there are "many economics instructors (…) even interested in asking their students what theories they invoke to understand or explain the economic dimensions of the society in which they live?" (2004: 260). They speak of learning economics as a "process of unlearning" (Ibid.), that is to say, forgetting the language in which one formerly articulated one's interest in economic issues. Asked what determines success in academia, as was one of the most quoted results of Klamer and Colander's study, there was almost unanimity that knowledge about "the economy" – that is, what students had in mind when first showing an interest in economics – is irrelevant: 85% in the first graduate year, 99% in the third year (1990: 20).

When moving into graduate programs, there is another gap to be bridged. Those students who graduate with a major in economics are more likely to do their PhDs in other than an economics department (Coats 1992: 344). Many graduate students thus have a weak background in undergraduate economics, though not to their disadvantage (Stock and Siegfried 2004, 2007). Graduate students in the all-but-dissertation stage, those who are supposed to leap the last hurdle from acquiring to applying tools, are more troubled with finding a research topic than financing their studies (Colander 2005: 177). And, although they passed technically high-powered courses, in their theses they end up not using more mathematics than they knew at the beginning of graduate school (Coats 1992: 347). As one of the graduate students told Colander: "The first two years were miserable [the Mas-Colell years, T.D.]. Now it is kind of fun and exciting, but I'm not sure the pain was worth it" (Colander 2005: 179).

Passing on to teaching and research, there is hardly any overlap. Teaching is institutionally separate from research, so that teaching is perceived as a loss of time instead of complement to research. This leads to the peculiar situation in economics that the standard body of economic theory being taught does not overlap with the way theories are used in actual research (Colander 2005). Graduating students, both those who leave the university as well as the ones who continue their research, face a strong discontinuity in their intellectual lives. Students applying for jobs outside the academy, as already hinted at above, rather signal the ability to acquire knowledge within a particular time frame – knowledge they should nevertheless forget as soon as possible. Inside academia, rather than being trained for a job as an economist, academic careers are favored over non-academic careers. In Klamer and Colander's survey, 53% of graduate students were planning to pursue an academic career, 33% were planning to go into policy-related work, 17% into business, 8% into research institutions, and 2% into journalism. Siegfried and Stock confirmed roughly the same numbers for 2002 (2004: 275). The hierarchy is also guarded symbolically. "I mentioned to one of my advisors last year that I might be interested in policy research, which I really am interested in, and she was definitely dismissive" (Colander 2002: 179).

If the training of an economist does not really make one qualified for a job either outside or, to some extent, inside of academia, then *what* makes an economist? Is the informal character training more decisive for one's career than the formal skills? I leave associations regarding what kind of people economic students are to the reader: Do you need to be one of these-money-kind-of-people? Particularly power-sensitive? Technocrat or liberal? Boring like bookkeepers? At the university where I write this thesis, not a day passes without the promotional activities of one of the McKenzies trying to recruit students, brimming with anti-academic rhetoric such as: "Success is a choice" – rather than a result of education or skill; 'Do you know the difference between theory and practice?', 'Do you have the entrepreneurial spirit in your blood?', etc. The sooner you choose, the better.

If I believe Borg and Shapiro's empirical study (1996), economics students are indeed a particular kind of people (see Frank et al. 1996 for more on the moral education of economists). Borg and Shapiro conducted a study on students' personality types, according to a certain Myers-Briggs-Type-Indicator as to their performance in an undergraduate macro course. Being of the ISTJ-type had a significantly positive effect on earning a good grade. These kind of persons are *Introverted* – interested more in concepts and ideas than in impressions. They *Sense* immediate reality and practical facts rather than the meanings of relationships and possibilities – the friends of reality. They *Think* in an impersonal way rather than weighing values – recall that "everyday economics" is rather personal. And they are *Judicious* in that they plan rather than doing things spontaneously. Thus, ISTJ-kind-of-people somewhat resemble the people who meet the axioms of rational choice – not because they are self-interested and greedy, but because they invest most of their meaning-labor into maintaining a neat line between matters of fact and matters of reason. Is becoming an economist to become (or, anyway, to already be) like these-kind-of-people?

Whatever kind of people, important here is the discontinuity between both the student's motivations for studying economics and his actual experience of going through the curriculum – and between studying and then either claiming to be an economist outside academia or doing research inside academia. Students live in two worlds: one described by the development of their interests and motives, the other described by the acquisition of the skills needed to pass exams. To put it more severely, learning to succeed in the exams without taking them seriously is the hallmark of the becoming economist. From first year courses until the graduate stage, those do well who are most willing to forget what they were initially up to. If those students who do not take their study seriously do better, what else than cynicism is induced by the teaching of economics? In response to the question of what he most disliked about graduate school, one student stated: "Being made more cynical than most would think possible. It is like seeing the inside of a sausage factory." (Colander 2005: 44)

There is a decisive building block of such teaching culture: *textbooks*. Rather than classical treatises or contemporary research, textbooks are by and large the only texts students need to read. How comparatively big is the business of textbooks, one can imagine, when considering that Samuelson's *Economics,* like any other intermediate textbook today, exceeded in only one year the entire lifetime sales of Keynes's *General Theory* (Lamm 1989: 104). From university to university in the western academic world, textbooks do not differ much. All economics students around the globe go through the same Varian-Kreps-Mankiw courses, and face ultimately the last line between studying and research: Mas-Colell, Whinston, and Green 1995. Again, I should mention the exceptions. Already in the 1970s, there had been attempts to write alternative textbooks, such as the *Anti-Samuelson* of Linder and Sensat (1977), updated recently as Stanford's *Economics for Everyone* (2008). Less biased to the left, but likewise with a pluralistic approach to economics, is the recently published *Economic Conversation* of Klamer, McCloskey, and Ziliak (2009).

Textbooks are a crucial means to the production of a core of economics, because textbooks as such do not reveal the position they represent in the profession. In this sense, Colander could state that the practices of teaching "play a larger role in determining economists' methodology and approach than all the myriad papers written about methodology" (Colander 2005: 175). Remember what Thomas Kuhn once said about the role of textbooks (1970 [1962]: 136 ff). Kuhn argued that textbooks normalize science in that they make scientific revolutions "invisible". They neutralize the different pasts, different paradigms, and different questions that motivated science within one well-ordered, ahistorical body of knowledge. Textbooks recondition science in such a way that the contested contexts from which research initially stems is *reformulated*, or better: substituted as the uncontested core of a discipline. Textbooks

> need to be rewritten in the aftermath of each scientific revolution, and once rewritten, they inevitably disguise not only the role but the very existence of the revolutions that produced them. (...) Textbooks thus begin by truncating the scientist's sense of his discipline's history (Ibid.: 137).

Because scientists socialize in their discipline by means of textbooks, Kuhn developed a strong skepticism about the beliefs scientists hold about their disciplines, and, for that matter, about the philosophers of science who comment on science as though it had been inscribed once and for all in the textbook of Truth.

Samuelson has received the Nobel Prize precisely for that. He has "simply rewritten considerable parts of economic theory" (Nobleprize.org). His textbook, *Economics* (Samuelson 1948) meant the breakthrough of the authoritarian teaching culture. It appears now in its 18th edition. It set the standard for generations of following textbooks. Only a little historical knowledge suffices to see that he did not merely "rewrite" "the same" "in a different style", but that he fostered a different image of economic science that is informed by the formalist revolution of the 1950s. Samuelson made neoclassicism Keynesian, and vice versa – which was inconceivable in the 1930s. In 1998, Samuelson had a second look at the book:

> With the objectivity that 50 years of perspective brings, I examined it minutely. To my surprise, it read much better than I could ever have suspected. No wonder it was an instant bestseller, which set a new pattern for all the late 20th-century economic textbooks. For, as I had almost forgotten, it was not merely the first text to bring effectively to beginners macroeconomic modeling along Keynesian and post-Keynesian lines. It was that (1998: 352).

Samuelson's textbook wiped out effectively the difference between Keynes's economics and the neoclassical tradition with the help of formal sophistication. Textbook economics since 1948 is constituted by the continuous deprivation of the historical environment out of which research evolves. In today's textbooks, there is nothing left that could remind to any environment of research, which is certainly true for the textbook admired and even mystified by my economic instructors, Mas-Colell et al. 1995 (see next page).

How I Did not Become an Economist – the Beauty of Perplexity, the Skill of Question Begging, and the Interpretive Indifference of Economics

Let me recall some moments from my days as an economics student (1998-2003), for I conceive of them as typical experiences that give rise to the complaints like those of the French students. Moreover, to depict those days reveals my "preconceptions" of economic theory. I

Contents

Mas-Colell, Whinston, Green 1995. *Microeconomic Theory*. London: Routledge

This 1000 pages textbook is presented to students as the ultimate hallmark between studying and research, which happened to me in the first lesson as an undergraduate. It is commonly used as the standard reference in graduate teaching. Studying the book is like chasing a carrot on a stick bound on one's back. The book is always a step ahead, and each step one takes, complicates the next step to be taken. Such exercise is very effective in producing the impression of an insurmountable core. Although I belonged to the first generation of students who had to study this book, I never even considered if there ever has been another generation that has *not* studied precisely the same. Informative for those who are not familiar with economics, and in reminiscence of the days others went through that book, I re-printed the full table of content.

What appears like the eternal core of economics, is the result of a row of historical truncations. The misunderstanding began more than 50 years ago with a publication of a textbook in mathematics that was decidedly aversive to all applications in science, the *Elements of Mathematics* of Nicolas Bourbaki. Although without this book Mas-Colell could not have been written, there is hardly any economist who ever heard of that name "Bourbaki", let alone read it – apart from Gerard Debreu. Because of Bourbaki and Debreu Mas-Colell is full of $x \in X$, *Definitions, Axioms, Theorems, Propositions, Lemmas* – in that order –, the last of which students are allowed to derive in their problem sets.

At the core of the book is certainly *General Equilibrium Theory* (GET). Its position is peculiar. It comes *after* Game Theory that is presented like a special case of GET, as well as *after* Market Failure that applies only to Partial Equilibrium. Moral hazard, incomplete information etc. do thus not show the shortcomings of GET. This order would suggest that most of the research done today takes place on an analytically lower level than

Mas-Colell's GET. The reversed positioning of GET is only conceivable if one considers the history of mathematical economics all the way down to the different conceptions of mathematics of Debreu and von Neumann. This I am going to do in the third part.

Here the first lines of the textbook that already covers great parts of the history that led to it.

> A distinctive feature of microeconomic theory is that it aims to model economic activity as an interaction of individual economic agents pursuing their private interest. It is therefore appropriate that we begin our study of microeconomic theory with an analysis of individual decision-making (3).

If there is a credo of post-war economic science, this is it: microfoundation. As harmless such a statement sounds, its degree of implicity is astonishing. Is the student committed to a sort of individualism when he understands the use of the word "appropriate"? In which sense does the study of "interaction" presuppose the study of individuals? Why "private" and not "self-interested"? The student has no time to sinuate about these questions. After having gone through the following 1000 pages, the student may be inclined to believe that such study must be the last expression of the tradition of invisible hand theorizing – if only there is not another page or problem set!

The first and last message of the textbook is thus that everything what substantially can be said about economics is a matter of the modelling of the individual. Between "social order" and the analysis of decision-making are a bunch of theorems and lemmas, but no economics. Indeed, with unanimity (9 of 10) students, interviewed by Colander believe that the rationality assumption is crucial in economics (2005: 35). Rationality is the assumption which indeed most post-war economics jumps on – and all critique stumbles over. Is the only problem that economists are poor anthropologists?

It took me a while to understand that the order of analysis goes the other way around. The question of economic theory is not: Which social order follows if we assume that

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individuals behave like this or that, but: What do we need to say about individuals in order to speak about a social order at all? The less the better. Not that economists are poor anthropologists, but that they are not anthropologists at all. This is what Mas-Colell et al. prove page by page.

Today I also know that it was a very particular teacher who gave us undergraduates this book to read. I remember him saying that studying this book later will be an advantage for a career in the U.S. The authority of the book was reinforced by the fact that my teacher did never loose a word about his own involvement in the research that Mas-Colell represents. Only later during my historical work, I run again into his name. I learned that he himself even published together with the economist who brought the *Elements of Mathematics* to economics: Gerard Debreu. It was Egbert Dierker, a friend of Debreu. He did never give us students the slightest hint that could make us ask whether he represents a particular tradition. Mas-Colell et al. *was* economics for me. A lot had to happen that the contingency of my education could become clear.

Nevertheless, such is not only a personal episode that shows nothing but that I have an axe to grind with economic theory. I deem it to be the very principle of the constitution of economics since the formalist revolution: The problem of economics since 1945 is not that it is too formal, but that because of its formality those who originated it do not appear – even if they themselves speak up!

What we need in economics teaching is thus not more emphasis on the application of theory, or textbooks that are able to put the same theory in more intuitive narratives. Such only reinforces the blindness that constituted the power of Mas-Colell "mainstream". What economics needs is a teaching based on the history of economics, and the reading of economic literature. Then the size of the empty hole of economics since 1945, of which Mas-Colell is an expression, can appear in its full light.

will go through three moments of my studies – one at the beginning, one in the middle, and one at the end.

The task of the first lesson in microeconomics introduced firmly the style of teaching: Assume that $\succsim$ is complete and transitive. Proof that $\succsim$ is reflexive, $\succ$ is not reflexive, ~ is reflexive, transitive and symmetric. This is exercise 1.01 of Mas-Colell et al. Since it was mentioned that $\succ$ means "better than", the task was roughly to prove the following: if something is either better or worse than, or about the same as all the other things, and this is the case in a specifically consistent way, then something is always at least as good as itself, is never better than itself, and always as good as itself. Puzzling, not because such a task seems odd, but because I did not know how to make the proof. For the answer – proved by means of intuition – I received no credit. I was not told about the status of intuition in economics – in particular not about the values of a certain school in mathematics in France in the 1930s, which conceived of mathematical proofs as the "music of reason". But even my perplexity had its beauty.

At the heart of the course in microeconomics is certainly the proof of the existence of a general equilibrium, which graduate students have to learn year after year "as if a rite of passage" (Weintraub 2002: 183). For most students, even those who turn out to be academic economists, this proof is probably the most mathematically abstract piece they encounter in their careers. It is an indirect proof using a topological fixed-point argument that comes with the name of a certain Kakutani. Nevertheless – or just because it is a piece of math one never encounters again – it sets the standard of research. The message of the proof as presented in class is roughly that market equilibrium is "consistent" – whatever that means – as long as preferences and technology behave in accordance with specific conditions that are put in mathematical terms. To be precise, commodities need to be i) finite, ii) convex, iii) with a lower bound, preferences j) continuous, jj) strictly monotonously increasing, jjj) strictly quasi concave, and technology k) strictly convex. Therefore, only the preference relations are interesting in market theory, I thought.

Microfoundations are indeed a great topic of research that seemingly have some philosophical bearings. The rhetoric of that research, which most students learn early on, is taken from everyday philosophy of science. One wonders about "anomalies" in that the "assumptions" made about the "individual" do not fit "reality". Libraries are filled with discussions of such "anomalies", with which one could map great parts of the entire current discipline. Let me mention only one anomaly of "context dependency," that the young Amartya Sen discussed in the 1970s before becoming famous for his work on developmental economics (1973). We may reject the last piece of cake at dinner with our beloved, though we prefer the same piece when it comes along in the supermarket. No consistent preference can be inferred by those two actions. The unscholarly answer is to claim that the example is misplaced because the dinner is not a consumption phenomenon, but a love-scene. This would be a bad move, because we would accept the slippery ground of counter-examples. There may be a commodity we choose consistently in love *and* supermarket situations. Reasoning by examples is not what satisfies the intellectual wants of economists. The proper answer, instead, is to point to the definition of the problem, which leads to a "reformulation" of the "commodity space" – that is, keeping the *structure* straight and adapting the *interpretation* – the cake, in one case, is not the same thing as in the other. Problem "solved".

All students who understood what "U" for utility does *not* mean are able to give such an answer after the first year – however, with the murky feeling that "U" turns out to be an "elaborate pun", to use Sen's words (1973: 243). After such a lesson, the upcoming economist learns perfectly how to do question begging. Economic theory, as students know better than most philosophers of science, does not follow the logic of examples, of the general and particular, of the theory and its domain of applicability. Instead, it follows the logic of *structure* and *interpretation*.

At the end of my studies, the same lesson was affirmed once more. It made me believe that not only the teaching culture, but also economic research, amounts to a great extent to nothing but elegant question begging. My task was to write an essay on recent research in "bounded rationality", which is one of the official replies to the problem of "anomalies". The paper I had to write on, published in the *Quarterly Journal of Economics,* started with an entire opposite *intuition* of decision-making, in contrast to the received expected utility theory (Gilboa and Schmeidler 1995 – the authors being longtime friends with Mas-Colell). The theory tries to cope with the blatantly implausible assumption of expected utility theory: that people are able to consider the probabilities and weigh the values of all possible future states of the world. Not forward-looking rationality – "expectations" – but backward-looking rationality – "memory" – serves as the intuitive ground of their so-called Case-based Decision Theory. People do not anticipate the future, but recall whether there was a similar case in the past and act in accordance with what they remember – a difference that indeed makes the world for some evolutionary and institutional economists. In a reply to this theory, a sophisticated mathematician, Matsui (2000), designs a transformation algorithm that makes both theories analytically equivalent. All results can be generated by the opposed theory simply by *reinterpretation.* The sophisticated mathematician concludes that the difference lies rather in the "descriptive theory" that underlies the intuition of backward- and forward-looking rationality. Where could such a theory come from? And what kinds of standards are entailed in order for it to count as an economic theory?

Intuition and the skill of reinterpretation from one context to another seem to be a second, but also secondary source of economic theorizing. It is not part of the training of an economist. Instead, the message put across at the end of my studies was that even in the case of basic differences regarding intuitions of how people behave, there always could be a sophisticated mathematician who wipes out such differences. All kinds of behavior can be made consistent with a general equilibrium in markets simply by reinterpretation. "So the results keep flip-flopping, endlessly, pointlessly," as McCloskey commented (2002: 45). And it is precisely this that makes the ethos of the economist problematic: the *interpretive indifference* of their theories. Economists are *beyond* the common rules of the hermeneutic play of meaning.

With this indifference, the threats I followed in this part slowly come to merge. It is not the case that economic science is a particular, more founded, more objective way of thinking about economic life, but that it is a practice that is particularly indifferent to meaning. The interpretive indifference is not only a problem of how economists explain. It determines the attitude of economists to their discursive environment – their ethos. It determines their relationship with their students, in that their study appears to be analytic, rigid, free of history, and beyond non-scientific discourses; it determines their relationship with those who believe economists to be professionals in that their theories appear to be beyond political bias; and it determines their relationship with the general public, in that economists cannot address the concerns of you-and-me-and-our-fellows because of the anonymity of their talk.

It was probably experiences similar to those I have just described that gave rise to the French students' petition. The reasons they listed for being dissatisfied resemble common points of postwar critique of economic science. All reasons, I venture, relate to the interpretive indifference of economic theory: the students wanted to "escape from imaginary worlds", and thus come closer to reality – truly empirical work in institutional and historical contexts. They were "opposed to uncontrolled use of mathematics", and thus against model building as an "end in itself" that "leads to a true schizophrenia in relation to the real world". They were "for a pluralism of approaches", and thus more teaching oriented at schools of thought, against the "dogmatism" of one sort of economics, and against the "pretence of being scientific". They wanted the teachers "to wake up before it is too late", in spite of the "academic constraints" of the profession. The French petition was a call for economists to be *self-responsible subjects of their theories* and to stand for their profession – thus against "autism" (Fullbrook 2003. 13f.).

On the level of theory, all critical points in the petition are all too well known. The petition covers the full list of standard (not entirely consistent) objections against economics: economics makes unrealistic assumptions, disregards distributional and environmental issues, plays down conflict and class, disregards intrinsic values, is useless for politics and the people facing real-life problems, is trivial, tautological, unfalsifiable, too scientistic, spuriously authoritative, biased, imperialist, hegemonizing, reductionist, and, last, and perhaps the most valid yet subtle objection: too economistic. Although economists are all too familiar with such (contradictory) voices, not much has changed in the last, say, four decades. Already in 1971, Leontief, who acknowledged this criticism and also contributed to it, spoke in an ironic tone about the policies that try to respond to such critiques.

> Much of current academic teaching and research has been criticized for its lack of relevance (…). In a nearly instant response to this criticism research projects, seminars, and undergraduate courses have been set up on poverty, on city and small town slums, on pure water and fresh air. In an almost Pavlovian reflex whenever a new complaint is raised, President Nixon appoints a commission and the university announces a new course (Leontief 1971: 1).

The same as in the 1970s happened in 1991 after Colander's and Klamer's survey. The COGEE commission appointed by the AEA, on the one hand, acknowledged the "considerable scope of improvement in ensuring that student's knowledge of economic problems and institutions enable them to use their tools and techniques on important problems" (Krueger et al. 1991). On the other, they concluded that merely a greater emphasis on applications is necessary, though the principle structure of teaching can be maintained without a "complete overhaul of graduate education" (Ibid.: 1052). Similar reactions the French Petition earned. Although the report of Fitoussi for the French Minister of Education acknowledged the need of reform, other eminent economists, lead by MIT Olivier Blanchard, even wrote a counter-petition (Fullbrook 2003: 4 ff.)

This raises doubts about the very sensibility of economists towards critique. In this part, I gave several hints that this sensibility depends on the sensibility of economists to the utilizations, perceptions, and interpretations of their theories by their discursive environment. Only if the practice of economists is in some sense or another informed by their discursive environment, critique can find entrance in the discipline. Critique can have an effect only if economists are *responsive* to their audience – be it their indirect audience in public, their direct audience that pays, or their intimate audience that will replace them. However, the meaning of economic theory, I argued in various ways, is not constitutive for the ethos of an economic scientist. To the contrary, the meaning indifference grants economists social identity: be it the anonymity of their object in that it differs from the rest of economic talk, be it the unspecificity of the public service they provide, be it the authoritarian mode of teaching. The problem is not *which* economic theory economists pursue. The problem is rather that their theories do not commit them to a particular *ethos*.

This locus of critique, beyond the usual lament about economic theories, is reflected in the French Petition. What the students saw at risk in their education is their intellectual responsivity: "Most of us *have chosen to study economics* so as to acquire a deep understanding of the economic phenomena *with which the citizens of today are confronted*." (Ibid.: 13, *e.a.*) Economic science, the students repeated the common complaint that I imitated in this part, is not concerned with the experiences of the people who conduct an economic life. It is therefore insignificant. But the French students did not speak about this illusive thing, economic theory. They spoke about themselves. Their petition expressed that they are not able seeing their interest in economics addressed in the theories they are taught. This is the main point of their political slogan "autism": there is no context through which they could understand themselves in their engagement into economic science. And this I regard symptomatic for economic science as such: the students cannot make sense of themselves studying and at the same time being interested in economics. Economic theory does not allow reflecting on the motives that give rise to it, with which I arrived at a first formulation of the main argument of The Phenomenology of Economics.

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I began this part with the notion that economics is a particular practice as any other human practice. I supposed economists' special world could be described from the point of you-and-me-and-our-fellows as to determine their place in the world-for-everyone. In this fashion, I asked for the discursive ethos of the economic scientist.

What then is the economists' ethos? What makes them a discursively identifiable character? What makes them interesting listening and worth paying? Do economists inform the public about the reality it faces day by day? Does the economist provide a particular good that can be shared with others? Is it to give professional political advice based on economic theory? Are their theories utilizable for some practitioners to glimpse through the economic aspects of their work? Do economists provide a platform for those young fellows who show a special interest in economic talk in order to develop intellectually their understanding of 'the economic phenomena with which the citizens of today are confronted'? More generally asked, do economists help us orienting ourselves intellectually in economic life? Do they help us in the challenges we face in the now already fourth or fifth century of capitalism? In which sense are economists relevant?

The preceding chapters suggested that economists are largely evasive of such questions. It is impossible to present a clear-cut pragmatic analysis of the discursive place of economists in economic talk. Their practice cannot be judged on pragmatic grounds. Economists do not provide a sharp and distinct contribution like cobblers, medical doctors or lawyers or any other profession. Economists' epistemic authority depends on the *anonymity* of their "object", the *indirectness* of their impact, and the *indifference* to the utilizations of their theories. Concerning their public ethos, economists' authority requires to evoke an image of complexity that in turn requires abstaining from the interest of the public. If economists enter into seemingly pragmatic relationships with their discursive environment, it is not clear by virtue of which feature. Their contribution may not be specific to economists (like in the job market), may be due to the image that has been created around economics that corresponds hardly with their practice (social technocrats), it may be due to mere authority suppressing the rest of economic talk (as in economic theory, and in teaching), or it may be due to political leanings that would undermine academic institutions if too openly defended (as the case of think tanks shows). This analysis could lead me to conclude: what grants the economist an identifiable character in the world-for-everyone do not stem from scientific practices of economists. Did I perhaps even suggest that all discursive effects of economists even threaten the ethos of the economic scientist? Is the *only* way to gain discursive identity as an economist to be "irrelevant"?

Nevertheless, as soon as economists' authority does play out, it tends to support a particular politics. Since a couple of decades, this seems to be neoliberal politics. How this influence happens is difficult to trace. Between the claim to scientific authority and its political effects, there is like an invisible hand that leads non-partisan science into partisan economic talk. Economists toggle between political irrelevance and political bias. This toggling explains that both images of the social character of economists can prevail: useless for being two-handed, and not credible for being "hired guns". Economists' ethos is torn between professionalism and scholarship, between social technocrat and market ideologue, between preacher and educator, between savant and priesthood sworn. Being (politically) relevant and (politically) biased are two sides of the same knife that cuts the line between economic science and the rest of economic talk.

Therefore, seen in light of the world everyone has the right to claim intelligibility of, the practices of economists c*annot be trivially determined.* This is the source of frustration of many commentators of economics since more than a couple of decades, be it outside of academia in the bad press economics earns, or inside academia regarding the skepticism between Colander and the French students. If there is *common sense* about the identity of economists, it is not without a good portion of these skeptical images.

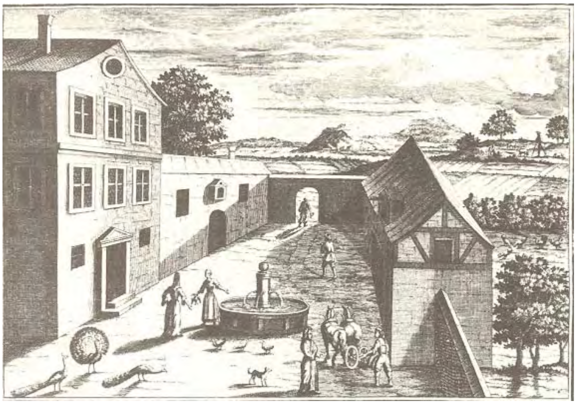
In terms of my guiding notion of the life-world, the problem is that economists stand outside the hermeneutic play of meaning that moves the rest of economic talk. It is not that economists inhabit their own special world, but they do not seem to live in *any* world whatsoever. For there is nothing that leads from the practical and moral interest of economic talk to the theoretical interest in economic science. Nothing leads from the word-for-everyone to the special world of economists. Economic science represents a rupture of the life world that no play of meaning could bridge. If economists' claims are nevertheless drawn into that play, it happens not by virtue of the economist. They enter the play of meaning only via *effects* for which they cannot take responsibility. In other words, the contribution of economic science to economic talk cannot be more than the misunderstandings they cause.

There is no reason, I announced in the introduction, to continue complaining about this lack of discursive integrity. I have promised to go beyond the genre of lamenting. In order to become critical in a truly phenomenological sense, I need to take a further step and give up the naivety of determining the *actual* ethos of economists. The critical question is rather: What could *possibly* be the ethos of an economic scientist? And this means first of all to ask: Has there ever been an integer ethos of economists? The question that needs to follow the lamenting tone of this part is thus: How did it come that way?

The image I presented in this part, I emphasized numerous times, applies to economics since roughly the 1970s. Since the 1970s economists and their institutions gained the shape as described in the preceding chapters. Since then, the critique of economics from the inside says that economics is politically irrelevant and does not relate sufficiently to real life problems, while, paradoxically, the critique from the outside says that economics fosters a particular neoliberal discourse. In order to assess both critiques, I need to adopt a different perspective. Rather than seeing economics in light of its discursive environment, I need to see it historically in light of the social history of European ideas. How did economists *themselves* gain a tradition through which they were able to perceive themselves as economists in the first place? How did it happen that at one point of history people began to think about economic affairs in a scientific way? How could this tradition become so powerful and at the same time so powerless as described in this part?

The further task is not to continue lamenting the crisis of economics. Instead, I need to understand the *genesis* of this crisis, that is to say, its necessitating forces. Only then its scope becomes intelligible. Precisely this was Husserl's motivation for a historical reflection:

> What we must do, however, in connection with our problem of the crisis, is to show how it happens that the 'modern age', which has been so proud for centuries of its theoretical and practical successes, finally becomes involved in a growing dissatisfaction, indeed must view its situation as one of distress (Hua VI: 342, E.: 294).



An *oikonomia* (here, a manor) as imagined by Franziskus Phillipus Florinus in 1702,
in his work *The prudent and righteous house-father: advices, teachings, and other considerations*, Nürnberg.

Part 2
History

A private ownership economy $\mathscr{E}$ is defined by:
an economy $((X_i, \underset{i}{\precsim}), (Y_j), \omega)$;
for each i, a point ω_i *of* R^l *such that* $\sum_{i=1}^{m} \omega_i = \omega$;
for each pair (i, j), *a non-negative real number* θ_{ij} *such that* $\sum_{i=1}^{m} \theta_{ij} = 1$ *for every j.*

"The economy" (here, a private ownership economy) as defined by Gerard Debreu in 1959, in his work *The Theory of Value: An Axiomatic Analysis of Economic Equilibrium*, Chicago

Part 2
History
Economic Science from the *Oikonomia* to "the Economy"

Economic science is there, firmly rooted in Western universities with considerable, and nontrivial influence on society at large. In light of such intrusiveness, is it not too far-fetched to envision a post-epistemic culture of economic talk that is free from those claiming scientific authority? If we stretch our historical minds, as I invite the reader to do in this part, and take economic science as a practice that is situated in a historical line of, say, Xenophon-Smith-Hayek, it appears, if not contingent, then at least historically late and peculiar, as Husserl said about science in general: "For the human being in his surrounding world there are many types of praxis, and among them is this peculiar and historically late one, theoretical praxis" (Hua. VI: 113, E. 111).

The reality of economic science is historical. Economic science happened. It happened to be a case of modernity. This occurrence I sketch with a large brush, outlining a *social history of the scientification of economic writings*. In order to understand economic science phenomenologically, we need to get an idea of the historical finitude of economic science: its concreteness, its richness, but also its contingency, and thus fragility. What, I ask in this part, is the historical horizon from which and in which the practices of economic science took root?

To answer this question, I need to adapt the notion of the life-world. It no longer refers to a priority of the world-for-everyone through which the special world of science is articulated. The priority of the life-world refers no longer to a primordial order of meaning within which economics "makes sense". Rather than an order of meaning, life-world refers to the *genesis* of meaning. In order to become critical and ask how economics *possibly* could be meaningful, I need to consider its *history of sense*. The history of sense, as opposed to the history of facts, is the history of economics insofar as it is its *own*. It is critical in this sense that matter is not a given order of meaning into which economics should integrate, but rather insofar as it concerns itself. Historical criticism is intimidating, since the basic phenomenon of history, according to Husserl, is "humanity struggling to understand itself" (Hua. VI: 12, E: 14).

Scientific practices have historical provenance. Only there do they find their *own* task rather than the task given by the present order of the world. The life-world in this part does not refer to a world one "has" as a given stock of meaning, but the world that is handed down as a historical task. The basic questions of the history of sense of economics, adapted from Husserl, are thus the following: How did economics come into being? What was it supposed to be? How has it given itself its tasks? And how have these tasks been handed over? How did it get where it is today? What brought it to its eminence? How did economics gain a history?

This history – how could it be else? – takes the form of a narrative. It begins, happens, and is open to an end. First, the scene: Why is it that in European economic writings there were not always people who claimed scientific authority? Thus, what is the *pre-history* of economics? Second, what gave occasion to, what was the need for, and what kind of contests accompanied the initiating scientification of economic writings? Thus, what was its *Urstiftung* (institution)? Third, in which way did this instituting motive inform its further development? How did one act upon this initial motive? What is it to "hold in grasp" the past of economics? How did its instituting motive develop over the course of its historical modification? How did economic science grow up and grow old? What kind of "sense-modifications" has it experienced? Thus, how could it settle as a *tradition*? Third, how about today, and what could we expect from the future of economic science? If there once was a rising need, could there perhaps also be a decline in need for scientific authority? In what sense is economic science historically finite?

How far, then, is it necessary to stretch our historical minds? Certainly further than Adam Smith, but, I propose, not as far as pre-European cultures. Economic science is a case of *modern* history. Political economy, under the name of which economics came to be known in late 18th-century England, emerged in an atmosphere beyond the point of return to the cyclic comforts of premodern times. It emerged at a time when European man was pulled "ahead of himself", to recall Heidegger's word for the *Verfallenheit* of modern man (1962). Modern man strives, as though he never wanted to arrive anywhere else than at its (never-quite-yet) modern state. Modern man is liberated from the erroneous oppression of the forces that prevent him from looking forward. In the spirit of striving, modern man was no longer reminiscent of the Aristotelian flourishing being. Instead of a being in the midst of the forces that pull down to the ground of earth and push up in the divine sky, as man is represented in the premodern triad, a new modern triad constituted the forces of human life: science, technology and growth. All three took the challenge of liberating man – of liberating man from authority (be it epistemic or political), of liberating man from the narrow limits of means, of liberating man from the burden of needs, and liberating man from the burden of a dark past toward a bright new future. Modern times are promising times.

When stretching our historical mind to such extent that economic science can be seen as a phenomenon of modern life, to question its existence is all but far-fetched. What gave occasion to economic science was the promising role it could play within this triad of modernity. If there was something that necessitated economic science, then it was that it promised to close an otherwise open link between science and growth. Instead, its relation to technology – particularly political technology – rather calmed the enthusiasm about this alliance.

In giant steps I will go through this modern history of economics. I tell it in not more than three episodes that are necessary for a narrative: a beginning, a middle, and an end; an early, a high and a late modern economic science; its rise, its heyday, and its decay; the coming into being and passing away – the "biography" of economics, to borrow a historiographical notion from Daston (2000). The history of sense, from beginning to end, runs as follows: After having set the scene for premodern economic writings, the rise of economics encompassed roughly 200 years of British history, from 1650 to 1850. The time of modern economic science proper encompassed roughly 100 years of European history, from 1850 to 1950. Since then, the present situation in economics encompasses roughly the last 50 years of U.S. history. Today, I will argue, the need for pushing further the ethos of economic scientists fades away. At the end of this narrative, we will have gained a historical understanding of what remained a riddle in the last part: that since around the 1970s, inside the institutions of economics the greatest charge is the complaint that economics became (politically) irrelevant, while outside the greatest charge is that it became (politically) biased.

How did economic science intervene in modernity? I will view this intervention as the attempt to push a *structuralist* turn in thinking about the world in which economic life is lived: from the *oikos* to "the economy". I speak of a structuralist turn, as explained in the Preliminaries, in that in oikonomic writings the order of the oikos is the temporalization of economic life, while in economic science "the economy" is constituted by the structural effects of economic life. I do not speak of the *oikonomia* and "the economy" as two paradigms, because neither did oikonomic writings have a distinct discursive identity, nor could the structuralist turn ever be carried out to such an extent that there was a full-blown paradigm of economic science. Never in European history was there something that could meet the pragmatic criteria of a normal science. For it was always haunted by the tensions between the reality "of" and *of* science, as the following history will explicate. The tension between the reality that was claimed in science, and the reality of claiming scientific authority describe the rise and equally the fall of economics. It is in precisely this sense that the following phenomenological genesis is critical, because it shows that economic science never did grow into a full-blown "paradigm". It never found its way into its own, because its history is the oblivion of its own history of sense – the history of the oblivion of the life-world.

In order to keep track of this critical impetus, the reader may keep in mind the cornerstones of this narrative from the *oikonomia* to "the economy". First, scientific authority was claimed in response to the economic suspicion, diverting attention from the question: Who are You – Arguing This! Economists managed to avoid this suspicion by means of cultivating a level of reflection beyond the imposition of special interests: the structure of "the economy". Such is the nature of abstraction in economic science: abstraction from one's own interests. How, secondly, did economists do so? Mainly by means of ignoring their *past*. The increasing ahistoricism of economics amounts to a tendency toward *formalism*. These are the two cornerstones of the scientification of economics: the *oblivion of its past* is operational for the structural object of "the economy", on the level on which the imposition of the *economic suspicion* is impossible.

Claiming scientific authority, to be clear about my argument on this point, is nothing but cheating in economic talk. Scientific authority was never claimed for the sole sake of truth (which truth could that be?). It was claimed in order to avoid the issues others had. Economic science does liberate, yes, but it liberates into nothing. In other words, the scientification of economic writings was not a particular sense-achievement, but was the covering of past sense-achievements. It is this history that describes the "oblivion of the life-world" *as* the scientification of economics.

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Such an argument, it is apparent, requires a different manner of arguing than is common among historians of economics. Apart from my remarks on the historiographic approach in the Preliminaries, let me remind the reader that this narrative is not designed in order to uncover historical truth. On this ground, I had to add countless specifications that would prevent me from arriving at an image of economic science as a case of modernity. The narrative from *the oikos* to "the economy" is not written in order to inform economists about their past. But it is written in order to address the problem of today's ethos of economists.

The historical consciousness of economists today is so underdeveloped that most may believe that there always was and will always be a need for economic science. Conjectural and Whig history together results in an attitude toward history that can be paraphrased as follows: 'When men began cultivating the soil, resources were scarce, and so there was the need to form concepts of how to deal with that basic human problem. If at one point in time these concepts were rather underdeveloped, it was due to the wrong-minded moralists that dominated European culture for too many years, and caused too much inefficiency'. Instead, I turn this attitude around: Remembering the history of the scientification of economics is to remember what had to be forgotten so that economists could evoke such an image of its timeless need.

The need for a Big History of economics is this: The history of the scientification of economics is the history of how economics forgot its history, a history of the decreasing degree to which historical reflection matters for science. A history of the scientification of economics is thus not a history of facts, but a history of the *historicity* of economics: how economics gained history. This purpose indeed requires an alternative narrative to the presently dominant narrative which allows the profession to be indifferent about its history – roughly speaking, the 1770s Smith classical – 1870s marginal revolution neoclassical – 1970s getting real again – sort of narrative of economics that was codified as the Smith-Mill-Marshall-Samuelson tradition (SMMS). The history of sense, alternatively, is rather of an MMM kind: Mun-Marx-Marshak.

There is an obvious misunderstanding in writing a social history of economic ideas that I had better anticipate at this point. What else could the oblivion of its history mean other than that economists have forgotten they are the children of *capitalism*? As in capitalism one too easily forgets that there was not always capitalism, economic scientists tend to forget that there was a time they could not have found a job. Is it this "best guarded secret of the profession", as Heilbroner and Milberg called it (1995: 113) that makes historical reflections for economists so awkward, at least in comparison with the splendor of announcements of the following sort: 'There is Economic Theory, the Problem of which Is: Price-Coordination.' Is this not a definite sign of the completion of the scientification of economics that this secret does not threaten or even challenge the ethos of economists? No economist needs to think about the historical genesis of one's ethos – therefore, economics is a science?

Do I thus write a materialist as opposed to an idealist history, to recall two somewhat dusty historiographic categories? Did history make economics or did economics make history? If I associate economics with capitalism, pointing to all the historical dirt on which economics was built, what is the difference from a Marxist materialist reading of economics? Do I aim at explaining economics via the history of capitalism insofar as capitalism required an epistemic instance of justification? Is it this, to be critical in a Marxist sense, unveiling the material base and thus the interest that informs the scientific claim to objectivity?

Why should I not associate economics with the great virtue of the enlightenment: the belief that reason, science, and the good life do not contradict each other? Why should I not associate economics with the belief in freedom, truth, and the moral law that come "from within" man instead of from beyond human bounds? Should I not focus, as historians of economics are inclined, on how economists were appealed by Newton's, Bacon's, and Descartes's metaphors for knowledge? Shall I celebrate how economists contributed to the success of modern science, and even ended up walking a line between "peace" and "physics" in Stockholm – only a couple of years after they helped out in Los Alamos? Are economists members of the modern liberators of man, or of those justifying the decay of modern man? More moderately asked, how do social history and the history of ideas relate in the following phenomenological narrative of economics?

Factually speaking, the social history of the scientification of economics certainly encompasses both great virtues and material dirt, visions of truth and ideologies using truth. Economists were both liberators and smooth talkers for modern man. In 17th century England there were both – new material problems and new worldviews of mankind, and they both came together in the political discourse of the time. Economic science is both the product of modernity and has informed the perceptions of it. Having said that, the historical approach of the oblivion of the life-world does not require distinguishing the material from the spiritual. To the contrary, I ask in which mode intellectual values are responsive to their concrete times. The two ways of understanding the history of economics are rather part of the *contest about its identity*, and thus constitutive of its social history. The idealist tradition represents the belief in the (social and political) relevance of economics, while the other materialist tradition represents the charge of its (social and political) bias. If I had to choose between the two historiographic paradigms, I could not even pose the question of history of sense.

The social history of the scientification of economic writings as the oblivion of the life, to come back to the seed of this chapter, is the tendency toward indifference to both (idealist) vision and (materialist) bias. The evasion of the ideological suspicion, I am going to illustrate numerously, happened at the cost of its modern vision. These costs are manifest in the tendency to formalism insofar as it serves as the last ground on which the scientification took a stance. To forget the life-world means that the contested line between vision and ideology disappears – that the claim to science neither responds to a vision nor to an ideology. In other words, it has always been an advantage in economic writings to say less, to merely indicate, not to spell out, to leave the meaning of one's claims to others. Such an attitude could be exploited as epistemic authority. Thus, a historical misunderstanding leads from the *oikonomia* to "the economy": that there is something to say in economic science.

(1) The Pre-History

In European history, there was not always an economic science. But there were always economic writings. And these writings had all but epistemic concerns. Among the first were Hesiod's poem *Works and Days* (1973 [700 BC]), and Xenophon's Socratic dialogue, the *Oeconomicus* (1923 [360 BC]). The literary genre they represent is that of *manuals*. It lasted until the rise of economic science. Most *oikonomic* writings, popular by their very nature, were manuals for how to run a house, how to till the soil, how to practice one's craft properly – that is to say, how to be cultivated. The main concern of these writings were the classification and description of all sort of (productive and reproductive) works that correlated with different professions and with specific social relationships, be it between the *oikoi* with one's fellows, or the relationships within the *oikos* with those who could not take care of themselves (children, women, slaves, and animals). Around 1700, one of the last revivals of this genre of oikonomic manuals was the so-called housefather literature.

While dealing with money was considered a necessary part of running a house insofar as there was surplus that could be exchanged, *trade* was not considered a profession proper for reasons central to any genealogy of economic science. Only late in early modern Europe, after merchants gained more and more social recognition, can we observe how the Xenophonian tradition of manuals also included manuals for traders. One of the most popular of these manuals was Jacques Savary's *La Parfait Negocian* (1993 [1675]). It was translated into most European languages throughout the 18th century. It included information about weights and measures of goods, exchange rates and fairs at different cities, rules on bookkeeping and letter writing, and an abundance of local customs of various European merchants. Such writings made the merchant appear skillful, learned, and thus honorable – as though trading were the same as housekeeping (see Cipolla 1994).

In premodern times, I explicate in this preparatory chapter, there was no need for epistemic authority. As manuals, these writings were not in the realm of the contestable, that is, not in the realm of actual public discourse. Manuals were kept in private at a secure place for the case one does not know what to do. They did not contribute to the premodern culture of knowledge. There was nothing to claim, but only to *instruct*. *Oikonomic* writings did not demand a specific interest, but were popular writings for all who have to take care of themselves. Oikonomic writings are for this reason best-suited historical sources of the life-world of premodern Europe. The *oikonomia*, one may say, *is* the life-world to the extent that it is the locus where a correlation of life and world is achieved. Perhaps the very idea of *everydayness*, of man "having" a world roots in these writings how to cultivate the world. For this reason, phenomenology may appear to some like a retro-philosophy, though it also can point to the particularly European origin of a natural conception of the world.

Two Basic Tones of Premodern Economic Writings: Instruction and Moralizing – both Prior to Epistemic Concerns

The pre-epistemic position of economic writings is perhaps best reflected in *the* text of *the* philosopher that set the tone of most other economic writings, namely Aristotle's *Politics*. If economics was taught (rather than be instructed) in early modern universities, it was Aristotle's *Politics*. Economic matter was not actually part of the political discourse, but it was *preliminary* to it, a preliminary to the question of the perfect state. "The household is antecedent to the city, and more necessary," Aristotle wrote (2000: 160). The reason is simple. Only insofar as someone is able to care for himself one can speak of human beings as political beings, and furthermore as beings capable of a virtuous and epistemic life. This preliminary character is the simple key to the premodern conception of economic life and writings. A "political economy", for the premodern world, is a contradiction in itself (though, for a factual history, economic policies can be observed, see Booth 1993: 55 ff.).

The instructive tone of economic writings, however, could not always be maintained. When it came to the issue how to deal with money, with traders, and in particular how to deal with those who trade with money, the usurer, clamor was great. Money, trade, and usury represented the greatest intellectual challenge premodern economic writers knew. Money and those dealing with it made economic talk contestable. Only there did one need actual authorities beyond mere instructors: moral authorities. After Aristotle's discussion of usury, this authority has been for most of Europe's history the clergy.

The most famous of these authorities have been the many Augustinians, Thomists, Franciscans, who moralized on the misuse of trade and money, down to jurists like Carolus Molinaeus (1500-1566), who began to turn more liberal about the use of money (see extensively Langholm 1992). In the 13th century, *Summae* like that of Thomas were the writings where the clergy reflected on the virtues and vices of economic life. They were used as manuals for confessors (Le Goff 1988: 12). Most efforts went into questions like that of Thomas's "Whether a Man may Lawfully Sell a Thing for More than it is Worth?" (1924 [1265]: 53). What for us seems like the condition and definition of the use of money – why else should someone sell anything at all? – prima vista for most clergymen was simply a contradiction *in se* – where should this "More" come from? It was not until 1850 that the moralizing on the use of money disappeared entirely from economic writings. Until Jeremy Bentham's attempt to convince Adam Smith that the interest rate should flow freely, no economic writing could pass moral muster without discussing the limits of trade usury (see Persky 2007).

Neither the instructing Xenophonians nor the moralizing Augustinians – even if the debates were deadly serious – claimed epistemic authority. It was not how things *are* in economic life that was interesting, but how they are supposed to be, and what to do if they are not as they are supposed to be. One never appealed to the authority of science, but rather to common sense, traditions, one's experiences and those of others, learned practitioners, and, in the moment when quarrels came up, religious virtues. To recall these writings is not to suggest that economics "originally" and therefore somehow "actually" is a practical or a moral matter. Rather, in order to get an idea of the historicity of economic science, we should come to acknowledge that there was no need for epistemic authority in oikonomic writings. Thinking about the Greek notion of the *oikonomia* in relation to the modern "economy", as one of the few social historians who did argued, "invites us to remove those very modern blinders that cause us to see the economy as a discrete set of phenomena with theoretically distinct borders (Booth 1993: 2). Before there was economic science, economic writings did not have a strict discursive identity because economic life itself was not worth quarreling about. It rather had to be done. And if things got shaky, one had to appeal to those other than economic authorities.

In Xenophon's genre, there was no reason to distinguish between the moral appeal to a virtuous life and the "scientific" appeal to experiential and expert knowledge. Seeds must be in the soil in spring because one's ancestors did so, because God told us to do so, because this is what the good diligent farmer does, because otherwise the neighbor would think we are lazy, because the Prince needs his share, *and* because otherwise there is no harvest in autumn. All good reasons to actually do it. Necessities are not contestable. Premodern economic writers, as Appleby wrote in his classic study, "enjoyed a freedom from a consensus on the meaning of the evidence. They lacked a paradigm" (1978: 21). Nobody claimed epistemic authority since everyone knew that economic life is only preliminary to actual matters of debate. Those who did care about truth were free from economic worries. Economic life, short and simple – how could it be of epistemic concern as long as it was the condition of epistemic life?

The pre-epistemic character of economic life had to do with its, say, *metaphysical embedding*. Economic life was meant to be the manifestation of a transcendent order. Economic order was given *to* economic life. Economic order was handed up from the bottom of nature and down from the moral sky. In this respect, economic science is clearly a child of the enlightenment: only after those other authorities grumbled could economic science take root. To say the least, scrutinizing the "causes of growth", as Adam Smith later would do, hardly made sense in a society that ran *cyclically* around some kind of transcendent order – be it the Greek cosmos or the Christian divine realm. If economic order comes from the stars running smoothly in circles, how could there be a perception of such a thing as "the economy" that could be subjected to causes? "The very meaning of 'economic' would be unintelligible outside capitalism", as Heilbroner and Milberg acknowledged (1995: 111).

The history of the emergence of "the economy", as is my general outlook, describes the history of the phenomenological becoming of economics. As before there has been natural science only natural philosophy could claim authority over the physical constitution of the world, so in *oikonomic* writings there were authorities other than science. As in natural philosophy metaphysical and theological principles like "One", "Being", and "Becoming" described nature, so was *oikonomic* order a manifestation and reflection of another order of higher dignity. In the same sense as the theoretical perception of nature replaced truth as the virtue of epistemic life with truth as the object of referential claims, reference to "the economy" allowed scholars to think about something beyond one's immediate human condition when engaging in economic talk.

The Metaphysics of the *Oikonomia*: The Calender. Economic Life and the Appropriation of World

What, then, was the metaphysics of oikonomic writings if not reference to "the economy". The task of oikonomic writings was to determine the way in which the transcendent (divine or cosmic) order was manifest in the oikos. The guiding clue as to how this happened was *time*. The oikonomia is an order of time. To know the oikonomic order was basically to know *the calender*. From Hesiod's *Works and Days* to almanacs still used today, oikonomic order is the order of days, weeks, months, years, and generations. Oikonomic things are things to be done at specific times, over and over again. Here some lines of Hesiod:

> The eighth and ninth, of the waxing month, are good for men to do their work, and very fine are the eleventh and the twelfth; those days are good for shearing sheep and picking fruit. But more outstanding is the twelfth, for then the spider floats on air and spins her web in daylight, and the Knowing One collects her stores. Upon that day a woman should set up her loom, and push her work ahead. Avoid the thirteenth of the waxing month for sowing; it is best for setting plants. Plants do not prosper on the midmonth sixth, but it's a lucky birthday for a male (…) (Hesiod 1973: 84).

In the oikonomia things are determined by the time they need to be done. "The economy", instead, being essentially an order in itself and beyond all particular conduct of economic life, is not a temporal, but a *structural* order. The rising paradigm of "the economy", therefore, must have taken place by means of the *neutralization* of time and the *ossification* of the oikonomia as a structure. The passage from the social order of the oikonomia to that of "the economy" is a passage from time to structure. This passage was less an event *in* history, but rather an event *of* history, a change of the historicity of economic life. My interest in this part is less the events that led from the oikonomia to "the economy", but the changing perceptions underlying economic talk insofar as they amount to a neutralization of time.

One of the historians of the Annales school, Le Goff, put this neutralization in the spotlight of the rise of modern life – in particular, in that the merchant demands the same place in professional orders as the economist. They both came to wear mechanical watches.

> For the merchant, the technological environment superimposed a new and measurable time, in other words, an oriented and predictable time, on that of the natural environment, which was a time both eternally renewed and perpetually unpredictable (Le Goff 1980: 35).

The changing perception of time has often been described as that of time becoming an infinite continuum rather than a cyclic renewal. Such perception of an ever-continuing time was vital for one of the constituents of the modern triad: growth. While in *oikonomic* writings time passed cyclically, in modern economic writings time passes steadily – a single line merging into the horizon of growth. The future becomes the object of projects rather than something for which one needs to be prepared. The principal and often also first line in modern economic writings is this image of a mankind in a continuous growth of cultivation: conjectural history that spans one arch from the so-called savage to modern man – economic teleology. Though Adam Smith doubted that mankind's situation really improved, he also believed that nature had rightly imposed that deception on mankind, for it keeps people busy.

> It is this deception which rouses and keeps in continual motion the industry of mankind. It is this which first prompted them to cultivate the ground, to build houses, to found cities and commonwealths, and to invent and improve all the sciences and the arts, which ennoble and embellish human life; which have entirely changed the whole face of the globe, have turned the rude forests into agreeable and fertile plains, and make the trackless and barren ocean a new fund of subsistence, and the great high road of communication to the different nations of the earth (1976b [1759]: 183f.).

What Smith celebrates as the achievement of a growing mankind (to turn forest into fertile plains), in Xenophon's tradition simply had to be done – day by day, year by year – *before* one could speak of a mankind proper.

To approach economic history – and for that matter, the history of economics – as factual history would be to misperceive this fundamental change in the perception of time. For only *after* the structuralist turn toward "the economy" was a sequence of irreversible events conceivable, which could relate like cause and effect. In the *oikonomia*, there were never irreversible events, but always a next round in which one got punished for the mistakes of the last, and hoped, God willing, to do better in the next. Economic history did not have to be written, but had always already been written in the calendar. There was no economic history before economic science evoked the opposition of theory and history. This is not to say that the change I consider did *not* correlate with a factual history. However, this factual history does not provide a clue as to its meaning.

In order to illustrate this calendar principle of oikonomic writings, consider, for example, one of the last pieces of *oikonomic* writings, taken from a short-lived genre of the early 18th century: housefather literature (1660-1730). In the midst of the years when European economic history crossed the threshold of modernity, this literature once more provided voluminous manuals for the aristocrat's household, often titled *oikonomia*. The period falls in the immediate dawn of cameralist writings that emerged in the 18th century (for example Johann von Jüsti, 2008). Cameralist writings were manuals for civil servants of the royal chamber, too, but they were already written within the mindset of 'counting the economy'. The housefather literature illustrates the edge between premodern and modern economic writings.

Franziskus Phillipus Florinus alias Philip von Sulzbach, a protestant priest, wrote two in its time popular, more than 1000 pages heavy pieces of this genre, one for the landowner and another for the aristocrat. The former was published in 1702, *Der kluge und rechtsverständige Hausvater: Ratschläge, Lehren und Betrachtungen*. Franziskus mentions as a first order rule of the household the notion of a "house clock". The rule describes what is required "to live in order with" the world (which is another meaning of the Greek word οικοειν).

> The household ought to be kept in proper order. Order is like a house clock, to which everyone ought to conform by going to sleep, waking up, eating, drinking, working and carrying out other affairs. What can be done at night and under the roof in bad weather, ought not to be done in bright and nice weather outside the house. The daily distribution of ordinary business ought to be in front of everyone's eyes displayed, as it were, on a board how to conduct the day, hour for hour. Then work goes ahead easier than if everything takes place in confusion and disorder (1988 [1702]: 41*).

Franziskus expresses here the *basic economic sense* that things belong here or there and have their time to be done. Human beings are economic beings by means of their sense of being-in-order-with-the-world. That this order is designed in such a way as to require the least effort –

that is, an efficient order, as the Paretean historian may interpret the second and last sentence – is not part of this economic sense. Franziskus' first principle does not describe how the burden of economic life can be avoided or minimized, but calls for carrying and enduring this burden "in posture". The economic sense of propriety and belonging is manifest in this "posture": the willingness to stand the persistence of the world and to endure its duration. The oikonomia represents a duty rather than something to economize. To put it in phenomenological terms, order referred to the sense of having as a task the correspondence of life and world, not the minimization of the burden of economic life in an already well-specified world. In this sense, one spoke in the stoic tradition of the verb *oikoiein*, referring to the act of appropriation. The oikos is the world to be appropriated. In the oikos, like in "the economy", everything is connected with everything, but as the result of appropriation, not as the result of disregarding our sense of propriety (which I ascribed in the preceding part to present-day economists). Both a phenomenological critique of economic life in markets as well as an economic critique of phenomenology had to begin at this point (see *Supplement 1*, Henry 1994, Düppe 2008).

This notion of economic life as the sense of propriety and the task of appropriation could be fleshed out along three distinctions, which describe the economic human condition, as perceived in premodern times. First, *needs* – associated with concrete material objects, the threat of nature, the necessity of work, and the virtue of sedulity – condition the life of *desires* – associated with the "actual" human life: moral, political, religious, and epistemic life. Needs are met in the world, and desires grow through the world; they flourish, to use the key term of Aristotelian ethics. Only those who were first good economists could be truly human, which included, for example, full citizenship or credibility as an orator. Even until the 17th century, a condition of being part of the scientific community was possession of a particular amount of wealth (Shapin 1994: 48 ff.). Economic life was *preliminary* to "actual" life.

Second, means – associated with tools, goods, and all practices of production – are valuable only in light of ends. Only those who knew what was good could know what was good for what. The capacity of seeing means in light of ends was called *phronesis*. Phronetic reason *embeds* the economic into "actual" life. Third, private life – associated with self-care, the family, and other dependants – was the condition of political life – associated with speech, discourse and conflict. Only those who could take care of themselves could have conflicts and relate to the foreign. Only those who did not have to be concerned with their economic lives from morning to evening were free men. They had *leisure* at their disposal. "True wealth, in this sense, is freedom from the necessity of labor" (Booth 1993: 41). Economic life *enabled* "actual" life. In this threefold sense of preliminary, embedded, and enabling, economic life demanded a life beyond itself. It was never fulfilled in itself, and was thus finite.

Economic life is constituted by these three basic experiences: needs and desires, means and ends, and private and political life. All major issues of *oikonomic* writings refer back to these terms. Economic talk, premodern *and* modern, I venture, is identified as "economic" by reference to such experiences. They allow economic talk to be concrete. If there is no reference to them, we do not identify a speech as economic. To trace the genealogy of these distinctions would be a work on its own. Concerning economic science, I deal with these experiences in a moment when they lose their hold on theoretical practices. Economic science formed its discursive identity at the cost of the perceptibility of these experiences. Needs loose their

FRANCISCI PHILIPPI FLORINI

Serenissimi ad Rhenum Comitis Palatini Principis Solisbacensis P. in Edelsfelden & Kirmreuth,

OECONOMVS PRVDENS ET LEGALIS.

Oder

Allgemeiner

Kluger und Rechts-verständiger

Haus-Vatter/

bestehend

In Neun Büchern/

Deren Erstes handelt von dem allgemeinen Grund/ worinnen die Haushaltung bestehen soll: Nemlich von des Haus-Vatters und der Haus-Mutter Pflicht/ von der Ehe/ der Sorge für die Kinder beyderley Geschlechts/ auch des Gesindes/ Gebühr gegen der Nachbarschafft/ Gutthätigkeit gegen die Armen/ Erkänntnus des Rechts/ der Artzney/ des Gestirns/ und der Bau-Kunst.

Das II. Buch von dem Bau-Wesen/ und denen darzu gehörigen Materialien/ als Holtz/ Steinen/ Ziegeln/ Sand und Kalch/ von denen zum Bau erforderten Metallen/ Bestellung der Handwercksleute/ Stärck- und Vestigkeit/ Bequem- und Zierlichkeit des Gebäues/ vom Grundgraben und Unterbau/ von denen Mauren/ Verding- und Eröffnung derselben/ von Dach und Feuer-Mauren/ etlichen Vorbildern der Gebäude/ völliger Fürstellung eines unmangelhafften Meyer-Hofs/ Bräu-Hause/ Maltz-Tennen und Dörr-Stuben/ von Wein-Obs- und Oel-Pressen/ Cisternen/ Quell und Brunn-Stuben/ Wasserleitungen/ Wasserfang/ Schöpff-Brunnen/ grossen Pump-Werck/ von Hand-Roß-Mahl-Zain- Schleiff- und Säg-Mühlen/ Feuer-Sprützen/ Feldmessen/ Marck- und Gräntz-Scheidungen/ Visiren und Sonnen-Uhren/ item was bey Erkauff- und Verpachtung eines Guts zu beobachten/ von Witterung der vier Jahrs-Zeiten/ denen Winden/ Sternen und Cometen/ samt einem Haus-Calender/ was in jedem Monat das gantze Jahr zu verrichten.

Das III. Buch von der Wirthschafft in denen Städten/ Dörffern und Höfen/ vom Acker-Bau/ Verzäun- und Versicherung der Felder/ Gärten und Wiesen/ von Aeckern auch darzu gehörigen Werckzeug/ von Dung- und Bauung der Felder/ Besaamung allerhand Früchte/ und wie man den Saamen fruchtbar machen könne/ Erkänntnus der Erden/ Verbesserung derselben/ von Hanf/ Flachs/ Taback/ deren Zubereitung/ auch Wässer- und Wartung der Wiesen.

Das IV. Buch vom Garten-Leben/ Gärten/ dem Gärtner und dessen Zeug/ vom Grund und des Gartens Eintheilung/ Umgraben/ Mistbeeten und Saamen-Bewahrung/ dessen Aussäung/ Umsetzen und Begiessen/ von der Baum-Schul/ derselben Ordnung/ unterschiedlichen Arten des Peltzens/ Versetz- und Wartung der grossen Bäume/ von Wildlingen/ Stein- Kern- und Stauden-Obs/ wie solches abzunehmen und zu bewahren/ auch die Bäume vor bösen Zufällen zu beschirmen/ vom Weinbau/ wie ein Weinberg anzulegen/ von Fechsern/ unterschiedliche Art dieselbe künstlich zu peltzen/ ingleichen vom Hacken/ Pfählstecken/ Anleiten und Entblätterung der Reben/ Abnehmung der Trauben/ Keltern des Mosts und Bewahrung desselben/ wie nach der Leese der Weinberg zu tractiren/ Abbindung des Geschirrs und wie mit dem Wein im Keller umzugehen/ unterschiedlichen raren Wein-Künsten/ von der Waldung und Holtz-Wachs/ wie solches mit Nutzen abzugeben/ vom Bechhauen/ Kohlen- und Rus-Brennen/ und was sonst bey dem Wald zu beobachten.

Das V. Buch wie eine Stutterey anzulegen/ Stutten/ Hengste und Füllen zu warten und zu erkennen/ von Belegung und Eigenschafft der Pferde/ Zäumen und Beschläg derselben/ deren Artzney/ von der Viehzucht/ des schmalen und Fasel-Viehs/ auch Hünern/ Enten/ Gänsen/ u. d. gl.

Das VI. Buch von Seiden-Würmern/ und völliger Abhandlung der Seiden/ biß zu deren Verkauffung/ von Bienen/ derselben Wartung/ vom Honig und Wachs/ wie solches zu bleichen/ von Weihern/ wie selbe anzulegen und zu besetzen/ von Fischen/ deren Unterschied/ Art und Eigenschafften.

Das VII.. Buch vom Brodbacken/ Mulzen/ Bierbrauen/ unterschiedliche Künsten/ das Bier gut zu erhal-ten/ Pichung der Fässer/ vom Schlachten/ Fleisch dürren/ Saltzen/ Bleichen/ Zubereitung allerhand Getrancks/ Thee/ Caffee und Choccolata/ auch denen Handwerckern/ die zur Wirthschafft nöthig sind.

Das VIII. Buch von der Anatomia, Erkänntnüs der Kranckheiten/ und dargegen dienlichen Artzneyen/ bey allerhand sich ereigneten Zufällen/ samt einem Anhang bewährter Haus-Mittel.

Das IX. Buch bestehet in einem kurtz-gefassten Koch-Buch.

Ferner sind alle obige Bücher und Capitel mit Rechtlichen Anmerckungen auf allerhand vorfallende Begebenheiten/ versehen/

Durch Herrn Johann Christoph Donauern/ J. V. D. Hoch-Fürstl. Nassauischen Rath/ des Heil. Röm. Reichs-Stadt Nördlingen Consulenten.

Welches nicht nur allen Menschen insgemein/ sondern auch allen Amtleuten/ Pflegern/ Kastnern/ Cent-Grafen/ Verwaltern/ Schössern/ Voigten/ Richtern/ Kellern/ rc. nützlich und nöthig ist.

Durchaus mit schönen und netten hierzu dienlichen/ sowol eingedruckten/ als Folio Kupffern versehen.

Mit Röm. Kaiserl. Majest. und Ihro Churfürstl. Gnaden zu Mainz allergnädigsten PRIVILEGIIS.

Nürnberg/
Franckfurt und Leipzig/ In Verlegung Christoph Riegels.
Gedruckt bey Johann Ernst Adelbulnern. An. 1722.

Gm

Franziskus' oikonomia

The drawing depicts how Franziskus, a not-too-gifted painter, imagines the order of a proper estate or manor – a "Meierhof", as it was called in feudal Frankish society. A "Meierhof" served as the administrative site that mediates between the earl and the dependent farmers. With this drawing we can intuit the metaphysical motif of the οικονομια that was valid for more than two millennia of European history. The oikonomia is the *reflection of a given order*. It was a primordial order, the first manifestation of an encompassing, transcendent order *within* the very locus we live. Oikonomia, phenomenologically speaking, is the world of concretization.

Let me go through this premodern imagination of the oikonomia by going through the table of content of Franziskus' handbook (left page). His "Meierhof" is a spatio-temporal order: the water well at its heart, its yard in pieces, the soil in parts, the fields surrounding. Rooms for all times: the dining room to eat, the library (tabularium) to read, the sleeping room to the right of the closet for the lord and left of the closet for the lady, a dormitoriolum for a nap, the parlor always warm, the "reeking-rooms" apart, well-watered and clean, an armanium to store weapons, a pinakothek for the prestigious stuff. There are barns for corn, an oven to bake, stables for animals, a garden inside the walls, and fields before the woods. All that is directed by the order of day and night, wind and light, weather and seasons (the Greek word oikoi also meant seasons). In the cycles of sun and moon, all moments of life pass between baptism and last rites. To know what to do was basically to know where the sun stands at the moment.

This reflection of order raises a major difficulty regarding the metaphysics of the oikonomia and the finitude of economic life that scholars have long discussed and I can merely touch on: how does the order of the oikonomia relate to the order of the polis? In which sense is political life "like" economic life? This was the opening question of Aristotle's politics: Is the politician an "economist of the state", as Plato has argued? Is running a state like running a household? Is the politician a special kind of economist, like all other professions? The question is fundamental to the extent that, if so, "political economy" could have been perceivable – but, if not, would amount to a sheer contradiction. The notion of the finitude of economic life suggests the latter, but the encompassment of the oikonomic order suggests the former. Is the metaphysics of the oikonomia ambivalent in this respect? (See Arendt 1958, Booth 1993: 37f, 55ff).

It is a misperception to think of the oikonomic order as a "model" for political life. It was one of Foucault's points that only in early modern Europe did the notion of a *unified* order between the governing of the soul and the governing of states and continents arise (2006). For this reason, one needs to be careful in identifying the oikonomic order with the romantic conception of an organic society. In an Aristotelian world, political order cannot be reduced to the same "principle" as economic order, and particularly not to a common structure, even if thought of as organic. The former may "reflect" the latter, but not in the way of two kinds being subsumed in one "genus". I would rather argue that economic life as a necessary condition *impresses* what it conditions. Political life, in other words, refers back to economic life by means of *being in debt*, and *thankful* to it. I could thus conceive of the political sense of justice, the scientific sense of truth, and the religious sense of transcendence as sensibilities that are different in kind, but nevertheless have their provenance in the economic sense of propriety.

But these are questions beyond my present scope. What is important here is to remember that it was impossible in premodern times to think of a common structure of the oikos and the polis, which would allow for the conception of a common principle. No *microfoundations* in Athens.

concrete, finite, necessitating character. In political economy they become the object rather than the condition of politics, while the striving of desires becomes the placeholder for all economic motivations. The phronetic relation between means and ends, at the latest since the marginalist revolution, is dissolved in the concept of instrumental rationality that is currently replaced with the acquisition of "information". And political life disintegrates as that which secures private life, rather than the other way around – which became the dominant political message since the 1970s.

This degeneration certainly has far-reaching implications for cultural analysis in general. My task here is to assess the contribution of economic science to the degenerating perception of economic life. The cultivation of the theoretical perception of "the economy", I am going to show, amounts to the same as the degeneration of the perception of economic life. Economic science, to be clear about what follows in this part, never promoted a new or different notion of economic life. If economists explicitly referred to such notion, it was always in the same terms as Europeans always did. However, this notion never informed their theories, and became ever more redundant to point out. The intuition of what it is to conduct an economic life is given to economic science. As a consequence, the scientification of economic writings results in a state in which economics is no longer identifiable as a part of economic talk.

Think for example about the decreasing number of explicit references to the acquisition of "material well-being". Who was the last economist who did so? Who would still do so today? While wealth, as Schabas has described this degeneration as denaturalization, once "was equated with "the fruits of the earth, and sea, with minerals, fish, and exotic plants" (2006: 2), Marshall insisted that "[m]an cannot create material things (…) he really only produces utilities" (Ibid.: 16). And that utility is a placeholder rather than a notion of economic life, Marshall's successors have made sufficiently clear. The increasing redundancy of expressing one's perception of economic life, I will show, describes the *oblivion* that accompanies the scientification of economics.

The Premodern Culture of Economic Suspicion: Paradoxes of Trade and Usury

If the rise of the theoretical perception of "the economy" goes along with a diminishing expressiveness of economic life, it prompts us to consider the possibility that economic science did not come as a response to a long-awaited problem of premodern economic talk. Then, it neither would have happened by means of a change in the sorts of problems tackled in economic talk, as Kuhn requires for a paradigm change. Economic science, instead, would have meant a calming of the virulence of the problems by which economic discourses were haunted before. Economic science would have enabled *avoiding* a pandemic problem. What could that problem have been that plagued the premodern world?

In accordance with the notion of a finite economic life subjected to a transcendent order, the plaguing fact was that the economist could be *merely* an economist, as though economic life were not *merely* preliminary and enabling, but All There Is. In these terms the culture of economic suspicion found its lasting premodern expression. Here are the illustrious lines of Aristotle's Politics that stamped this culture for centuries to come:

> Hence some persons are led to believe that getting wealth is the object of household management, and the whole idea of their lives is that they ought either to increase their money without limit, or at any rate not to lose it. The origin of this disposition in men is that they are intent upon living only, and not upon living well; and, as their desires are unlimited, they also desire that the means of gratifying them should be without limit. (…) [A]nd so there arises the second species of wealth-getting. For, as their enjoyment is in excess, they seek an art which produces the excess of enjoyment (1996: 24).

Next to the economist, there was a second way of conducting an economic life, an "unnatural" kind of wealth-getting: *chrematistics*. It intents upon "living only" instead of living "really". To condemn such people was particularly intricate because they, as "merely living beings", do not develop a sense for the good life in the first place. Due to misunderstanding the preliminary nature of wealth, there are some economists who get somewhat stuck between animal and human being, between the life of needs and that of desires. They meet their desires in the world, and therefore need always more. And the placeholder of desires that are met in the world is, of course, money. Hence, chrematistics referred to the greedy money-kind-of-people.

Chrematistic practices were ascribed in particular to two kinds of persons: first, to the trader, and more severely to the usurer. The trader and the usurer subverted the *oikonomic* order, for they did not fit in the categories designed for economic life. When "living only" is All There Is in life, according to Aristotle's categories, it is not clear what one actually does. If one is *merely* an economist, one does not really do anything, at least nothing of the status of human praxis. This we could call the ancient paradox of trade, which is the key for much of premodern economic literature: when dealing with money, one does not "really" do anything. Illustratively speaking, traders did not bring things *about*, but only from *here to there*. And the usurer, who lives on the trade *of* money, did not even do that! Moving things from here to there, the trader did not have an address, was not addressable, not reliable, and could not be made responsible. The trader, coming and leaving, is always already somewhere else when being asked, "What did you mean when praising this 'good' that turned out to be useless?" No reply – already gone. As loud as one may shout, the trader cannot hear it from the middle of the ocean. For this reason traders were excluded from civil law, and were not conceived of as political beings. The trader was like the cunning counterpart of *Hermes* in that he did not leave his message but took it with him in his jingling purse. Trade means silence!

The quarrels about the status of trade and usury were *the* locus of argument in all *oikonomic* writings. Those who wanted to show intellectual wit instead of merely giving advice had to argue why the usurer would not pass Peter's gate. That is, one had to interpret Psalm 15: "O lord, who shall sojourn in your tent? Who shall dwell on your holy mountain? He who walks blamelessly, …who lends not his money at usury." This literature encompasses more than a millennium of European economic writings, from Augustine, via Thomas of Chobham, Duns Scotus, down to Molinaeus, beautifully illustrated by Le Goff (1988), and exegetically analyzed by Langholm (1992). Bentham and Smith have contributed too (Persky 2007). Between them there is a whole industry of legal writings that designed economic policies excluding traders and usurer from the polis, from guilds, from the aristocracy, etc. If there was a premodern economic history, it was the history of the ostracizing of trade and usury. The success of economic science can be measured in terms of the gradual disappearance of this genre.

In order to seize how far-reaching the problem of trade was, let me compare the trader with the *idler*. As hinted at above, the imperative of *oikonomic* order was to be diligent. The trader who did not bring about anything was thus subjected to the same moral judgement as the non-economic character, the *idle drifter*. The arguments against idlers are crucial for a genealogy of economic science, for it represents a bulwark against the perception of the difference between consumption and production: idlers, just like traders and usurers, consume without producing. There are principally two ways of condemning the idle drifter, on consequential and inherent grounds. First, idle persons were blamed for the blatant reason that they did not do anything for their living and thus, sooner or later, needed to live from others. Having no work was, in some dark times, reason enough for the death penalty. In Hesiod's poem *Work and Days*, the moral denunciation of the idler reads as follows.

> O noble Perses, keep my words in mind, and work till Hunger is your enemy and till Demeter, awesome, garlanded, becomes your friend and fills your granary. For Hunger always loves a lazy man; both gods an men despise him, for he is much like the stingless drone, who does not work but eats, and wastes the effort of the bees. (…) From working, men grow rich in flocks and gold and dearer to the deathless gods. In work there is no shame; shame is in idleness. (1973: 68).

Idlers live at the cost of others, and thus annoy gods and fellows. Hesiod's tone is threatening. At some point, when it comes to a moment of scarcity – in winter, say – the paradox of eating without working will be naturally solved. At the end of the day, or at the latest at the end of the year, you will understand that only those who work can live with prospects. This sense of prospect, care, and anticipation is what distinguishes the economist from the non-economic person. To live meant to *do* something *for* life; life cannot be *merely* living.

Note that the Greek word for idler (ετοσιοεργοσ) did not only refer to someone who did literally nothing – the sunbathing idler. It also meant to do things *in vain* – that is, not obeying the instructions of one's manual. Was the idler, then, someone who wasted his work, who did not work *efficiently*? Not really, insofar the opposite of being idle was, in Franziskus' words, "unverdrossene Emsigkeit" – that is, assiduous, sedulous, diligent, not querulous, morose, and surly. In this sense, Franziskus' second rule of the house reads as follows:

> Idle drifting (*Müßiggang*) is a vice in itself detrimental and, for that matter, petulant, by which man himself makes his life sour. For it takes strong feet to support lazy days. (...) Who does not work, ought neither eat (1988 [1702]: 44*).

Being idle is to be dismissed *in itself*, on its own grounds. In idleness life turns against itself. Here "not working, but nevertheless eating" is not a problem solved by its consequences, but is unbearable in and of itself. And so the maxim of 'who does not work shall not eat' was as immediately understood, as production and consumption were two sides of the same act – like breathing in and breathing out. Eating is like the celebration *of* work, and work the assiduous preparation of that celebration. Eating without working and working without eating were not *actually* eating or working. For this reason medieval economic utopias such as that of *Cockaigne* were perceived as, rather than images of happiness, the horror of boredom (see Pleij 2001).

The trader and the usurer did not do anything, but could still live off it! Disturbingly, one could not condemn them on consequential grounds. Those dealing with money ate without working, and even ate well – more than those who work! This is the backdrop of the premodern "definition" of money: money is *sterile*: *nummus non parit nummos* (money does not reproduce), as Augustine translated Aristotle and others repeated over and over again. It was a sheer impossibility that money grows from money, and that one could get more money merely by exchanging things. Wealth that stems from trade or usury stems from nothing – a creatio *ex nihilo*, feasible only for God. Usury and trade meant a threat to the divine order.

One of the arguments against usury was, for example, that taking interests is like stealing – profiting from something that belongs to someone else. How come? How could the creditor be conceived of as stealing from the debtor? Is the debtor not the one who does *not* own the money? Yes, but owning money is not to own wealth, but the mere possibility of wealth. Saying that money is sterile, *it is never anything in itself*, but what one does with it. In modern terms, prices are relative prices, which is still today the fundamental intuition of all market theory. In the *oikonomic* tradition, it meant a practical imperative. In the same sense as Xenophon wrote that buying a horse while not knowing how to treat it does not add to one's wealth, so is money only money by means of its use (1923: 365). Those who do not use their money, like the trader and more clearly the usurer, may possess the money, but they do not have the right to it. Having money is to be indebted to someone else, not the other way around, as conceived of today – a document showing that someone else owes us something. Gaining profit without doing something for it is impossible (see Langholm 1992: 44 ff.).

Another example of the reversal of today's economic common sense is the conception of the relation of trade and exchange. Today we think of trade – selling in one place higher than paid in another place – as an institution of exchange, at least in an Edgeworthean and Wall-Streetean world. But not in premodern times. Exchange, just like consumption, was part of the, say, articulation and communication of production rather than a different institution. For exchange to be possible, one had to live in the same world (*koinōnia*), or at least in the same city. Otherwise it would have been impossible to agree on the value of things. Dealing with traders, who themselves do not bring anything about, how could he know what things are worth? He had to stay for a while in order to know that. Trade and usury only appeared to be exchange, but were actually stealing: giving the world away. The philosopher Duns Scotus (1265-1308) was one of the first brave enough to consider traveling, and the risks and hassles connected with it, as a justification for gains from trade (in Braeuer 1952: 29). One had to view the world inclusively in order to appreciate his point.

The usurer was considered an even worse case since he did not even travel. He gives money in cases of emergency and wants back not the same, but even more. The word *chre* in Aristotle's chrematistics also means emergency. Trading with money meant living from exploiting others' needs. The poor became dependent on usurers and therefore stayed poor for their entire lives, always kept at the edge of survival – just the opposite of a voluntary and just exchange. Because the violence exerted by usurers was so subtle and invisible, usury had a special status among criminal acts and had to be punished more severely. Carolus Molinaeus, for example, had to flee from the authorities after he wrote that usury is not unlawful per se, denying that there "was something peculiarly and inherently vicious about usury or usurious gains, more than in unjust deceitful sales, or other similar kinds of fraud" (1924 [1546]: 105).

Within these arguments against usury, we also find the guiding threat of all oikonomic writings, *time*. One of the clergymen wrote: "every man stops working on holidays, but the oxen of usury work unceasingly and thus offend God and all the saints; and, since usury is an endless sin, it should in like manner be endlessly punished" (in Le Goff 1988: 30). Usury did not fit into the temporal order of the oikos – the calender. Caesarius of Heisterbach writes in 1220 in his *Dialogus miraculorum*:

> *Novice*: Usury seems to me a very grievous sin, and one most difficult to cure
> *Monk*: You are right. Every sin has its periods of intermission; usury never rests from sin. Though his master be asleep, it never sleeps, but always grows and climbs (Ibid: 30).

An even more perfidious, but telling argument against usury is the following in a *Tabula exemplorum* of the 13th century. Ultimately, the judge became so radical that to deal with money was considered equal to stealing directly from God: stealing time. The unknown author argues along the lines of the sterility of money with a scholastic analogy of night/day and hell/heaven.

> Since they [usurers] sell nothing other than the expectation of money, that is to say, time, they sell days and nights. But the day is the time of *clarity*, and the night is the time of *repose*. It is, therefore, not just for them to receive eternal light and eternal rest (Ibid.: 41, *e.a.*)

Selling expectations, selling time, neutralizing the difference of clarity and repose, of being and sleeping, of light and night, meant a deadly sin. What Aristotle formulated as a matter of excess, is here spelled out in terms of a neutralization of recurrent time. In order to become moderate about the violations of His Law and thus allow for another mode of economic thought, therefore, time has to become mundane. Only after time is subjected to order rather than being itself the order of economic life could economic modernity take root. The order of time becomes ordered time. The continuous renewal of a cyclical time turns into a time that never rests, that is never enough, infinite – the time of *wanting*.

When considering the difference between premodern and modern economic writings (particularly its policies of exclusion) how could I silence the countless hells of oppression? If trade defined the boundaries of politics, these boundaries were of course politicized. The problem was that not only were traders ostracized for being traders, but that people became traders because they were ostracized. Those who had been excluded from the guilds and from social ranks in general could do nothing but trade, which, in turn, justified their exclusion. The sophistication of the literature on usury and trade only sealed their destiny. In this way, the premodern culture of economic suspicion falls together with the history of the many Diasporas, above all that of the Jews. In the late Middle Ages – which keeps us in the frame of temporal order – Jews/merchants were allowed to stay within the cities only from morning to evening. The time of sleep was left untouched by economic concerns (Cipolla 1994).

The vital question regarding the cultural origins of capitalism is hence the following: What came first? Anti-Semitism or economic suspicion? Do liberals tend to embrace the former, socialists the latter? This question was perhaps the most urgent until 1945. But economists, who gained a discursive profile by moving beyond the culture of economic suspicion, have never done so. Hence, to associate the history of anti-economics with that of anti-Semitism, as Coleman concludes his apology of economics, is, to say the least, anachronistic and ignorant of the social history of economics after 1945 (2002, 2003). Coleman should have made this point when reviewing Sombart's 1911 *Modern Capitalism and the Jew*. Now it is too late.

One of the great obstacles for developing a theoretical interest in economic talk was to become moderate about the ever more apparent violations of the laws restricting trade and usury. The tension between law and actual practice stamped European economic history throughout its early modern time. How difficult it was to move beyond the moralizing about usury we can imagine in this modest phrase of Molinaeus: "The necessity of allowing for some usury is similar to the necessity of using money, but not so great" (1924 [1546]: 118). What insecurity about an insight that is so obvious to us! Nevertheless, it took ages to overcome usury laws. Even in the U.S. one had to wait until Ronald Reagan. Only since then young students can accumulate debts to their hearts' content.

At the moment that one grants traders and usurers the slightest social recognition, the matter of debate changes. Then one runs into hair-splitting debates on where to draw the line between exchange and thievery, between a 5% and a 6% interest rate. Smith believed 5% traces the edge between good an evil, while Francis Bacon argued that next to the 5% for the public, another limit of 9% should hold for merchants. Others also set the limit at 30%. In this way, moral quarrels moved into the background of a new problem of exactness. How could one argue about numbers? William Petty, then, was able to make a *systematic claim* about the contested line between a reasonable interest rate and usury. He argued that the interest rate should be as high as the rent of land, using, as it were, an equilibrium argument (1899: 48).

When listing the disadvantages of usury, Francis Bacon shows a peculiar mixture of the premodern and modern conception of money, laziness, usury, and trade:

> The discommodities of usury are, first, that it makes fewer merchants; for were it not for this lazy trade of usury, money would not lie still but would in great part be employed upon merchandising, which is the vena porta of wealth in a state (2005 [1625]: 121).

The usurer is still perceived as an idler. But instead of being condemned, he appears in an equilibrium argument showing that a high interest rate leads merchants, who are the source of wealth, away from their honorable service to society. This neatly leads into the next chapter.

Only after the issue of interest-rates was no longer a matter of life and death, but one of cause and effect, could economic science settle. The increasing price level of the 18th century, the first economists argued, is not the result of the bad economic character who greedily charges ever more, but a result of the money supply – that is, the supply of metals such as gold. The public ethos of economists could strike root only after economic modernity was already on its way, in particular in England in the late 17th and 18th century. Moving away from the premodern world of economic writings, I now turn to the concrete historical circumstances where scientific authority was instituted: reformation England in the 17th century.

Summing up the pre-history of economics, between the *oikonomic* notion of an economic life that is prior and conditional for epistemic concerns and the dominance of the culture of economic suspicion, scientific authority was inconceivable. Within the resulting quarrels surrounding money a theoretical perception of "the economy" could not evolve. Economic science, as the preceding notes suggested and the next chapter will confirm, did not emerge as the solution to a long-plaguing problem, but as the *avoiding* of the culture of suspicion.

(2) The *Urstiftung* in the Late 17th Century

Economic science is a child of the scientific revolution, but not one of its makers. Economists never made science, but science made a new kind of economist. Economists used science in order to gain authority. They relied on the belief in science as practiced and celebrated elsewhere. The social history of the scientification of economics is largely independent of the establishment of modern science itself.

Instead, what economists had to put effort into, according to my approach, was the work of translation, mobilization, and yes, imitation of the science of nature as the "science of wealth". They achieved to speak about "the truth" of "the economy". Such was the *social* condition of exerting epistemic authority – *whatever* the epistemic grounds. The genuine achievement of the first economists was to subject economic talk to epistemic authority. This is more than a mere free ride on a new epistemic culture (or its application, for that matter). It was indeed revolutionary for European economic writings. Thus, I ask in this chapter, how did economists infuse epistemic concerns into economic talk?

The epistemic revolution in economic talk, broadly conceived, did not come out of the blue. In early modern Europe, there was a clear opportunity for the alliance between epistemic and economic life. Did science and growth not share the same affective grounds on which they could prosper? If so, science came to economic talk not because of its distinguished methods, but because of its affective make up – namely, *scientific optimism*. Scientific optimism was expressed in manifold visions of discovery, of liberation, even of eternal piece – but certainly of progress and growth. Science prospered because of the hopes that man could cross the hill of history and get rid of the heavy burden of his past. Economic writers of the 17th and 18th century could rely on a fresh and booming passion for a prospering mankind by means of science. The decisive aid to the economist's epistemic revolution was thus that early modern science had already been tinctured with connotations of "wealth". Bringing science to wealth was in this sense not more than revealing its underlying political vision. Hence a 'science of wealth' was suggestive to early modern man. Thinking of modernity in terms of the triad of science, technology, and growth, the science of wealth was possible because it promised to close the otherwise open link between science and economic growth.

On which grounds the first economists made their truth claims, and what kind of methods they applied were not the actual social obstacle. For how to get from the science of nature to a science of wealth was *not* contested. Issues one may have today about the philosophy of the first economists did not describe their concerns. The object of their talk was simply assumed to be lawful, "like" in science. Early modern economists hardly used the words "science", "method", "knowledge", let alone engaged in explicit conceptualizations of their discipline. Even Newton, let us not forget, did not hold an explicit philosophy of science. Neither did the first "great" economists between Smith and Ricardo lose much time discussing their ideas of science. Not until the mid-19th century, which marks the beginning of the second wave of the scientification of economics, have the grounds on which one claims scientific authority been questioned. Until that time the philosophical beliefs about the nature of science did not make the economic scientist.

Given the purpose of a social history of the ethos of the economic scientist, therefore, I do not need to reflect on the epistemic principles with which early modern economists expressed their scientific optimism. Be it the so-called problem of induction, or the adoption of the mechanistic world view, be it Petty's Baconianism, North's Cartesianism, Cantillon's conception of natural law, Turgot's positivism, or Smith's alleged Newtonianism, none of these philosophical issues are decisive for *The Rise of Political Economy as a Science*, which, for example, Redman discussed in precisely these terms (1997). For these issues the reader may rather consult other historians, such as Schabas and de Marchi (2003), who have spelled out the rich, intricate and often curious associations that were made between the political oeconomies of 17th and 18th century and the spirit of Newtonian science. These associations, as Schabas (2006) has shown, did not after all contribute to the rising perception of "the economy", which, instead, happened by means of what she calls the "denaturalization of economic order". If the concept of nature appealed to economists, then it was the nature of natural philosophy rather than of science. I thus do not consider the association of the science of wealth and the science of nature as the actual accomplishment of early modern economists. Instead, they accomplished to subject political discourse to epistemic authority. For the present narrative, the beliefs of early modern economists about science were not more than the scenery before which they gained a discursive identity. Until the mid-19th century, science was not more than a stylistic device – but nevertheless an effective and path breaking device.

The following remarks on the rise of economic science are neither on the philosophy of the first economists, nor on what they actually claimed. I rather ask what gave occasion to, and what was the need for scientific authority? In what kind of intellectual milieu could the ethos of an economic scientist gain ground? What had to happen to economic talk in order to open it to scientific authority? How did the perception of "the economy" qua locus of truth arise? Or, as also Mirowski wanted Redman to ask: "Why does it seem to matter so much to everyone from the eighteenth century onwards that they be granted the vaunted status of being 'scientific'"? (2000: 88)

This moment of the initiating perception of the need and opportunity for scientific authority I call, with Husserl, the *Urstiftung* of economic science. As the mere perception of an opportunity, this moment refers to a prenatal state of the actual claim to systematic, scientific knowledge. It concerns late 17th-century economists rather than the "thirty-year Boom" in the late 18th century, as Hutchison called the time of the Hume-Steuart-Smith's between 1746 and 1776 (1988). The latter had already incorporated a different discursive milieu from France. There scientific authority has been cultivated by economists such as Jean-Baptise Colbert and

Francoise Quesnay, for which the reader rather consults, for example, Peter Groenewegen's account of the rise of economics (2002: 48ff.).

The critical question regarding the *Urstiftung* of economics is in which sense it presentiates the milieu it was responsive to. Only if such presentiation took place, can I speak of the initiation of a tradition. If not, as is indeed the case, the following generations will have difficulties expressing their historical consciousness. In other words, when science came to economic talk, was it a revolution that could be celebrated over and over again? Or was it a violence or even trauma that had to be suppressed once and for all?

Notes on the literature

There are not many, but some substantial accounts of the epistemic revolution in economic talk. Most of the work on economic writings before Smith is filtered by what Smith said about his "forerunners". Since he reduced them to a theoretically fallacious equation of wealth and treasure, and the balance of trade doctrine (1976: 429 ff.), most accounts discuss this period in terms of what they have or have not achieved for economic theory – at least by many 19th- and early 20th-century economists who wrote about the origins of their discipline. I mention three historical sources: McCulloch (1825: 1-60) – perhaps the first historical account of economics – Ingram (1967 [1888]), and of course Schumpeter (1976). Two of the standard reappraisals of late 18th-century economics include Groenewegen (2002) and Hutchison (1988). The latter has downplayed any external motivations of the theory boom since 1850 (187), which, in turn, grants more importance to 17th century economist. The exegetically inclined reader may consult the most complete and advanced study in mercantilism, by Heckscher (1955).

For my purpose, I rather consulted "outsider" sources of historians of science, such as the classical study by William Letwin (1963), on which much of this chapter is based. Appleby's work has also been helpful, although he too runs into Smith's Trap of Whig by being blind to the instability of the theoretical perception of the 17th century (1978). For an insider who debunks these outsiders, see the young Mark Blaug who missed in Letwin's study the question "how realistic were either the assumptions or the conclusions of economists in the seventeenth and eighteenth centuries." (Blaug 1964: 113) The historian Michel Foucault, mostly neglected by historians of economics, presented a substantive account of the rise of modern politics, liberalism, and political economy of 18th century (2008, a similar argument in Firth 1998). The most advanced of the more recent work is that of the literary critique Mary Poovey in her *History of the Modern Fact*, which I invite all historians of economics to read. I also recommend the account of the cultural historian Agnew (1988), who showed how the theoretical perception of "the economy" came to be expressed first in 16th- and 17th-century theatre.

The Perception of "the Economy" as the Germ of Modern Life: "the Phenomenal Republic of Interests"

Cornerstone and stumbling block of the intellectual milieu of 17th-century economic writings is a notion that would haunt the scientification for centuries to come: the *interest*. It is crucial, since the notion of interests distilled the culture of economic suspicion in such purity that it could become the *object* of a new political: the Economic Political. Let me measure out this change in economic talk that I exhibit in this chapter starting from a simple comparison of Adam Smith's only lines on the nature of political economy with those of Aristotle.

First, as in Aristotle, economics remained a part of political science. Everything said in political economy was directed to 'the statesmen' – although the difference between science and (political) art already ranked prominently in France (Fontaine 1996). Political economy was, as it is still called in German, *Volkswirtschaftslehre*, or *Nationalwissenschaft*. Second, as in

Aristotle, political economy was that part of political science that is concerned with the "acquisition of wealth". But apart from these two commonalities, modern political economy differed in that economic life lost its role as the condition of political life. Economic life became the *object* of politics. This move is standard among the writers of modernity such as Arendt (1958), Foucault (2008), and Agamben (1998).

Here the few lines that Smith has spent on characterizing the nature of political economy.

> Political economy considered as a branch of the science of a statesman or legislator proposes two distinct objects, first, to supply a plentiful revenue or subsistence for the people, or more properly to enable them to provide such a revenue or subsistence for themselves; and secondly, to supply the state or commonwealth with a revenue sufficient for the public service. It proposes to enrich both the people and the sovereign (Smith 1976 [1776]: 428).

Recall that in Aristotle man can be a political being only insofar as he is able to take care of himself. In politics people cannot take care of each other because they need to argue against each other. Now, in early modern Europe, self-care is conceived of as something that needs to be secured from politics. The task of caring about oneself in order to become a political being turns into the political right not to be constrained in one's self-interest. The duty of self-care becomes the right to self-interest. Self-interest is not conditional for the public interest, but can contradict with it. Self-interest is a matter of excluding, not disclosing the other. Here, for example, in one of the frequently quoted lines from Smith's *Theory of Sentiments*: "Every man, as the Stoics used to say, is first and principally recommended to his own care; and every man is certainly, in every respect, fitter and able to take care of himself than any other person" (1976b: 219). The first sentence is in line with Aristotelian thought; the second would hardly make sense to Aristotle – as though there were a (political) choice of caring about oneself. In this manner, economic life became a prime concern of, rather than a condition for, politics.

This reversal has affected the political discourse to the extent that the consequences for economic interests moved to the fore. At the beginning of the 17th century, even the clergy adopted this new tone when speaking of usury: "To leave the proofs of the unlawfulness of usury to divines, (…) here is only to set down some arguments to show how great the hurt is it does to this Kingdom" (in Letwin 1963: 82). It was in this manner that *the oikonomia* was no longer a matter in accordance with which law one conducted an economic life (be it cosmic or divine), but could be conceived of as a locus of epistemic judgments: "the economy". Is the corn price high (and thus people poor) due to the tariffs of corn or due to the speculation of bankers? "The economy" becomes the locus of "veridiction", as Foucault discusses this new "sovereign". "The market must tell the truth (*dire le vrai*); it must tell the truth in relation to governmental practice" (2008: 32).

On this ground, Foucault has suggested an alliance between the rise of the 'science of wealth' and the rise of the new state – the nation state. The nation state required an epistemic instance in order to govern. Government from the late 17th century on, he wrote,

> no longer has a direct hold on things and people; it can only exert a hold, it is only legitimate, founded in law and reason, to intervene, insofar as interest, or interests, the interplay of interests, make a particular individual, thing, good, wealth, or process of interest for individuals, or for the set of individuals, or for the interest of a given individual faced with the interests of all, etcetera. Government

> is only interested in interests. (…) Government is now to be exercised over what we could call the phenomenal republic of interests. (2008: 46 f)

In the moment when economically determined interests become the objects of government, according to Foucault, an epistemic space between economic and political life is disclosed: a "phenomenal republic of interests". What else could that be than the object of "the economy"? For Foucault, changes in the political required political economy. For those familiar with his vocabulary: "it was political economy that made it possible to ensure the self-limitation of governmental reason." (2008: 13)

From Foucault's point of view, the idea that a sense for economic theory developed by means of abstracting from the social and political context is myopic. Such is one of the standard accounts of "mercantilist economics", presented for example by Appleby (1978: 26, 70ff). The point of Foucault and others, instead, is that this abstraction had its own political motives and also changed the political discourse. Economic theory, just by means of abstracting from the socio-political context, could serve as a means *in* politics.

In a similar fashion, Mary Poovey has emphasized the role of interests in her account of the rise of economics. Rather than an alliance of political economy and a new form of government, she suggested an alliance between political economy and modern science itself (1998). Scientific optimism had its source in the impression of dealing with a problem as though nobody else, including oneself, has ever dealt with it before – unbiased, "disinterested". Speaking "science" meant first of all to adopt a different, less heated tone of disinterest. This notion of disinterest, Poovey suggests, was only possible on the basis of the perception of "the economy" that economists cultivated.

> The modern concept of 'disinterestedness' arose in the second half of the seventeenth century, for not until society was conceptualised as a congeries of competing interests that lacked an institution capable of negotiating those interests was it possible to imagine a state of mind that might be called disinterestedness (Ibid.: 86).

Therefore, if I spin this threat further, the perception of "the economy" as the institution that is capable of negotiating interests was conditional for, or at least supportive of, the very idea of unbiased knowledge – and thus for modern science as such! Only in an economic milieu in which questions of veracity and mendacity were omnipresent could science flourish. No modern science without "the economy" – the primordial object of modern science! The perception of "the economy" was thus operational in moving from an image of intellectual life as the virtuous exercise of human reason to that which unveils referential truth.

For Foucault and Poovey the epistemic revolution in economic talk was thus crucial for both the transformation of the political and the establishment of science itself. For both, the theoretical perception of "the economy" represents the germ for modern life to prosper. The critical point is less, as Foucault continues, that the truth of "the economy" was nevertheless a truth that legitimizes, or hides power relations. More interesting regarding the ethos of economists is, as Poovey argued, that there was an inherent tension in the very project of a "science of wealth" for the economist. In Poovey's terms, the constitution of the "modern fact" is coined by the tension between the interest of recognizing something as a fact, and the recognition of the fact as something beyond interest.

> On the one hand, facts seem (and can be interpreted as being) simply the kind of deracinated particulars that Bacon claimed to value; on the other hand, facts seem (and can be said) to exist as identifiable units only when they constitute evidence for some theory – only, that is, when there is a theoretical reson to notice these particulars and name them as facts (1998: 9).

This distinction resembles the distinction between the reality "of" and *of* science, with which I opened the present exercise. Since the very beginnings of economic science, I will argue in this chapter, this tension was operational. The decisive question regarding the *Urstiftung* of epistemic concerns in economic talk is this: How could one theoretically perceive "the economy" beyond interests in such a way that it nevertheless could nourish both the hopes for political prosperity and the optimism of science.

The virulence of this tension increases if we additionally consider Shapin's account of the social constitution of truth in early modern Britain. "Disinterestedness" of scientists was not the absence of character, but was connected with a particular ethos. To appear disinterested, one had to meet the norms of a "gentlemanly conversation" (1994: 42 ff.). But these norms traditionally have excluded those dealing with money – the traders. As soon as the trader demanded political representation, the politics of scientific disinterest had to face its double morale. Although the idea of a science of wealth was thus politically suggestive, it was precarious for reasons that those who increasingly dominated economic talk – traders – did not meet the criteria of a gentlemanly conversation. The alliance between the disinterestedness of science and the perception of "the economy" as an institution beyond interests had to surmount the hurdle of a different perception of the trader.

Within these intermingled concerns for science, politics, and trade, the economic scientist had to find a place. In this central chapter, I will show how that happened. At the beginning of this early modern period, around 1650, economic discourses were still fully determined by economic suspicion. Every claim was virtually undermined simply because it was made by someone in particular. At the end of this period, around 1850, there was a full-blown science called "political economy", that had gained epistemic authority within a particular political position of free trade, saying: In the Name of Science: Laissez-Faire! How?

The Feeling of Economic Abstraction in 17th-Century Britain, and the New Epistemic Genres

Britain in the mid-17th century. In 1649, the Civil War between Parliamentarians and Royalists came to and end, followed by a short-lived government of Cromwell's commonwealth, and then the years of Restoration under Charles II. When in 1660 the Royal Society for Science was founded with Robert Boyle (1627-1691) as its leading figure, William Petty (1623-1687) as a Charter member, and Newton (1643-1727) as its rising star, people may have had Bacon's eye in mind, but they had the Civil War in their bones. Science promised a new conversational culture free from the old regime of ancient rhetoric – the reliance on authority, on Aristotle + deduction = truth. Science was modern method against ancient rhetoric, replacing autocratic talk with Observation and Measure. There certainly were great philosophical issues to be resolved in these days. Think, for example, about the difference between Descartes's and

Bacon's image of knowledge, or later between Leibniz and Newton. But the Royal Society was rather liberal in its philosophical politics. "To cast a mathematical mantle over a problem", Letwin commented, "was tantamount to solving it", (1963: 91). If I additionally remember Shapin, who showed that Robert Boyle was not at all very learned in mathematics (1994: 126 ff), I rather turn to the political environment of science, leaving their epistemic principles aside.

What were the issues of the new political discourse that brought economic issues to the fore? After the Civil War, the governance and land reforms of subjugated Ireland was one of the pressing questions. Often mentioned as a context for getting economic abstractions off the ground are the declining British sales of cloth in the 1620s, the abundance after the good harvest of the 1660s, as well as the recoinage crisis of 1680. Much political attention went also to the Low Lands, which had quickly become the economic elite in Europe throughout the 17th century. The rivalry with the Dutch remained a reference point for all political and intellectual discourses, before and after the Glorious Revolution of 1688. In 1651 the Navigation Acts granted England, but particularly the East India Company, a monopoly of trade. These acts led to the Dutch-English wars that may be called the first wars conducted solely for economic reasons (the first in 1652, the last in 1780). Though prospering only later in the 18th century, colonial issues of the occupied and the New World had, already in the 17th century, a great impact on political order. At the end of the period I consider in this chapter, in the first decades of the 19th century, Britain with its growing capital London became the economic head of Europe.

In this pre-industrial mercantile capitalism, when merchants dominated the political scene evermore, epistemic claims in economic talk settled. It was the time when politics gained its modern shape, when the *res publica* became to be referred to as the *commonwealth*. The liberation from authoritarian politics, roughly stated, came as the subordination of the political under the economic. The common wealth of England was still a matter of its land, but moreover of its "treasure", which in turn was a matter of international trade. Such became one of the doctrines assigned to late 17th-century writers. Until the mid 18th-century, the domain in which scientific authority could find expression was the *foreign trade* of Britain. The perceptions surrounding production, instead, were only crucial for gaining the first fruits of this new epistemic culture in the 18th century (Groenewegen 2002).

The object onto which scientific authority could be established was political. Rather than the sudden fascination of ontological transparency (as one may associate with the Royal Society), and rather than sudden technological and manufacturing innovations (as one may later believe when reading Adam Smith), science was first an opportunity within the political discourse. To write economics was to make politics. Economists were "partisans, fighting for particular policies; they aimed at persuading their fellows to act in certain ways rather than teaching them new truths" (Letwin 1963: 47).

The new genre of economic writings where epistemic claims could settle, was thus the *political pamphlet*. Sophistry in denunciating the usurer was replaced by emphatic pamphlets in favor of or against specific economic policies. The titles of economic writings changed from the preaching like John Blaxton's 1634 "*The English Usurer, or usury condemned by the most learned and famous divines of the Church of England*" to pamphlets like Ferguson's 1677 "*The East-India-Trade a most profitable trade to the Kingdom, and best secured and improved in a company, and a joint stock*" (both in Letwin 1963: 33). The audience of these pamphlets was the Council of Trade, Parliamentarians – that is either Whigs or Tories – and the King between Charles and the Georgians. The issues were exclusively daily, specific policies circling around the balance of trade, protection versus free trade, and exchange rates as in the recoinage crisis of 1696.

But perhaps the most important change was that the authors of late 17th-century economic writings were the traders themselves – simply inconceivable in the Aristotelian world where traders were not even allowed to enter the gates of the polis. Within the new political, traders gained ever more power *in* society, and thus claimed political representation. One of the earliest "unions" was that of the Merchant Adventurers, from which the East India Company later sprung (1600). The presence of traders in politics challenged the social conditions of epistemic authority. How could they possibly be credible? Skepticism was great, as the following words of an anonymous writer of *The Rich Cabinet* show:

> The Merchant is only traduced in this, that the hope of wealth is his principall object whereby profite may arise, which is not vsually attained without corruption of heart, deceitful protestations, vaine promises, idle oaths, paltry lyes, pedling deceit, simple denials, palpable leauing his friend, and in famous abuse of charitie (quoted in Shapin 1994: 94).

In this discursive milieu, there was an opportunity to make *politics with science*. Although in political pamphlets there was no clear reference to "science", what is important for us within this genre is that a theoretical *perception* (rather than the expression of it) has been cultivated. As the cultural historian Jean-Christophe Agnew ascribed to the 16th century, this abstraction was first not more than a feeling of a "problematic of exchange" that rose from the frustrations of continuous lamentations about commercial culture.

> What stands out in the 'long-sixteenth-century' inventory of complaints is its groping to envisage a social abstraction – commodity exchange – that was lived rather than thought. (…) In the century preceding the English Civil War, then, Britons could be described as feeling their way round a *problematic* of exchange; that is to say, they were putting forward a coherent and repeated pattern of problems or questions about the nature of social identity, intentionality, accountability, transparency, and reciprocity in commodity transactions – the who, what, when, where, and why of exchange. The answers to such questions form the basis of any ruling class's claim to authority, legitimacy, and justice (Agnew 1988: 9).

The question of what kind of people one's fellows were became so encompassing that it evoked an impression of something else. Economic talk seemed like a play that was subjected to rules nobody was aware of while being enmeshed in issues of credibility and trust. Everyone struggling for his or her share in the common interest – how could one still take that clamor seriously? Perhaps there is more to say than declaring sincere motives and denouncing the opponent. Such were the suggestive questions of the intellectual milieu in early modern Britain. Economic science came out of a genre in that the culture of economic suspicion climbed to a peak and gave rise to a reconfiguration of the political.

Seeing such perception as the main precondition of science rather than the actual claim to it, I do not have to draw a strict line between those writings that Smith has termed mercantilist and others who have been titled first economists, such as William Petty. To ask who were the first economists is certainly one of the less fruitful discussions one can entertain. If there were first convinced economic scientists, one could rather find them in France (see next page). It is

Jean-Baptiste Colbert (1619-1683) Presenting the Members of the Royal Academy of Science to Louis XI

The rise of economics as a science encompasses more cultures than I discuss here for the British case between "mercantilism" and "political arithmetic". There are other candidates that may seem more suitable for narrating the first encounter of epistemic and economic concerns. Above all, the French episode in which Colbert – who initiated the Royal Academy of Science in Paris – give way to the liberalism of the physiocrats in early 18th century could be considered (Hutchison 1988, Groenewegen 2002, Schabas 2006: 42-57). In Germany, one had to inquire how cameralism was adopted and how it changed the image of the civil servant. And in Italy, writers such as Ferdinando Galiani changed the political scene, too. One could even go back to the 15th and 16th centuries, when double entry bookkeeping gave economic talk its first epistemic taint (Poovey 1998: 29ff.). These developments took place largely independently from each other. So why do I focus merely on the British negotiations between mercantilism and political arithmetic?

In France, the new epistemic culture of economic talk, both empirically and theoretically, flourished more intensely than in Britain. Marshall, Marx, and McCulloch all agree: Political economy comes from France. French economic writers, such as Boisguilbert, Quesnay, Condorcet, Turgot, Mirabeua, and later Say were more advanced in claiming scientific authority, more outspoken on their epistemic principles (natural law), and also more explicit on the theoretical conception of "the economy" as an object on its own – manifest in Cantillon's Tableau of 1732. At the beginning of the 18th century, Britain even lagged behind in terms of scientific pretensions. The distinction between the science of political economy and the art of government, for example, is a French invention. "Political economy is not concerned with the motives that drive governments, but with their acts," Say announced (in Fontaine 1996).

France does yet not figure prominently in my social history of scientification since the social resistance French economists had to deal with was lower. Although the social role of merchants was even more contested in France, the epistemic culture was protected by the world of public administrators. This will remain the same throughout the history of economics in France until Edmond Malinvaud. French physiocrats were "consultant administrators", and as such mostly free from suspicion (Schumpeter). The need for epistemic practices came from the centralization of bureaucracy. All authority was thus gained from the *existing* governmental institutions without challenging them fundamentally. Economists did not have to make politics with science, but could make politics by the means of science. For this reason it is tempting to interpret the physiocrat's notion of economic life – that land is the only productive factor – as an echo of pre-modern 'who does not work, shall not eat'. Agriculture and the administration of it resembled the ancient ethos of the oikonomist. In a similar way, I would approach cameralism.

Around 1830, the British scene had incorporated much of the scientific verve from France and other countries. The achievement of the British political economists, however, was not only to translate the French texts, but to establish scholarship in something that was previously ruled by economic suspicion. Issues of veracity and ethos were most pressing on the Island rather than on the continent. From the point of view of economic theory, France and all the other developments in Europe were certainly important for the establishment of political economy. From the point of view of the social history, however, the decisive milieu in which the breakthrough of the epistemic revolution took place was Britain in the 17th century. Economic science made its way not via the concerns of the advisor, but rather as a political agenda opposed to the rhetoric of merchants. The *Urstiftung* of economics took place in Britain.

not that important when exactly I draw the line, but we should not lose the sense of the existence of this line. At one point in history there were no people making truth-claims in economic talk, and then at a later point science was an effective way to make one's point. I follow Letwin, who stated for the case of Britain: "Before 1660 economics did not exist, by 1776 it existed in profusion" (Letwin 1963: ix).

The title 'mercantilism', as it is well known, is misleading for there was no *–ism*, but rather a genre of writings in that merchants pushed into the political discourse. Thomas Mun held the balance of trade doctrine, yes, but not all have shared it. If not by means of a theoretical position, one could draw the line politically in that the first economists argued for free trade, and mercantilists for protection. But this line too, if I believe Appleby, is drawn in black and white. Appleby argued that the landlords, rather than the traders themselves, have achieved political protection for traders (1978: 242 ff.). And Heckscher argued that mercantilists and laissez-faire proponents shared the same *Weltanschauung* (1955: 271). I thus simply list without distinction some of the first writers who infused epistemic concerns into the political discourse.

The mercantilist, according to Smith, was Thomas Mun (1571-1641), one of the heads of the East India Company. In his *England's Treasure by Foreign Trade* (1949 [1630, 1664]) he started by arguing morally about "The Qualities which are required in a perfect Merchant of Forraign Trade". He thus connects to the business literature that buttressed the social recognition of the profession of merchants. But Mun ended arguing rather theoretically that "Forraign Trade is the only means to improve the price of our Lands" (Ibid.). The real surprise is less that he argued that the accumulation of treasure is the result of a positive balance of trade that, in turn, is equated with wealth – the position to which Smith later would reduce him. More surprising is that Mun did not refer a single time to Aristotle. Yet the heritage of clergy's discourse is apparent: merchants need to care about the common interest in order to demonstrate social acceptability. The achievement of writings such as that of Thomas Mun was that care for the common did not necessarily go at the cost of the trader. Trade could be good for everybody. The greed of traders does not contradict moral codes, even if they do not directly pay their share to the poor. Trade contributes to the commonwealth, of which everybody profits in turn.

This new tone of early modern writings is well met in the case of The Merchant Adventurers, perhaps one of the first monopolistic trade guilds of overseas merchants. In their *Discourse Consisting of Motives for the Enlargement and Freedome of Trade; Especially that of Cloth, and other Woolen Manufactures* ..., they claimed political influence with the following words:

> The strength of a Kingdome consists in the riches of many subjects, not a few, in so much that were this Trade enlarged, it would tend to the multiplying of able and wealthy Merchants, it would disperse to it to a greater latitude, and further ennobling the Trade, and prevent the encrease of poore men and beggars up and downe the Land: For it is one of the maine reasons why there are fewer beggars seene in Commonwealths than in Kingdoms, because of community and freedom of trading, by which meanes the wealth of the Land is more equally distributed as amongst the natives (Merchant Adventurers 1645: 22-3).

The authors respond to the premodern logic of economic talk, and by doing so shift emphasis to a different issue. The question is that of traders being virtuous or greedy – the reply is free trade, yes or no.

The character of the merchant was central for reasons of social acceptability, but was also interesting in order to understand their professional success. One moving question was who are the Dutch that they are so successful. Here for example the opening of Sir Josiah Child's (1630-1699) *Brief Observations Concerning Trade and Interest of Money*.

> The prodigious increase of the Netherlanders in their domestick and forreign Trade, Riches, and multitude of Shipping, is the envy of the present, and may be the wonder of all future Generations: And yet the means whereby they have thus advanced themselves, are sufficiently obvious, and in a great measure imitable by most other Nations, but more easily by us of this Kingdom of England, which I shall endeavour to demonstrate in the following discourse (1668).

Child continues discussing various characterizations of what kind of people the Dutch are. But he concludes that their success is due to their low interest rate (see also Appleby 1978: 73 ff.).

The interests of merchants gave occasion to political debates about trade. These motives are not yet stigmatized as later in the 18th century, or even ossified as an assumption in a deductive analysis, as later in the 19th century. They are contested. Merchants can be good and bad. One of the first "theoretical" debates discussed in the literature, for example, is that between the assay master of the English mint Gerard de Malynes (1586-1623) with his *Centre of the Circle of Commerce* (1623), and the Hackney merchant Edward Misselden (1608-1654) with his *Circle of Commerce or the Balance of Trade* (1623). Hackney argued against Malynes in favor of free trade. Occasion for this quarrel was the depression in clothes trade – in particular, whether it is due to exchange rates or coinage shortage (which is in turn also a matter of speculative behavior by bankers). A political issue could be discussed in theoretical terms rather than in moral or legal terms. Poovey has emphasized in this debate that although both have used numerical examples, they did not use them as representations of reality, but, being suspicious of their accurateness, as a mode of illustrating (1998: 77 ff.). They drew their authority rather from appealing to good faith – that is, their ethos. Misselden, for example, opened his treatise with the following words:

> It is true that I am a brother, though unworthy, of that worthy society (of Merchant Adventurers) (…) and also I am a member, though one of the least, of the great common wealth of this Kingdom; wherein I have learned to prefer the public, to all these particular obligations (in Letwin 1963: 91).

The highlight of this period of cultivating truth claims in economic talk was certainly the meticulous William Petty (1623-1687) with his *Political Arithmetick* (1963 [1676]). Petty was a sort of English cameralist who cultivated the authority of numbers and measure (see e.g. Hutchison 1988: 27 ff.). For perhaps the first time "the economy" (or better: "the income of the people", as he called it) becomes an object of a truth claim: "the United Kingdom has improved in wealth and economic health" (1676: 96). That was not the way the people felt at that time, and it was by means of including some and excluding other measures, thus by means of definition, that Petty could warrant his claim (see Poovey 1998: 133).

The culture of theoretical reasoning had been clearly developed by the end of the 17th century. At the time when Dudley North (1641-1691) published his *Discourses Upon Trade*, there was no longer any doubt that theoretical reasoning would replace the old moralizing of the clergy (2004 [1691]). Deductive reasoning combined with a strong appeal to free trade, and a changing focus from trade to production phenomena came to set the tone for what followed in the second half of the 18th century.

The Rhetorical Opportunity for Science within the Merchants' Milieu, but Against their Protection: In the Name of Science – Laissez-Faire!

Taking part in economic talk in Restoration England meant to be able to show a credible commitment to the public interest, which was confined narrowly by the "treasure of England", and broadly by the rivalries England had with other nations. Letwin illustrated in full color the common practice among economists to blame those who allegedly talk in the public interest. "Most men evidently believed that anybody's recommendations on economic affairs ought to be examined suspiciously" (Letwin 1963: 19). Economic talk was associated with political lobbying, cunning, covetousness, and being base, a fraud, or a hypocrite. Arguing in economic talk meant handling others' doubts to argue in one's own interest.

> No group of Restoration society that might interest itself in economic questions could escape the imputation of mercenary motives. A universal cynicism had become dominant. (…) Accusation and cross accusation had become the most constant feature of economic writings (Ibid.: 86/88).

Sir Josiah Child, for example, writes in his pamphlet: "My ends have only been to serve my country, which I can with a sincere heart declare, in the presence of God and men" (Ibid.: 19). However, Child was a merchant, a member of the Council of Trade, and later director of the East India Company, so that such lines were taken to demonstrate the opposite. Only to mention "a sincere heart" could already show that he serves only his own ends. Although Child argued in favor of the protection of merchants as a way to actually increase the treasure of England – thus serving the public interest – his ethos undermined the grounds on which he argued. All his arguments were undermined *a priori*, or, better, *ad hominem*. This indifference about what is said in relation to who says it made it possible to perceive an order beyond actual economic talk, which is governed by something other than *what* is said.

The critical task of economic writings in order to pass from a premodern to modern stage was rhetorical. Early modern economic science "was inspired by the needs of rhetoric" (Letwin 1963: 47). By what means could economists made themselves credible? What I have discussed above as the paradox of the trader – bringing things from here to there without being responsible for them, or at least for their meaning – now found a last expression in that the ethos of merchants made it impossible to make a claim. "(W)hy should a London merchant [trust the writer of a pamphlet] who had prospered by never trusting a stranger?" (Ibid.: 83) Economic talk was entrapped, run into the dead end where mutual suspicion undermined any position of the opponent. "If the magnitude of the difficulty rather than the extent of the achievement be the measure, then the making of economics was the greatest scientific accomplishment of the seventeenth century" (Ibid.: 148).

How then did the first economists deal with this rhetorical challenge? There are several conceivable strategies (Ibid.: 79-98). One is to publish anonymously (as, for example, the first edition of Dudley North's *Discourse* of 1691), or to use a pseudonym, showing willingness to renounce the personal honor of the author. Others pretended to hold other professions than they actually did, diverting the suspicion of being involved in matters discussed. Yet such a move could also hide a well-known base identity. Others simply returned to appealing to the authority of other people who were known as honest subjects of the nation, as was the intellectual rule in all scholasticism (Aristotle + deduction = truth). Yet all strategies involving the identity of the person could be easily used against the intentions of the author.

A subtler move is to present the argument as being *opposed* to one's own interest, as, for example, Nicholas Barbon (1640-1698) did in his *Discourse Concerning Coining the New Money Lighter* (1696) as a reply to the honorable John Locke:

> So that if I were to consider my private interest, I ought to be of the contrary opinion to that I argue for. And therefore I hope I shall be believed when I declare that I have no other design in writing this discourse than the service of my country (quoted in Letwin 1963: 94).

Whether or not this was the case was a matter of debate. Perhaps, if events turned out favorably, Barbon, the well-known banker and financial speculator, would win if his proposal for recoinage went through parliament. Either way, he showed a clear perception of the need for rhetorical innovation to deal with the hermeneutical dead end of cross-accusations.

This need could only be met by switching the locus of argument to a *structural* level. It was Thomas Mun who argued that public and private interests *cannot possibly conflict* (in Letwin 1963: 92). He showed it numerically, and added that such holds as long as King and people restrain from their excessive appetite (for that would be unfavorable for the balance of trade). Therefore, he suggests, traders need to be supported. Thomas Mun introduced a lasting move with which later economists would claim scientific authority: to argue on a level where one's own interest is *systematically* excluded from the argument; a level beyond, prior, and preliminary to – or in any sense not affected by economic suspicion. In this way the horizon was disclosed in which the theoretical perception of that elusive thing "the economy" was possible.

An open question is for *which* position such abstraction could be mobilized. Could scientific authority be employed for any political claim? Was it possible that science could have served the protectionist lobby of merchants? As subtle and surprising as the argument of Thomas Mun may be, what good does it do if the author, a merchant, concludes that merchants should be protected? Though only within the discourse that is ruled by the imposition of mercenary motives was there a need for an economic abstraction beyond mercenary motives, this abstraction could nevertheless not be played out for any interest. The argument ad hominem still holds *despite* the detachment of reason. Although it was only a matter of the ethos of merchants that disclosed a perception of systematic knowledge, it was reserved for others to earn its fruits. The merchants' discourse only set the scene for economic knowledge, but could not claim it. Late 17th-century economic writings could not achieve what made 18th-century economists in the Hume-Steuart-Smith line so prolific: being scholars.

The punch line is apparent. I do confirm the alliance of the liberal and epistemic revolution in economic talk that Foucault and Poovey stated. Scientific authority came into being on a level *beyond interest*, yet was claimed against a *particular interest* – against the protectionists' demands of merchants. Only there, *within* the discourse of trade, but *against* the protection of trade could one gain a discursive identity by claiming scientific authority: In the Name of Science – Laissez-Faire!

The Formalism of William Petty's Empiricism: Bacon's Blind Eye

Rather than the rise of science, is this not a farce of it? Which role did science play in this account – science in the sense of a practice that aims at the referential truth of "the economy"? Was the rise of political economy not the rise of a culture of claiming economic *facts* rather than coming to terms with a particular *abstraction*? Let me thus add a defense to my account of the *Urstiftung* of economic science.

"The economy" did not come about as an *object* if object means to be described in terms of distinct qualities. For this reason I continue using quotation marks. It only came about as an object in the sense of being independent of one's own interest. Although these two aspects of objectivity do not exclude each other, the latter does *not* necessarily imply the former. And it is precisely for this reason that economic science, granted, came *with* evidence, but it was not made *by* evidence. Instead, it was made by theory.

In order to assure clarity at this point, consider William Petty's enthusiasm for Bacon. This enthusiasm made him appear an economic scientist, but it did not grant him the ethos of an economic scientist. Here, his Baconean credo:

> The Method I take to do this, is not yet very usual; for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the course (as a Specimen of the Political Arithmetick I have long aimed at) to express my self in Terms of Number, Weight, or Measure; to use only Arguments of Sense, and to consider only such Causes, as have visible Foundations in Nature; leaving those that depend upon the mutable Minds, Opinions, Appetites, and Passions of particular Men, to the Consideration of others (Petty 1899: 244).

In William Petty (just as in German cameralism), "the economy" was an object of measurement and counting. There we find an image of "the economy" that seems ready-made for a phenomenological critique of economics. There we find a world objectified in numbers. What Petty called the "income of the people", Heidegger had called the *Gestell*, the world *qua* resource. "The economy" refers for Petty to the amount of acre of fruitful land, of corn growing on it, of people harvesting it, of things one could trade with the harvest, of populations one could feed with it – the world qua "petrol station": "Now the world appears like an object, upon which counting thought (*das rechnende Denken*) sets its attacks, which nothing can resist. Nature turns into one huge petrol station" (Heidegger 2000: 523*). Petty was moved by the question of How Much? – which meant above all: How much does England depend on its colonies? And how much of the British population needs to migrate in order to re-cultivate Ireland after the massacre of the Civil War? The title of Petty's treatise illustrates the point:

> Political Arithmetick: Or a discourse Concerning, The Extent and Value of Lands, People, Buildings: Husbandry, Manufacture, Commerce, Fishery, Artizans, Seamen, Soldiers; Publick Revenues, Interest, Taxes, Superlucration, Registries, Banks Valuation of Men, Increasing of Seamen, of Militia's, Harbours, Situation, Shipping, Power at Sea, &c. As the same relates to every Country in general, but more

> particularly to the Territories of His Majesty of Great Britain, and his Neighbours of Holland, Zealand, and France. By Sir William Petty, Late Fellow of the Royal Society (1899: 233).

"The economy" in Petty's image was finite, countable, and in this sense objective. For this reason, others have counted him the first economist – Marx, for example. "By the early eighteenth century numbers had acquired a set of connotations that would soon make them central to what counted as knowledge in numerous domains" (Poovey 1998: 143). However, it was not the mere fact that Petty counted that made him appear scientific, but the complexity of what he counted. What he counted were not merely total amounts (1, 2, 3…), which people always did when determining King's tenth. Petty, rather, was interested in the *relation* of the amount of land and the number of mouths that can be fed with what grows on that land:

> 'twould be expedient to know the Content of Acres of every Parish, and withal, what quantity of Butter, Cheese, Corn, and Wooll, was raised out of it for three years consequent; for thence the natural Value of the Land may be known, and by number of People living on within a Market-days Journey, and the Value of their Housing, which shews the Quality and Expence of the said People (1899:180).

Is this not the same concern of which Ricardo, the so-called "inventor" of economic theory, would later be driven – namely, whether the rising cost of corn (and thus bread, and thus poverty, and thus starvation) was due to the rising land scarcity and growing population, which makes the rate of profit on capital decline? (see Blaug: 1996 [1962]: 86 ff) Neither the fact that Petty counted made him an economist, nor that he perceived it as a completely determinable thing to be measured. But he was economist by means of perceiving a structure of "the economy". Theory, not evidence. This is why Petty was also a child of the discursive culture described above that first has disclosed the opportunity of epistemic claims.

Even if Petty's self-image were as anti-theoretical as Bacon's eye itself, all economists after him were interested in his work only because of the structure that made him count. At least ex post, in light of what Petty helped getting off the ground, how could we believe that his empiricism made him a founding economist? Petty did not accomplish that those after him, from Steuart down to McCulloch and Nassau-Senior including Ricardo, would not look at numbers with suspicion. Petty's statistics were soon outdated – just like the weather in 17th century Ireland – but not the structure he conceived. Even Poovey acknowledged that in the passage of the 18th century

> instead of promoting the elaborate infrastructure necessary to collect numerical information, British theorists of wealth and society developed a mode of analysis that could be used in *absence* of numerical data. (…) (W)hat counted as knowledge (…) was a form of theoretical generalizations that devalued observed particulars in favor of something that could not be seen and, in so doing, made collecting numerical data all but redundant (1998: 215).

What made numbers appealing for claiming scientific authority was not their referential accuracy or precision. It was the *formal procedure* with which the numbers were produced. Rather than Petty's counting, but the trope of his "gestural mathematics", as Poovey called it, made him an economist (Ibid.).

In other words, Petty was "objective" not in the sense of referring to the existence of particulars, but he was objective in the sense of appearing beyond his interest to claim something particular. This difference is vital when returning to Foucault's and Poovey's argument about the alliance between liberalism and the science of wealth. Foucault has argued about that link that only in the absence of political intervention can "the economy" reveal its nature independent of human design (2008: 30 ff). Thus only a *spontaneous* order of "the economy" is capable of being the object of an epistemic claim, while everything else is man-made and thus possibly corrupted. Only a self-constrained politics can be sensitive to the "veridiction" of "the economy".

> [W]ith political economy we enter an age whose principle could be this: A government is never sufficiently aware that it always risks governing too much, or, a government never knows too well how to govern just enough. (Ibid.: 17)

Foucault's analogy of objectivism and liberal discreetness means less that in the political discourse there is a new agreement among all parties on a particular object that actually does show its truth, let alone an agreement about the methods to gain that truth. "The economy" – you can quote me on this – has never shown itself, even if government was on holiday. "The economy" does not denote a phenomenal object for Foucault, but a new order of the political discourse that is subjected to the truth of "the economy" – *whatever* that truth means. Without this "whatever" we miss the point of the alliance of the epistemic and liberal revolution in 17th-century Britain.

Consider, for example, how the inflationary use of natural metaphors functioned within the new economic political (Schabas, de Marchi 2003). Did liberalism not go hand-in-hand with the adaptation of the mechanistic worldview as applied to the political domain? No. The metaphors of natural scientists (who were the actual makers of science) did indeed suite the relation between political actions and consequences on "the economy". To talk about interests as though they are related like cause and effect is to talk about interests in such a way that they cannot harm the author who states them. It was for this reason that the metaphors of Newtonain science were just as good, or even better than those of Bacon's eye. They indeed came to stamp the self-perception of economists' practices for years to come. This inclination of economic science to borrow from the ethos of successful – particularly hard sciences – remained an effective rhetorical tactic throughout its modern period. Political economy was "like" science. That was sufficient. Whatever the grounds of science, its discursive role was always the same: to avoid the question Who Are You – Arguing This!

Be it by virtue of observation and accounting or by virtue of Newtonian metaphors of physics, neither stems from the discursive need for science in economic talk. What economists needed were not statistics, were not causes; they needed to go beyond the logic of economic suspicion. They needed to appear detached. Whatever met this need, it contributed to their ethos. Yet implicit philosophical beliefs did play a buttressing role. The obvious tension between arguing beyond interests and nevertheless arguing in favor of or against a particular interest was softened by the use of metaphors that were successful in other sciences. In other words, as long as economics could free-ride on the image of other sciences, as long as there was a developed scientific optimism, it was possible to hide this tension and declare wholeheartedly in the spirit of modern life: In the Name of Science – Laissez-faire!

The Oblivion that Made Adam Smith a Scholar: Beyond the Merchant's Suspicion, yet not Falling Back on the Clergy's Lament

What then about Adam Smith – to whom the profession of historians still today allocates more time than to any other economist? Letwin concludes that Smith did not invent political economy from scratch, but was able to gather and summarize the finest pieces of all topics that had been discussed before. "Everything useful that they [17th and 18th century economic writers] did, Adam Smith incorporated" (Letwin 1963: 221). Granted, he substantively took more from the 18th century than from the 17th century, more from the Hume-Steuart-Franklins than the Malynes-Misselden-Muns. In the passage of the 18th century, the detachment from the mercantile milieu had already taken place. In light of the rising manufactories, focus shifted slowly from trade to production phenomena. The epistemic genre switched from pamphlets to *treatises*, and the science of wealth gained its first *Principles* – such as Steuart's *Inquiry into the Principles of Oeconomy*, 1993 [1767]. The last echo of oikonomic writings vanished. Adam Smith reaped the benefits.

Adam Smith was not a revolutionary of science. He was indeed rather a moderate believer of science. Smith was a literary scholar of the Scottish Enlightenment, gathering rather than measuring. He was open to moral philosophy and the rhetorical tradition rather than constrained by observation and suspicion. The contribution of Smith to the present history of scientification was not that he pushed science, but that he substantiated its body. Adam Smith – this made him special – was a *scholar*. His achievement was to re-codify political economy, which is to say that he managed to detach it from the discursive milieu I have described in this chapter. Smith's distinct achievement was to be able to solve the rhetorical dilemma of doing two things at the same time: abstracting from particular political interests by means of relying on a theoretical perception of "the economy" – based on the idea of division of labor – *and* addressing political interests. No ambiguity appeared in arguing beyond interest but nevertheless for a particular interest. Smith's *Wealth of Nations* – therein lies its greatness – is policy-oriented yet not a pamphlet!

> Fortune saw to it, for one thing, that Smith faced no special problem in establishing his credentials. Granted that his use of deductive demonstration would have guarded him, as it did North, against the charge that he wrote to feather his own nest; still he needed no protection. He certainly took none. Not one word in his book follows the apologetic formula that seventeenth century writers used (Letwin 1963: 221).

There could be an Adam Smith only thanks to countless pamphlets that had prepared the ground for his economic abstractions. Smith, using the abstraction of the merchants, was himself also suspicious of them, and in particular of those who, like Tomas Mun, pretended to act in favor of the common interest. "I have never known much good done by those who affected to trade for the public good", Smith writes after his mention of the invisible hand (Smith 1976: 456). What made Adam Smith the first of the moderns (rather than an early modern) is that neither did he have to defend *himself* against the suspicion of being guided by his self-interest (like all merchants), nor did he fall back into the clergy's moral lament. Beyond the clergy and the merchants, there was the scholar. Modern times have moved on.

Here I arrive at a first step in the phenomenological genealogy of the invisible hand. We have now gained an initial understanding of the success of the phrases that made Adam Smith famous: "It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own self-interest (…) Nobody but a beggar chuses to depend chiefly upon the benevolence of his fellow-citizens" (Smith 1976: 26f.). Such statements only softly echo the intellectual milieu of the 17th century, in which those gentlemen appear respectable who argue subtly against the motives of their opponent. Merchants since Smith are admittedly self-interested. They no longer have to pretend to be of good will, because there is a level of reflection where such no longer counts: "the economy". The character of merchants became stigmatized.

Although the motive of the initiating economic abstraction was to avoid the imposition of self-interest on the author, self-interest since Smith came to be a matter of theory: one could allow for, or even "presume" merchants to be self-interested. This will constitute the central trope of the invisible hand in 19th-century political economy. Everybody who had done so before immediately had to face doubts about his integrity. Political economists, however, did not have to feel *personally* addressed. Since Smith, economists can see themselves as "outside-judgers", as Colander said about economists today (1991: 23). Yet even if they were outsiders, did economists not only assume, but also *justify* merchants' greed? Were political economists able to bring an end to the culture of economic suspicion simply by avoiding it? This would be the question that prompted the second wave of the scientification of economics.

Adam Smith, in order to anticipate the rest of the intervention of economic science in European history, was, so to speak, the first John Stuart Mill, the first Alfred Marshall, the first Paul Samuelson, the first Mas-Colell et al. They all codified the body of economics to such an extent that the problematic context from which their work was produced disappears. Since Smith, economists can make claims beyond the threat of the imposition of being guided by their own interest. Smith was the first step toward an economic science that forgets its history in an eloquent summary. He was the first who made it futile to read what preceded him. Considering the entire SMMS line, Smith would later be a victim of precisely the same move that allowed him to be popular for some time – later, when the invisible hand was axiomatised.

Being beyond suspicion, economists could claim a place in academia. After Smith, but not until the first quarter of the 19th century, economists settled in the modern halls of truth. Before that, there was anyway no public ethos of a *scientist*. University before was academia (the Greek word), and economics was *oikonomia* (Aristotle's text). Smith was less of a "scientist" than all economists before him and also less than those economists who promoted Smith as a scientist. I mean those who reaped the first institutional profits from Smithean scholarship in academia, namely the first professors like the Ricardian John Ramsey McCulloch (1789-1864), who took a chair in political economy at the University College of London in 1827, Nassau William Senior (1790-1864), who took the Drummond chair of political economy in 1825, or Richard Whately (1787-1863), who spoke in the 1830s of economics as the deductive science of catallactics as Edgeworth would later do. In this series of political economists, there were only a few new William Pettys, such as William Whewell (1794-1866), who defended an inductive image of economics. The rest celebrated the new freedom of economic abstraction, free of the annoying question: Who Are You – Arguing This!

Regarding this arrival of political economy in academia, it is noteworthy that not one of those economist who are today called the first classical economists, held a chair at the university. Thomas Malthus (1766-1834) was a clergyman, and David Ricardo (1772-1823) a stockbroker. Both were political agents. Considering the disgrace political economists would earn later in the 19th century, they indeed entered the social history of economics *only* as political agents, precisely as their pre-Smithean predecessors. Sure, Ricardo cultivated economic abstraction as no other before them, which made Mark Blaug believe: "if economics is essentially an engine of analysis, a method of thinking rather than a body of substantive results, Ricardo literally invented the technique of economics" (1996 [1962]: 132). Philosophically speaking, yes. But regarding the social history, Ricardo did not contribute to the ethos of economists. To the contrary, considering the harsh criticism he and Malthus would earn in mid-19th century, they have shown that the ethos of an "economic scientist" was not yet fully established, despite Smithean scholarship. The disgrace they earned showed how brittle was the ground on which one claimed: In the Name of Science – Laissez-faire.

Regarding the present social history, there is one telling episode, though, in which Ricardo, Malthus, and James Mill (the father of John Stuart) were involved: *The Political Economy Club* of London, founded in 1821. It was founded in order to come at an agreement about the basic principles of political economy. Agreement is surely a basic feature of science. Political economy, if it deserves its name, could impossibly be a locus of contest, but needed to be capable of agreement. The project of *The Political Economy Club* showed that political economy could not afford to be an open field of contest and debate. If a discourse is undermined instead of constituted by the disagreement of its participants, we hardly speak of a scholarly community, as Adam Smith and his Scottish friends would have liked. The nerve-crumbling discussions in the club soon made Ricardo despair about the prospects of agreement.

Disagreement remained a stumbling block in the further scientification of economics. Only as long as those who are committed to science are able to agree on the tenets of political economy can it be science. If not, what else is it than the expression of a particular political interest? Actual scholarship, gaining identity from debate, was never an option. The agreement was achieved, as I am going to show in the next chapter, not by means of merging opinions, but on the cost of different political interests. On a level where less is at stake, agreement is more easily achieved – arriving at a structural level beyond interests, which is the master key to the scientification of economics.

Summing up this first wave of the scientification of economics, I have shown an instability of the *Urstiftung* of epistemic concerns in economic talk. Economics did not rise as a stable institution, did not rise as a new paradigm suited for the new modern world. The liberalism of early modern economists functioned doubly. On the one hand, as Foucault correctly argued, it discloses an epistemic realm and is thus the *condition* of scientific authority. On the other, the liberal position needs to be presented as the *result* of scientific reasoning. Therefore, what allows economists to claim epistemic authority and what they claim are somewhat the same. And so epistemic authority could be claimed only with a tone of inhibition, with a slightly trembling voice that neither fully accorded with the optimism about science nor about growth: In the Name of Science: Laissez-Faire! But please don't ask: Who are You – Arguing This!

(3) The Century of High Modernism (1850-1950)

The first wave of the scientification of economic writings lasted roughly until the 1840s. At this point, political economy was dominantly British. It could incorporate much of the theoretical sophistication achieved in France and the rest of Europe. Moreover, British political economists have gained a disciplinary profile in association with a particular political position – namely, free trade. There were simply not many other people who appealed to the scientific authority apart from those arguing in favor of liberal policies. Political economy came to be known as a sophisticated way of arguing for the merits of laissez-faire (see e.g. Winch 1976).

Fixing a date for the end of the first wave of scientification, I could choose 1846, the year when the Corn Laws were abolished, and liberals as the Peelites came to be. The Corn Laws were a protective import tariff on Corn. The debate surrounding these laws shaped British political discourse since 1815. It took place between conservative Tories supporting landlords, and Whigs supporting manufactures including their workers (at least, so it seemed). Ricardo's doctrine of the comparative advantage served as one of the benchmarks in these debates. George Stigler, characteristically, disassociated the end of the Corn Laws from the acceptance of political economy. He argued "materialistically" that the laws had to be abolished anyway due to market forces of efficiency (Stigler 1982: 65). How would have Ricardo responded?

The authority of political economists crumbled since the moment their doctrines came in too close contact with the folk doctrines that were inspired by them. Since the 1830s writers such as Jane Marcet (1769-1858) and Harriet Martineau (1802-1876), who were in close contact and had friendships with prominent economists, have watered down Smith's and Ricardo's abstractions to digestible teachings for the working class. The principles of free trade and division of labor should have enlightened the working class, and liberate it from its misery. Harriet Martineau published monthly novels, the *Illustrations of Political Economy*, that translated the tenets of political economy into laissez-faire apologies by the colors of heart-breaking stories in the worker's milieu (2004 [1832]). And the textbook of Jane Marcet, *Conversations of Political Economy: in which the elements of that science are familiarly explained* (1839), presented laissez-faire policies as a must-know of the same rank as the alphabet:

> *Caroline*: Well, Mrs.B., I see that you will not allow of any exception in favour of the corn-trade, and that I must consent to admit of the propriety of leaving all trade whatever perfectly free and open.

> *Mrs.B.* That is certainly the wisest way. Instead of struggling against the dictates of reason and nature, and madly attempting to produce every thing at home, countries should study to direct their labours to those departments of industry for which their situation and circumstances are best adapted (1839: 359).

Although there was no full agreement about the principles of political economy – at least not inside the shrine of the *Political Economy Club* – such too openly announced link between what 'reason and nature dictates' and the laissez-faire politics was gnawing heavily at the credibility of economists.

> It was this body of literature, with its dogmatism, apologetics, and facile transformation of theories into policy conclusions, that was responsible for the chorus of abuse directed at the subject from all directions – from working class and Tory radicals, from aristocratic humanitarians, and from the spokesmen for the Romantic movement (Winch 1976: 541).

Around the 1840s a crisis of political economy slowly evolved, which would not be resolved until, say, the 1880s. In a new political landscape there was a clearly perceived inadequacy of the discursive identity of political economists. The pressure I allude to, of course, are the social changes surrounding the second wave of industrialization – that is, those developments in the 19th century that made people doubt that modern life will liberate mankind without conflicts. Metaphors of pre-stabilized harmonies in the rhythm of which "the economy" swung lost their grip. By mid-19th century it became clear to all British people, and moreover to all European people that there was a disturbing by-product of the liberation by growth: *the culture of capitalism*. It is worth risking superficiality when recalling some of the popular images of this new culture since they would come to represent the basic tone of all economic skepticisms.

The Culture of Capitalism and the Battle of Ideologies: By the Means of Science – Revolution!

Since mid-19th century trade ceased to be the source of perceptions surrounding "the economy". Instead, economic talk became to be associated with the predicaments of production. This perception was more intensive for it affected the entire society, and also more extensive for it affected all of Europe. "The economy" was ripped from its British home ground. It crossed nations, and became enriched with a new international consciousness: the class-consciousness. The new subjects were "the workers" using machines, and "the capitalist" owning machines. Like previously merchants, workers claimed political representation, and thus challenged the social conditions of truth in economic talk. Initially legal acts tried to cope with this change, but the revolutionary spirit soon changed the modern economic political. Already in the first decades of the 19th century labor movements began to mobilize. Luddites stormed the machines, Saint-Simonean sects gathered, and Owenses built New Harmonies (for an early history of socialist movements, see Sombart 1968 [1896]). *The Reform Act* of 1867, to pick one event, marks one turn in Britain. Until then the formation of labor unions was a criminal act. Those reformers of life who around the turn of the century still did not have enough of revolutions met on Monte Verità.

Since the mid-19th century it became clear to many that the installation of freedom is itself a forceful act, which is one of the genuine Marxian thoughts. Foucault, for example, repeated this argument in his history of liberalism: "With one hand freedom must be produced, but this very act entails that the other hand establishes limitations, controls, forms of coercion, and obligations relying on threats, etcetera." (Foucault 2008: 64) The liberation from corrupt politics by means of its subordination to "the economy" lost its grip and credibility. The culture of capitalism is not as harmonious as the modern triad of science, technology and growth suggests. There is conflict.

One source of complaints about this new culture certainly was the loss of faith in progress by technology. One of the leading tropes of 19th century cultural criticism was that man-made machine turns against man. Adam Smith surely would have been more careful with his Newtonian metaphors of mechanics after his little needle manufactory came to be a sooted hollow of mass-production, as production phenomena came to be portrayed. The new era of self-moving heat engines shed a different light on problems of "poverty". Being poor was no longer not only a matter of survival, death, and hunger, but became a social problem – a problem of "commodification", as Thomas Carlyle said, and "alienation", as Karl Marx said, and thus, of course, of "exploitation", as many continue to say over and over again. The machine ceased working for man, and man began working like a machine – "an appendage of the machine" as Marx and Engels wrote in their *Manifesto*.

Such social imaginaries of the industrialization went so deep into the European mind that I hardly need to illustrate further. Many, until today, have drawn from their humanitarian sentiments huge energies for describing painstakingly the social miseries of the culture of capitalism, for blaming its actors polemically, and envisioning *en detail* a world free of these miseries. A new genre of cultural criticism came to be, starting from the utopians, and continuing in novels like Carlyle's *Past and Present* (1843), Bellamy's *Looking Backward* (1888), George's *Poverty and Progress* (1871) – a genre that continued throughout this century as in Arnold's *Folklore of Capitalism* (1937), and down to the present-day Galbraith-Bell-Klein literature. Never outmoded, always needed. Carlyle set the tone of this genre:

> And yet I will venture to believe that in no time, since the beginnings of Society, was the lot of those same dumb millions of toilers so entirely unbearable as it is even in the days now passing over. It is not to die (…). But it is to live miserable we know not why; to work sore and yet gain nothing; to be heart-worn, weary, yet isolated, unrelated, girt in with a cold universal Laissez-faire: it is to do slowly all our life long, imprisoned in a deaf, dead, Infinite Injustice, as in the accursed iron belly of a Phalaris' Bull! This is and remains forever intolerable to all men whom God has made (1848 [1843]: 211).

This tone of cultural criticism entered deep into western literary minds of the 20th century, from romantic literature to realism, in theatre, film and music. Here is the literary root of the Marxian tendency in everyday economics that I have observed in the last part.

Speaking of the culture of capitalism, how could I forget the new political in post-1848? Politics was no longer a matter of this or that special interest, but a matter of *systems*. And while laws are objects of parliamentarians, systems are objects of revolutions. In 1848 politics became revolutionary, it became European, and yet national. And most important, it became conflicting as almost all of Europe shouted one clarion call against each other: Liberty! In this clamor, the soft-spoken laissez-faire economists debating in their private Club could hardly claim authority to this title of liberty. Liberty needs force; the force of those who are on "our" side. Thus, perhaps the most salient development for the social history of economics that stamped the modern image of "the economy" is the rise of *ideologies*. The "phenomenal republic of interests" turned into a *battle between political systems*. Ideology is the historical phenomenon that describes the political century between 1850 and 1950 – the century of high modernism in European politics. It encompassed nationalist, liberal, restorationist, fascist, cosmopolitan, bourgeois, democratic, socialist, communist ideologies, etc., fought for in all hemispheres of the globe. It was not before 1945 that politics ceased to be a battle of ideologies.

Returning to the scientification of economics, in the decades between 1840 and 1880 the general esteem of political economy was considerably low. In 1876, "[o]n the occasion of the dinner given by the Political Economy Club of London to mark the centenary of the *Wealth of Nations* it was suggested that the economists 'had better be celebrating the obsequies of their science than its jubilee'." (Coats 1954: 143) The very existence of Section F "Political Economy and Statistics" of the British Association for the Advancement of Science has been openly challenged (Ibid.). The reason was that with the doubts about the integrity of the modern triad, there emerged a rising industry of blaming political economists for justifying social misery. Most popular are Carlyle, Ruskin, and Tawney – compiled with all their friends in Coleman's history of anti-economics (2002). Again for the sake of tone it is worth quoting one of the popular lines of Carlyle.

> In brief, all this Mammon-Gospel, of Supply-and-demand, Competition, Laissez-faire, and Devil take the hindmost, begins to be one of the shabbiest Gospels ever preached on Earth; or altogether the shabbiest. Even with Dilettante partridge-nets, and at a horrible expenditure of pain, who shall regret to see the entirely transient, and at best somewhat despicable life strangled out of it? At the best, as we say, a somewhat despicable, unvenerable thing, this same 'Laissez-faire;' and now, at the worst, fast growing an altogether detestable one! (1848: 184)

The verve of scientific optimism that political economists could once rely on passed away in these lines. Political economy disregards, if not distorts or hides, and is in any case inadequate for the miseries of 19th century capitalism. Clearly, with this culture the economic suspicion had to fall back on the British economist. But now it came not from the clergy, but from inside the profession, like here from the German nationalist economist Friedrich List (1789-1846).

> Any nation which by means of protective duties and restrictions on navigation has raised her manufacturing power and her navigation to such a degree of development that no other nation can sustain free competition with her, can do nothing wiser than to throw away these ladders of her greatness, to preach to other nations the benefits of free trade, and to declare in penitent tones that she has hitherto wandered in the paths of error, and has now for the first time succeeded in discovering the truth (List 2005: 47).

Back is the economic suspicion. Economic science had to prove itself not against the protective interests of merchants, but against those who believed in a better world for all human beings. Now political economists had to face up to the New Good as opposed to the Old Clergy.

The stigmatization of political economy could have been deadly. Political economy could have sunk back in some British debates about policies of yesterday if there had not been others who re-claimed science – socialists. If they had not re-claimed scientific authority, my narrative could have ended here. Socialists claimed their own vision of how technology (means of production), growth (towards the class-less society), and science (which I explain in a second) come together. The idea of "Scientific Socialism" combines loosely related attempts that associated scientific optimism with a vision of a post-capitalistic society. Marx added his share, as did the Austromarxists around Hilferding, and Otto Neurath gave it a real kick. Later, Oskar Lange and Jakob Marschak actually designed the socialist model of "the economy". I will describe these attempts later in this chapter. One could also recall the British movements such as Bernalism, and the Association for Scientific Workers of the 1930s that associated scientism and Marxism (Werskey 1978). The difference between early utopian socialism of the first quarter of the 19th century and scientific socialism was that science was not merely a virtual ally within a political project, but socialists regained the very theoretical perception of "the economy". Scientific socialism made science itself politically contested, and thus provided new ground for another wave of scientification. Without scientific socialism, the science of wealth could have ended at the margins of the archive for the Scottish Enlightenment.

In 1848, two texts were published that mark the symbolic beginning of the second wave of scientification, *The Communist Manifesto*, and Mill's *Principles of Political Economy*. Both nourished the belief that the project of economic science could be *rescued*. Marx and Mill mark the beginning of the century of the ideological battles *for* scientific authority. The difference of the first and second wave is that scientific authority is no longer employed *in* the political discourse, but scientific authority itself became politically contested. Now, political debates could be entertained along the question of which party can claim scientific authority. Science became politically contested, and politics became scientifically contested.

Next to those saying 'In the Name of Science: Laissez-faire!' a second clarion call provided affective ground for the project of economic science: By the Means of Science: Revolution! Here are the origins of the division between "real" and "pseudo" economic science. The second wave of scientification marks thus the beginning of a philosophical awareness of economists. The outcome of this contest, I will argue in this transitional chapter, was not the victory of one over the other. The actual scientification did *not* happen by means of a decision between political systems, but by means of moving beyond them. Both political sides claimed a reality *beyond* economic life. Both sides fostered their own structural giant of "the economy" – "the market" and "capital". Only on this structure, on the preliminaries to meaning, the scientification could ultimately calm. Since the marginal revolution of the 1870s, "the economy" ever clearer was only accessible for scientific concern as long as it was stated beyond this political contest. Efforts of scientification have been channeled into an abstraction beyond the ideological battles, not a solution within these debates. The rough-and-tumble of the ideological battles gave rise to a new discreetness, and a new moderation that made the success of the formalist revolution in the 1950s possible. The socialist calculation debate between 1920 and 1945, which I discuss below, stands in this respect as a monument of high modernism in economics. 1850-1950 while heated on the one hand was the century for becoming moderate on the other; a moderation that culminated in the silence of the formalist revolution.

Thus, 1850-1950, the century through which I will slide in this chapter more or less neatly. I call it the century of high modernism for only then was economic science actually contested – for this reason the bulk of historians of economics focus solely on this period. There are at least three definite, and interrelated markers that allow for this periodization: (1) 1850 socialists began to re-claim science, while at 1950 the claim to scientific authority was free from the ideological battle between socialism and liberalism. (2) 1850 historicists began to claim representation in science, while 1950 any historical reflection was out, and history of economic thought became a separated sub-discipline – halfway, around 1900, economic history moved to the history department. (3) In 1850 economists became philosophical about their science, and started to think about the particular epistemic character of their science (thus they stopped using the metaphors of other sciences implicitly); while 1950 marks the end of the philosophical awareness of economists since there was no longer any need to make up one's mind about the scientificity of the discipline – philosophy of economics became a separate sub-field in the 1970s.

Marx's Reclaiming of Scientific Authority between Materialism and Positivism: The Concern for the Concrete

It would be easy to downplay Marx's role in the scientification of economic writings if he had not snuck into the minds of so many others – though the point can quickly be made. Marx himself was disposed of such an abundance of affective resources that he could easily renounce scientific optimism as a source of his undertaking. He did not worry too much about science. His reference was German Idealism. His materialism comes from there. And all he said about science, in turn, comes from his materialism. Marx's contribution to the scientification of economics had to be mediated by others.

There is yet one overwhelming notion that overshadows the entire century of political battles for economic science: Marx's conviction that political economy is bourgeois. In light of the time described above such a statement does not need to imply more than that Marx shared the common perception that political economy was insufficient for coping with the misery of the day. At the same time, Marx was clearly a political economist. Every exegesis of Marx's text must recognize the debt he owes to the reading of Mill, Ricardo, and down to Boisguilbert and Petty. From where else did he take all his economic categories? One actually has to go into the writings of Engels in order to find a quote that confirms Marx's blame of political economy being bourgeois.

> Political economy came into being as a natural result of the expansion of trade, and with its appearance elementary, unscientific huckstering was replaced by a developed system of licensed fraud, an entire science of enrichment (*Outlines of a Critique of Political Economy*).

Though Marx acknowledged the preliminary accomplishment of political economy, he undermines it because of the historical situation in that it appeared. In this fashion, Marx took over the culture of economic suspicion. His materialism cultivated, and even systematized the critical question: Who Are You – Arguing This! How could such vigor have found ways into the economists' body of knowledge? A direct encounter was difficult since bourgeois political economy meant nothing but an effect of the capitalist mode of production. And those who believe the contrary will vanish anyway as soon as class struggle increases, as he wrote in the *Afterword* to *Capital I*. So how could have Marx engaged in a dialogue with political economists? Historical materialism, to say the least, makes it difficult to talk to each other.

Marx would have not entered in the order of economists if he had not relied to some extent on scientific optimism. His materialism re-cultivated not only the culture of economic suspicion, but also the notion of "real" economic science. Both! Marx did claim science, *real* science of economic reality "as it is", not merely as we think it is. Science begins, he writes in the *German Ideology*,

> where speculation ends – in real life – there real, positive science begins: the representation of the practical activity, of the practical process of development of men. Empty talk about consciousness ceases, and real knowledge has to take place.

These two associations of Marx's materialism – one undermining past political economy, the other envisioning a future "real, positive" science – draw a huge cross in the constellations of economic talk since 1850. With one hand, Marx nourished positivism, and with the other he nourished cultural criticism. The former affirms reality, and the latter affirms reflexivity. The former refers to truth, and the latter evokes it. The economic genres inspired by Marx thus continued on two different continents: in science, and in social philosophy. Until today, references to Marx in the *AER* do not overlap with references to Marx in the *New Left Review*.

To ask on which grounds Marx actually claimed scientific authority does not help solving this tension. It meant, first and simply, 'not religion', through which communists initially were identified. I cannot avoid associating Marx with the rigor of Hegelian dialectics, which makes his writings not less formal than what readers knew from Ricardo. But for Marx science also refers to the opposite of "philosophy", in particular Hegel's German Idealism. He even associated science with Bacon's eye for he held some esteem for Petty and Boisguilbert: "Empirical observation must in each separate instance bring out empirically, and without any mystification and speculation, the connection of the social and political structure with production" (*German Ideology*). He moreover linked science with historicism: "We know only a single science, the science of history" (*German Ideology*, crossed). But ultimately science was simply knowledge that serves the class struggle. Interestingly, this does not mean that economic science became socialist, but "Socialism became a science", as Engels wrote in 1877:

> To thoroughly comprehend the historical conditions and thus the very nature of this act [the universal emancipation of the proletariat], to impart to the now oppressed proletarian class a full knowledge of the conditions and of the meaning of the momentous act it is called upon to accomplish, this is the task of the theoretical expression of the proletarian movement, scientific Socialism (*Socialism: Utopian and Scientific*).

By the Means of Science: Revolution! – *whatever* science that is.

Surly, Marx could inspire so many streams of thought that are perceived today beyond the limits of economics only because of this variety of meanings of "science". Sociology, economic history, and political science are all disciplines that gained (new) shape in the second half of the

19th century with reference to Marx. But for the same reason of variety, those who obeyed the scientific clarion call never could rely heavily on Marx's texts. "Scientific socialism", at least to the extent that it entered the institutions of economics, was never "Marxist" since economists never wanted to make socialism a science, but they wanted to make economic science socialist. As another consequence of this variety, there are only a few writers who defended Marx as an actual "scientist" in accordance to the standard body of philosophy of science (Little 1986, or in their own way, analytical Marxists of the 1980s, such as Jon Elster 1986).

What combines the various genres that owe credit to Marx is a certain sensibility for the *concreteness* of economic life. Marx could mobilize that same concreteness that I have downplayed in William Petty because of the latter's structuralism. Concrete economic life for Marx is constitutive of social structure, not the other way around. After the economic turn of the political in 17th century Britain, Marx again reversed the relation of political and economic life: now once more political life comes *from* economic life. It is for this reason that there is, as compared to the liberal tradition of political economy, astonishingly much of Aristotle in Marx – not only if we think of chrematistics and the fetish of "merely for the money" (for an unheated comparison see Booth 1993).

The materialist aspect of this sense for the concrete, I repeat, never entered the shrine of science. *Material* economic life was that *from which* we conduct an epistemic life. The hurdle of Marx's materialism that is difficult to take for any intellectual is that economic life not only enables and liberates to a moral, epistemic, and political life, as in Aristotle, but also determines it. This relation of economic and epistemic life was supposedly the very object of Marx's science – which made it much too philosophical to be possibly true. The difference of Marx's materialism and any abstract reasoning is more than a simple opposition. The object of a materialist science is economic life *before* it takes form: 'consciousness explained from the contradictions of material life', as he wrote in 1859 in the *Preface* to *A Contribution to the Critique of Political Economy*. The "form", instead, as it will describe the scientification of economics, "captures" content only because it does *not* entail it – so that formalism after all has nothing to do with *abstraction*. Hence there could never be any real encounter between a materialist notion of knowledge and that of modern science, which views knowledge as referring to truth. How could reflexivity, or a simple reflection on the material conditions of science, ever meet the intellectual needs of economists who just overcame the culture of economic suspicion?

This gap overshadowed much of the political contest about the meaning of science, even if Marx's materialism came with softer and more coherent tones. Let me mention some of the streams of thought that were inspired by Marx and took hold in the prospering economics departments of the last decades of the 19th century. One that would have been impossible without the Marxian re-cultivation of the economic suspicion was the revival of the Who-question. Marx's conception of critique has incorporated, rather than avoided, the culture of the economic suspicion. If laissez-faire policies are not made In the Name of Science, but For the Sake of the Bourgeoisie, the question is: what kind of people make capitalism? Who are the bourgeois capitalists? Or more moderately, why does *homo oeconomicus* dominate capitalist culture? This question triggered a new series of writings: most notably are Veblen's *Theory of the Leissure Class* (1994 [1899]), a critique of the envy, emulation, and swank of the modern consumer who inherited far more premodern irrationalism than usually imaged; Max Weber's

Spirit of Capitalism (2005 [1904]), showing how protestant belief was decisive for homo oeconomicus to be self-constrained, and thus, Weber infers, successful; Sombart's *The Jews and Modern Capitalism* (1982 [1911]), repeating *the* voice of three millennia of European history of economic suspicion; and, not to forget, Schumpeter's *Capitalism, Socialism, and Democracy* (1994 [1942]) staring the heroic entrepreneur, with which Schumpeter turned Marx's depiction of the capitalist on its head, yet confirmed at the same time the break-down of capitalism.

Although these texts were written as contributions to economics, none of them made a contribution to the scientification of economics. Today they are perceived as part of sociology. After WWII, this genre fell for its political incorrectness. It had to give space to the discussion about an abstract notion of "rationality" as a normative principle of markets. In the same sense as in the later Marx the notion of the cruel character of the capitalist vanishes at the cost of the analysis of capital, so does the intuition of the *homo oeconomicus* vanish at the cost of the analysis of the market. In the case of those who still pose the who-question explicitly in a Marxian context – such as recently Boltanski and Chiapello in their *New Spirit of Capitalism*, (2005) – there is not even the question whether it could count as a contribution to economics. And the same is true if one indulges in such an exercise in the liberal tradition, as recently for example McCloskey, asking 'does capitalism make us virtuous?' (2006) The anachronism is blatant.

The political contest on scientific authority took place in two other schools of thought that were inspired by Marx's sense of the concrete: historicists, mainly on the continent, and institutionalists, mainly in the U.S. There was also the romantic school between Adam Müller and Othmar Spann, the discussion of which I leave for another occasion. The historicist school in Germany reaches from Wilhelm Roscher (1817-1894) and Karl Knies (1821-1898) to Gustav Schmoller (1838-1917), and the last politically more problematic historicist, Werner Sombart (1863-1941), whose spirit spread in the dawn of nationalism. These historicists came to be pejoratively titled *Kathedersozialisten*. While for Engels they were philanthropic vulgar economists the appraisals among historians differ. Some show how much they anticipated later innovations in economics (Streissler 2001). Others deny them to be historical altogether (Pearson 1999), or show the distinctness of the school (Hodgson 2001: 56 ff., Caldwell 2001). In England there was a more moderate historical school represented by John Ingram (1823-1907), and William Cunningham (1849-1919) (Coats 1954). In the U.S. the concern for the concrete flourished under the heading of institutionalism, notably Thorstein Veblen (1857-1929), and John Clark (1884-1963). Though they coined the beginnings of the AEA, they soon lost their influence under the pressure of marginalists in the first decade of the 20th century (standard reference goes to Rutherford 1994; for a rather heterodox account from the point of view of Dewey's pragmatism, see Amariglio and Ruccio 2003: 171-215).

The rise and fall of historicists and institutionalists are vital to the scientification of economics in that both schools coined academic writings for several decades without much reference to the authority of received political economists. The association with the Marxian tradition rather than the Scottish enlightenment, even if loose, was apparent. "The economy" did not mean a structure beyond interest, but described the historical reality of capitalism with its concrete market institutions. The intellectual efforts of institutionalists and historicists were always informed by a notion of the finitude, contingency, dependency, or, as one says today, embeddedness of "the economy". "The economy", just as in the oikonomic literature, is constituted by something that is in itself not economic. The market is, rather than a principle of society, an institution, and thus a matter of political design! Moreover, if the market is a historical reality, then this reality had a beginning and could possibly find its long-awaited end. Otherwise, even revolutions would not make a difference.

With these more or less explicit connotations, Marx entered the history of the scientification of economics during the decades around 1900. Without going into details, let me ask also for the reason of the phenomenological concern for history: What made the historical school historicist? Historicists certainly did not start from a philosophical defense of a Marxian conception of history. Instead, they were historicists simply by virtue of their painstaking local studies, for example, on weavers in Strasbourg between the 13th and 17th century (Schmoller 1879). The historicist came to articulate a methodological position only in the so-called *Methodenstreit* between Carl Menger and Gustav Schmoller in the 1880s to the result that they defended the position that economic theory should be *found on* economic history. Rather than defending a materialist conception of history, historicists argued in terms of a foundational empiricist position. The *Methodenstreit* had the effect that historicists came to be associated with "induction" as opposed to "deduction". If there had been any materialist aspect of their historicist stance, they should not have engaged in the debate in the first place. Marx had never entered such debate because for him the very difference of history and theory was inconceivable. Theory, for him is an expression of history.

Only after history and theory are opposed can history degrade to "historical-statistical *Kleinmalerei*", and "historical micrographs", as Menger polemically called Schmoller's work in his *Irrthümer* (1884: 37*). History became factual history: the "collection of dead facts as it is with the empiricists (themselves still abstract)" (Marx, *German Ideology*). Menger and his friends achieved to let history appear as something prior to meaning, a sequence of events that are only understandable as long as there is a conception of it, preferably in terms of causes, occasionally in terms of reasons. History merely shows "whatever specialissima of certain cloth-weaver guilds" (Ibid.: 40*), or the "meat prices of Elberfeld, Pforzheim, Mühlheim, Hildesheim, Germersheim, Zwickau" (Ibid.: 38*), but never the laws according to which these prices are formed. Instead, history for Marx is the source of the concrete, not because it was opposed to the abstract, but because it evokes our conception of it. Only as long as there is no history/theory divide, can history be telling, and shows itself rather than its surface data. Could Menger have also referred to later works such as Sombart's three volumes of *Der moderne Kapitalismus* (1902-1916) that could hardly be ridiculed as *Kleinmalerei*? Is not the very project of such a history a theoretical claim?

Accordingly, the victory of theory over history was not the victory of one method to obtain truth over another, as it is usually discussed (Mäki 1997). There was no contested truth. Instead there was a contest about the *meaning of truth*. The institutional result was thus not the rise of two different schools or paradigms, but the crowding out of historical studies from the economics department. Economic historians in England, France, and the U.S. – though for different reasons – moved in the first decades of the 20th century into other branches of economic talk. Braudel, Febvre, Bloch, and their friends did not rank high in the growing AEA. If historians are unwilling to distinguish economic and non-economic phenomena, how could they provide a genuine approach *in* economics, let alone contribute to its scientification?

A similar argument could be made for other contests about the meaning of science that accompanied the turn of the century, such as the contest about the divide between the institutional reality and the theoretical logic of markets, as well as the contest about the normative/positive divide. But I spare that discussion for another occasion. There is still a lot of historical work to be done on how the connotations of scientific authority have been politically debated. Let me merely give a hint of the intricacy of these negotiations by considering the notion of "positive economics".

From the point of view of the liberal tradition, one had to embrace "positive economics" insofar as it was opposed to the normativity when penetrating into the individual sphere. On the other hand, one had to be opposed to it because "positive" also meant *empirical determination*. "Positive economics" implied ontological transparency of "the economy", full determination, and thus the possibility of a *planned* economy. In order to avoid the science that prints out the plan for replacing government with scientific administration, one had to restrain oneself to "truth in abstract". Such is the confusion Marx has caused in the history of the scientification of economics: Positive economics had to be embraced in that it had to be free from ideology. But it also had to be rejected since it was associated with the planning of "the economy".

This confusion around "positive economics" was operative, and fatal for economists' scientific optimism. By means of this confusion, Marx resurrected the forgotten ambiguity inherent in the clarion call In the Name of Science – Laissez-faire! The ambiguity between arguing beyond any interest, but nevertheless in favor of a particular interest now takes the shape of supporting positive economics as far as it pushes beyond ideology, while keeping it at a distance in order to avoid ontological transparency that leaves no space to formulate liberal doctrines. The battleground, on which these connotations had to come to a point, was the socialist calculation debate, which I discuss below. The contest, to anticipate it right away, was not resolved until the formalist revolution, which met both requirements: it appeared to be beyond ideology *and* beyond determination. But before entering this debate let me survey some basics about the new genres in science that sprung from the liberal tradition.

Moderation, Separation, and the Liberal Retreat: For the Sake of Science – Calm Down!

Marx shared the belief that political economy is insufficient for capturing the new realities of the culture of capitalism with the economist he disliked most but nevertheless learned most of his economics from: John Stuart Mill (1806-1873). Mill, too, wanted to update political economy in light of the new predicaments of modern life. He shared the skepticism of the culture of capitalism. Think for example of his unforgettable: "It is questionable if all the mechanical inventions yet made have lightened the day's toil of any human being" (1998 [1848]: 129). Mill was an ardent reader of Thomas Carlyle, and shared his social concerns with his wife Harriet Taylor. He even embraced a "qualified Socialism" as long as it was democratic (2000: 191). Most honorably, he made these frank admissions to political economy after an exclusively liberal education from his father, and his father's friends including Say, Ricardo and others. Mill attempted to combine political economy with the social ethics (not the mathematics) of utilitarianism, and a considerably broad yet concrete notion of freedom. He was certainly more moderate in his political views than any other of his British colleagues before 1848 – the year he published his path-breaking *Principles of Political Economy*. About the changes in the second edition of 1849, he wrote:

> In the first edition the difficulties of Socialism were stated so strongly, that the tone was on the whole that of opposition to it. In the year or two which followed, much time was given to the study of the best Socialistic writers on the Continent, and to meditation and discussion on the whole range of topics involved in the controversy: And the result was that most of what had been written on the subject in the first edition was cancelled (2000 [1873]: 234).

Mill's *Principles* have been the most read book in the second half of the 19th century before it was replaced by Marshall's *Principles of Economics* (1890). Mill's contribution was to free the body of political economy from the political context in which it was stuck. He accomplished to re-codify the body of knowledge of political economy. His *Principles* actually *made* political economy a body of knowledge, and thus, as Schabas has argued more thoroughly, represents the very first conception of "the economy" (2006: 125 ff). He organized political economy around the categories of production and consumption – which related for him like "nature" and "will". Mill, as many after him, wanted to improve politics *and* science by means of *separating* them. Rather than the materialist denial of such distinction the master trope of the tradition that dominated the rise of economic departments was *separation*: the separation of the necessity of nature and the morality of justice, of the concrete and the abstract, of theory and application, and of science and art, as Jean-Baptiste Say must have told the young Mill when he came to study in Paris.

The theoretical structure of Mill's textbook coined all Principles in the rest of the 19th, and early 20th century. Even the textbook of historians, and to some extent of socialists obeyed roughly to this structure: (1) production, (2) distribution, (3) exchange, (4) a chapter on the "progress of society", and then separate from the rest of the text, (5) a chapter on government. These categories, sufficiently abstract yet rooted in concrete intuitions about the constituents of economic life, made generations of economists believe in the expressive possibilities of political economy – the science of the production and distribution of wealth. Today, these categories still transmit much of the general perception of what is going on in "the economy". Before Mill, these categories were not as distinctly perceived. Recall that in the oikonomic literature, for example, the difference of production, distribution, and exchange was a matter of whether one did one's homework or needs to rely on the trader. Only for lazy rascals, production and consumption was not the same.

With a body of political economy in his back, and its bad reputation in mind Mill initiated the battle for the epistemic grounds of scientific authority. In the political rough-and-tumble of mid 19th century it was no longer obvious what this authority really amounts to. The implicit image of science, and the mere reliance on Baconean and Newtonian metaphors was not sufficient to secure that authority. Until Mill it sufficed to say that political economy is "like" science. Now one had to acknowledge that it is just *not* like any other science. It is special. Thus, the questions arose: What is economic knowledge? What is the proper object of economics? What can economists really know, and what is beyond their scope? This was the birth of the philosophy of economics that lasted for about one century – the century of a new genre in the epistemic culture of economics: The *scope and method literature*.

Thinking of scope and method could be called the natural way of conceptualizing a discipline. It expresses the epistemic self-understanding of a scientist. Scope and method describe what scientists "have in mind" – the intentional correlate of their practice. With the question of scope one negotiates the perception of "the economy". In question are the "distinctions between phenomena that are of chief economic importance" (Keynes 1999 [1890]: 75). And with the question of method one negotiates the propriety of how one speaks about "the economy". While before this philosophical turn the scope of epistemic claims was implicitly agreed upon through the social context in which this claim was made (the trade of England), now, after economics was uprooted from its home ground around the British Parliament, the scope of economics became a *problem*.

Insecurity arouse around the common perception of "the economy", and the proper way to talk about it. The politization of economic science made it necessary to cut the edges of "the economy", since *too much* appeared to be put into the field: even revolutions! The scope and method literature was a defensive, or at least moderating literature. It lowered "the tone and temper of political thought", as Marshall commented by hindsight in 1897:

> General economic principles had to justify their existence before a court which no longer had any bias in their favor, and perhaps had some little bias against them. Consequently, they became less dictatorial, and more willing to admit their limitations (…) Much must be taken on conjecture (117).

Moderating one the one hand, as long as the scope of economics was contestable one thought of "the economy" as a particular domain, and thus as an institution, rather than a principle of society. There only could be a scope and method literature as long as an institutional reality of "the economy" was perceived. In Mill's world between 1850 and roughly 1900 there was, therefore, not yet a clear difference between political economists and institutionalists. As a consequence, it would be shortsighted if I neglected the political connotations with which economic knowledge was characterized. The line between *doxa* and *episteme* was politically contested, though revolutions, everybody agreed, belonged to the former side.

Let me shortly recall some essentials of that literature. It started with Mill (1874 [1844]), had its heydays in Robbins (1972 [1932]), and found an end in Becker (1976). It circles around the separation and combination of the concrete and the abstract, of induction and deduction, of theory and application, and of science and art. Mill's *Definition of Political Economy; and on the Method of Investigation Proper to it* (1844) made the beginning. It was first published in 1836, and attempted to rescue Ricardo's high flights in abstract reasoning with some empirical flavor. But hardly anyone embraced this flavor full-heartedly. After Mill's essay in the 1850s, quarrels arouse between the old Nassau-Senior (1790-1864) together with Richard Whately (1787-1863) against William Whewell (1794-1866) about deduction and induction – though even the inductivist Whewell was critical about the leisure empiricism of Mill (Whewell 1849). Other texts that took up Mill's discussion were Cairnes' *The Character and Logical Method of Political Economy*, arguing that "no economic or social truth, meriting the name of scientific, ever has been discovered by such [inductive] means, and it may be safely asserted, none ever will be." (1888 [1857]: 79). Favoring the deductive method, Cairnes argued that the political economy of laissez-faire merely amounts to a theoretical exercise, not an actual political claim (in Coats 1954: 146). John Neville Keynes' *Scope and Method of Political Economy* rescued Marshall by moderately arguing for an 'unprejudiced combination' of art and science (1891). And Carl Menger's *Investigations into the Method of the Social Sciences* (1883), which caused the *Methodenstreit*, initiated the exclusion of the concern of the concrete.

After revolutions and wars, a later wave of this contest took place in the 1930s when Robbins wrote his *Essay on the Nature and Significance of Economic Science* (1932) – with his astonishingly successful definition: "Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses" (16). Against Robbins, Hutchison took the positivist stance in *The Significance and Basic Postulates of Economic Theory* (1938). Though at its surface, the issue was about positivist and abstract reasoning, it also increased the conflict between an institutional perception of "the economy" and scientific authority. Robbins shifted attention away from Marshall's partial (thus institutional) market analysis to Lausanne's general market analysis – although, as we will see below, he argued only two years later against the socialist use of Walras's GET. By hindsight, Robbins essay was successful not because economists came to an agreement about the scope of their discipline, but because this question began to loose relevance. Hutchison did not contribute to the red color of positivism, and Robbins' success may be due to its being the last of its kind. The more economists referred to Robbins, the less it meant something particular. In his *Three Essays – the* bible of economists' formalism – Koopmans would later refer to both Robbins, and Hutchison supportively (1957).

The scope and method genre is definitely past today. In the course of the following decades, Robbins' definition would become a placeholder for the scope of economics beyond the perceptibility of scope. The anti-philosophical attitude that, as I will show in the next part, was constitutive of the formalist revolution in the 1950s undermined the grounds on which one could discuss scope and method. Such attitude is most apparent in Friedman's much-read *Methodology of Positive Economics* (1953), and, even more popular, in Becker's *Economic Approach to Human Behaviour* (1976). Becker rejected the very question of a scope of economics (since economics is itself a method), and Friedman rejected the question of a suitable method for discovering truth (since economic knowledge is instrumental). Friedman and Becker could thus be said to mark the end of the contests about referential truth in economics. Since the 1970s, one can be a successful economist even if one does not have the slightest interest, let alone opinion about the epistemic character of one's claims (one can refer calmly to specialists in economic methodology). Moreover, one can be a successful economist without having any perception of what it is that is going on in "the economy". It suffices is to believe that There Are Decisions Made – whatever that means. In short, the scope and method literature was initially meant to make explicit the image of "the economy" as a particular social institution, but it ended up supporting the image of the market as a theoretical principle of society.

As an illustration of this degeneration of philosophical contestability let me discuss shortly the destiny of Mill's main doctrine: the "method a priori", and its principle of the "pursuit of wealth". In terms of the passage from the *oikonomia* to "the economy" Mill's considerations represent the move from *phronesis* to instrumental rationality, the nature of which accompanied the scope and method literature down to Robbins and Becker. When Smith wrote about the *homo oeconomicus* he thought in terms of virtues, phronesis being one of them. Now instead, phronesis came to be used as a principle for science beyond the reference to particular kind of people. Mill set forth the *pursuit of wealth* as a *deductive* principle of economic science reflecting the deepest credo of modern life projected to a principle of mankind: *to want*.

Recall that the pursuit of wealth in the *oikonomic* literature was possible only insofar as one is able to keep in mind what wealth is good for. Phronesis was the ability to see wealth in light of one's needs. The greedy usurer was beyond practical wisdom because he lost sight of the meaning of wealth. For the usurer the difference of means and ends is just a matter of which side of the coin one looks at. Means could be an end in themselves just as ends can be means for yet another end. The contested line was, of course, where "mere" wealth ends and "truly" human *eudaimonia* begins. For this reason one quarrelled about the nature of wealth in opposition to those who believed it to be All There Is.

Mill marks the end of this discussion when posing *the pursuit* of wealth (rather than wealth itself) as a deductive principle. Wealth is disassociated from a notion of material needs, and comes close to a notion of material means in general – *whatever* is the end. With Mill, a substantive notion of wealth was substituted with a principle of human motivation. Instead of discussing what wealth is, the issue became whether the deductive method of the "pursuit of wealth" leads to "truth", or only to something "like" truth. Mill's epistemic culture could flourish at the cost of avoiding a question – moderation.

This moderation is present in Mill's notion of deduction. Deduction prior to Mill (in North, Whately, and Ricardo) did not *prevent* from an actual claim, but simply was the way to make a claim. Instead, for Mill deductive knowledge was merely second best. Why? Since he no longer conceived an inherent nexus between various human motivations (like the conditioning of desires by the meeting of needs), Mill spoke of the pursuit of wealth as *one* motive *next to* others. Man not *only* pursues wealth, but his motivations are *complex*. With Mill political economy became cautious, moderate, and aloof of making a claim about motivations that were still so vital for anyone in the 17th and 18th century. Too direct claims were not permissible because economic reality that results from economic life is *complex*, and full of "disturbing causes", as Mill expressed himself. Hence there was the *method a priori*.

> Since, therefore, it is vain to hope that truth can be arrived at (…) while we look at the facts in the concrete, clothed in all the complexity with which nature has surrounded them, and endeavour to elicit a general law by a process of induction from a comparison of details; there remains no other method than the a priori one, or that of "abstract speculation" (Mill 1844: 148).

Marx also used the word "speculation" in precisely the same context: "where speculation ends – in real life – there real, positive science begins: the representation of the practical activity, of the practical process of development of men." (*German Ideology*). Marx and Mill seem to agree on terms. But precisely where science in Mill holds still, it begins in Marx.

But such does not do fully justice to Mill. He had a great sense of what he called "disturbing causes", and most of his texts are concerned with them. How else could he express his worries about the human miseries of his time? His *Principles* did not exclude these disturbances, and he believed that its success was due precisely to this sensibility, as he wrote not without pride:

> It [the *Principles*] was, from the first, continually cited and referred to as an authority, because it was not a book merely of abstract science, but also of application, and treated Political Economy not as a thing by itself, but as a fragment of a greater whole; a branch of Social Philosophy, so interlinked with all the other branches, that its conclusions, even in its own peculiar province, are only true conditionally, subject to interference and counteraction from causes not directly within its scope (Mill 1873: 236).

Mill's method a priori was not designed in order to prevent him from a concrete claim. It was designed in order to arrive at a claim with the caution matters deserve, and without loosing sight in the disturbances of a world that calls too quickly for revolutions.

However, as a consequence of this separation of the abstract and the concrete – and of political economy and social philosophy – his notion of "disturbing causes" could also serve as an excuse *not* to engage in these turbulences *at all.* The pursuit of political economy was disassociated from making a concrete claim just as the pursuit of wealth was disassociated from the meaning of wealth. In short, the method a priori was initially a call for caution, soberness, and prudence in the rough-and-tumble of revolutions – a symbol of the respect for the concrete. Yet it backfired on the concern for the concrete.

Until the last decades of the 19th century, Mill's separation of science and art worked as a warning against the misuse of the authority of science, though art was nevertheless a necessary part of the full ethos of economists. Later the ethos of the economic scientist excluded art – that is, political, applied, normative, institutional, historical, or any other interpretive concerns. Beyond the separation for the sake of the concrete there arose an epistemic *hierarchy* of science and art, of the pure and the applied, of positive and normative, of theory and history, or even of 'method and ideas', as Edgeworth celebrated mathematical economics (1889: 541). In the decades between the marginalist revolution and the beginnings of the formalist revolution the epistemic distance that describes the ethos of economists was realized in these hierarchies.

With this step from (moral) phronesis to (deductive) instrumental rationality Mill's essay anticipated much of what happened in the decades around 1900 when marginalism settled in the institutions of economics. Notions of the scope and method of economics became evermore weakened in favour of greater scientificity. This was the time when political economy became economics, that is, freed itself from its "political" conditions. The marginalist revolution and its aftermath could expectably be *the* episode in a social history of the scientification of economics. Historians have granted it much attention (path-breaking Mirowski 1991). There emphasis is put on the role of other sciences such as psychology, logic, mathematics, and physics, which was indeed crucial for scientific optimism around 1900. This period is all the more interesting since it describes the professionalization of economics, as thoroughly researched by Coats (1993). Around the turn of the century academic journals emerged, which soon replaced the writing of treatises. It is common sense among historians to argue in one or another way for a degeneration of economics in this period that Veblen has labeled "neoclassical" – evoking therewith an idyllic image of political economy. Historians tend to argue that since the 1870s economics slowly went off track. Most expressively by Mark Blaug: "neoclassical economics indeed achieved greater generality but only by asking easier questions" (1996: 282). But what, given my previous remarks, was really new?

In the history of economic theory, the marginalist revolution is called a revolution because it replaced the theoretical structure of Mill's *Principles* (production, consumption, exchange, etc.), with a unique principle of constrained maximization (of "utility"). Rather than the style of reasoning in marginal terms (already present in Ricardo and others), the path-breaking shift was that away from a clear perception of the constituents of "the economy" to a unique conceptual logic of economic theory. Economic theory was no longer designed in order to represent the "laws of the phenomena of wealth" (Cairnes 1888: 35), but in order to represent *value* in *prices*. Since Jevons, population, production factors, land rentals, subsistence rates, and all that was perceived as the scope of political economy, came to be *given* – that is to say shifted beyond scientific concerns. When Jevons represented labor as consumption with a negative sign, it meant the knock out for the possible expression of class-consciousness, or for that matter, the question of what-kind-of-people.

By virtue of a steady dematerialization of economic life the marginal revolution gave way to the mathematization of economic theory. Revolutionary in one respect, early neoclassicals continued the steady "denaturalization of economic order", in terms of which Schabas has discussed this dematerialization (2006). Late 19th century mathematical enthusiasm accomplished Mill's task of sober separation – namely, to make it "impossible for the educated economist to mistake the limits of theory and practice, or to repeat the confusions which brought the study into discredit and almost arrested its growth," as Foxwell celebrated mathematical economics in 1888 (in Edgeworth 1889: 552). In 1890, Simon Patten, later a president of the AEA, argued that economics should replace mathematics and physics in general education, and that moral and political thought should be separated (in Siegfried, Hinshaw 1991: 373). In the following decades, however, such demands had one great hurdle to take: the association of mathematical precision with scientific socialism. This association, which I discuss in a moment, hindered the depolitization of economic theory and represents the last remedies of a concrete intuition about "the economy".

According to the general outlook of my phenomenological narrative, I can only downplay the scientific efforts that have been invested in the scientification of the theories of values. The psycho-physiological branch of the marginalists (above all Francis Edgeworth, 1845-1926) took their enthusiasm from their readings of Helmholtz, Wundt, Delboef and others. The hope was to provide scientific substance to the law of diminishing marginal utility – precisely as today neuro-sciences make some behavioral economists believe to provide scientific substance to (ir)rational choice (see following chapter). These debates, however, were never contested for matters of an anthropological intuition of homo oeconomicus. Edgeworth may have read Veblen on the weekend without associating it with his own work. Scientific efforts rather resulted in a steady thinning out of substantial claims (for an classic account see Wong (2006 [1978], more detailed Mirowski and Hands 2006). From cardinal to ordinal to revealed preferences, from psychology of utility via the logic of choice to the sets of convex preferences, these innovations were written in the spirit that was anticipated by Jevons:

> The food which prevents the pangs of hunger, the clothes which fend off the cold of winter, possess incontestable utility; but we must beware of restricting the meaning of the word (utility) by any moral considerations. Anything which an individual is found to desire and to labor for must be assumed to passes for him utility. In the science of economics we treat men not as they ought to be, but as they are (Jevons 1879 [1871]: 41)

Becker did beware restricting utility to 'clothes fending off the cold of winter' when speaking of children as utility. Since scientific authority was gained by means of *not* presuming a particular understanding of "utility" I may seriously ask: has there ever been any utilitarian economist other than Mill? The scientification of the theories of value moved economists away from the social philosophy of utilitarianism. More surprisingly, it neither moved it closer to the psychology department. Why else could institutionalists, who did not write "psychology" on their forehead, challenge the poor psychological foundations of marginalists, who did? (Veblen 1961)

Since scientification between the marginal and formalist revolution did not take either road via the psyche, or via moral intuitions, since economists thus never wanted to prove to be bad anthropologists – as behavioral economists assume when proudly observing that people do not behave rational – this scientification found its theoretical expression not at all in a different notion of economic life, but in the degeneration of the perception of "the economy". Decisive in this time of prospering economic theory are thus rather the changing connotations surrounding "the economy". This was the locus where all fruits of scientific optimism could be earned. The fall of the psychological imaginary behind the Theories of Value prepared the ground for the Theory of Prize as *the* theoretical problem of "the economy". There the actual political battle for scientific authority took place.

The Socialist Calculation Debate and the Diminishing Weight of Meaning of Economics

The battlefield, where the political connotations of the theoretical perception of "the economy" were fought for, was not that of the nature of economic behavior, but the *socialist calculation debate*. It was *the* battle *for* science. The question of the scientific determinability of "the economy" employed for socialist policies stands in the background of most theoretical innovations in economic theory for the first half of the 20th century. The open clash of ideological and scientific motives in this debate make these decades vital for a social history. Most scientific optimism in economics of that period came to be expressed in this debate. "Market socialism", as it was called, was the most ambitious project of the political utilization of science: science makes the market, and replaces competitive production with administration, which would amount to an end of the horrors surrounding the culture of capitalism. The debate stands as a monument for both, high modernism of economics between capitalism and socialism, and at the same time for the very degeneration of a possible scientific resolution. The debate was never resolved, but interrupted – 1945. It is worth recalling the debate in some length since it sets the scene for the following part. The debate starts with the time when positivism gained reddish shades, and ends with the colorlessness of the formalist revolution.

Allow some preliminary remarks. What is said to be the core of the debate is the challenge liberal Austrian economists have perceived when leftish inclined positivists claimed that market theory could be utilized for socialist purposes. Today, the debate is still the hobbyhorse of some Austrian economists. Boettke has compiled an extensive documentation (2000). The lessons to be drawn are still open to debate. Don Lavoie has offered an Austrian-biased interpretation (in Boettke 2000, VI), while others have reformulated the socialists' stance against Austrians (for example O'Neill 1996). For Austrian economists the debate was an initiating moment to the extent that they distanced themselves from scientism, and as a result marginalized themselves in the decades after the war. Austrians remained literary economists. For this reason their liberalism today stands somewhat aside from the rest of the profession.

In the Austrian community the debate echoed long after the rest of the profession already had forgotten it. Even Marxists of the 1960s, for example, hardly relied on the same scientific optimism of their pre-war predecessors. Most of them cultivated the same anti-scientism that Austrians have acquired with their "subjectivism" during this debate. This results in a highly irritating vicinity of the so-called "postmodern" Marxists and the epistemology of some neoliberals. I consider this vicinity as symptomatic for the subtle influence of economics in today's economic talk. The road to post-modernism via the neoliberal critique of scientism has not been sufficiently considered by those who feel as though they make a contribution to cultural criticism by announcing the plurality of knowledge. Considering the following debate, one may seriously ask: Is post-modernism the new hermeneutics of capitalism? The subjectivism of Austrian economists, including their flirtations with scholars such as Alfred Schütz, also made me consider Austrian economics when starting this project (see e.g. Lachmann 1991, Madison 1994). Today my presentation of the debate is rather informed by the works of Steele (1992), O'Neill (1996, 2006), Mirowski (2001), but also Caldwell (2004).

The socialist calculation debate was not only one but a continuous debate spanning through various decades and places. After first flaring up in Pareto's Lausanne, the two main trouble spots were Vienna of the 1920s and Chicago of the 1930s. Additionally, the unity of the debate is questionable because the issue of debate – like in all good debates – was part of it. On the surface matter was theoretical, but underlying was the contest about the meaning of economic theory – and thus the political ethos of economists. As Mirowski has put it, "competing conceptions of the very nature of 'science' were at stake in the controversy, and not untrammeled Luddism, although this passed unrecognized at that time" (2001: 237). At stake was not the truth of "the economy", but the meaning of this truth. Is economic truth something *by means of which* one achieves something, or is economic truth something *in the name of which* one takes a political stance? This contest was interrupted by 1945. But the theory went on. For this reason the debate hardly entered textbooks and most students of economics have never heard of it. The techniques that evolved from the debate, however, all students know. They entered without change into a McCarthy-clean environment of the years after the war.

Socialist scholars also may not know a great deal about the debate. For the socialist prominence in the first half of the 20th century did not play a major role in it. They were busy with their ongoing revolution, power politics, and transition plans. For such activities they did not need the vehicle of scientific optimism to motivate themselves. When the revolutionaries had to face the task of economic design – Lenin's New Economic Policy and Stalin's Five Year Plans – the notion of installing the economic heaven of the class-less society was quickly crammed into the lower drawer. Nationalization is anyhow not the same as socialization, as Kautsky understood early. The socialist prominence was quick in agreeing on a slow passage to communism that includes (themselves as) governors. Russia, for example, first had to be "civilized" for socialism, so that the ultimate plan of the classless society was never printed. Economic plans were political plans. They never substituted politics.

The issue nevertheless touches at a sore point of their revolutionary spirit: how to actually design the administrative institutions of a socialist system, which are, in the end, supposed to replace the exploitative culture of competitive production, if not money itself. Marx's question that formed the backdrop of the debate was this: "What social functions will there remain, then, analogous to the present functions of the State? This question can only be answered with the help of the scientific method" (in Boettke 2000, I: 301). Or more concretely, as Engels wrote in a letter to Marx in 1855, how is it possible to arrive at an "association of free and equal producers acting consciously according to a general and rational plan" (Marx, Vol. 13: 241-242). The debate, however, was not "Marxist", as Arnold has forcefully argued (in Boettke, IX). For a historical materialist talk about markets is meaningless in socialism. Revolutions change terms on which society is understood to such extent that it is futile to discuss it on the grounds of bourgeois science. No discussion. Marxists believed, like Engels, that socialism is a science, not that bourgeois economic science can serve socialists, which was the issue of the debate. For socialists, such issue is attractive and risky at the same time; attractive since one can reclaim science, but risky since one would prove that economic science is not bourgeois. These two aspects will be fatal for those holding strong positions in the debate.

Socialist quarrels about economic policy, moreover, were hardly informed by scientific optimism. Bukharin and Trotzky did not quarrel about science, but about farmers – though Bukharin does refer to Mises on the occasion of explaining NEP (in Boettke 2000, I: 593). Socialist prominence had no time to discuss the scientific possibility of socialism. They had to make it – which took a long time. If they touched the problem at all they did not show scientific optimism, but mainly scientific naivety. When it comes to the point to get rid of the capitalist system, as Lenin dreamt, it will happen just like that, "immediately overnight", only with the "knowledge of the four rules of arithmetic, and issuing, appropriate receipts" (in Steele 1992: 68). Simple accounting – like in a firm, like in an oikos – suffices to run the classless society. Also Bukharin's *ABC of Communism* does not exceed basic arithmetic. Perhaps after educating the working class to socialist managers proper, Bukharin suggested, "the economy" will run just fine (in Boettke, I: 427f.). Social democrats in the west, like August Bebel, shared this naivety when speaking about the feasibility of statistically determining who gets what: "when some experience has been gained, everything will run smoothly" (in Steele 1992: 59). It remained a dream. Apart from the early 1930s, the Russian plans never performed 'at least as good if not better than' "free" markets in the west, which was the core claim of the debate.

The debate, therefore, largely remained within the rising institutions of economic science. So what was the issue? It was the possibility of an economic organization of collectively owned means of production: expropriation and regaining surplus value that makes an end to the exploitative competitive culture of capitalists. By taking control of production, moreover, one has the possibility to distribute national income according to needs rather than productivity. This was the dream in socialist terms. Regarding economic theory let me present the issue in terms of the framework that slowly evolved from this debate, namely Walrasian general equilibrium theory. Walras represented "the economy" as a set of equations that are all simultaneously solved by one price vector. He designed this theory as a theory of pure competition. Was it possible to utilize a model of competition for socialist purposes? Could this inclusive conceptual framework of GET justify the design of "the economy" by statistical determination? Is it possible that one "fills in" the quantities in all equations, and fully represents "the economy"? Could the government act as a Walrasian auctioneer altering prices to reduce excess demand? The benchmark was whether this could also be efficient in the sense of equal marginal productivity of all factors – no waste. Hence, was it possible to "choose" a desirable distribution of income that is just as efficient as competitive markets? Roughly said, is it possible to replace the invisible hand with a sophisticated bookkeeper?

The first flaring-up was in 1908 within Pareto's Lausanne. One of Pareto's followers, Enrico Barone (1859-1924), took seriously a remark of his master that relative prices are merely 'accounting devises' (in Boettke 2000, II). Could a statistically sophisticated government not play this accountant? If so, Barone argued, government would be subjected to the same principles, as laid down by Pareto and Walras for the case of free competition:

> From what we have seen and demonstrated hitherto, it is obvious how fantastic those doctrines are which imagine that production in the collectivist regime would be ordered in manner substantially different from that of the 'anarchist' production. (…) [A]ll the economic categories of the old regime must reappear, though maybe with other name: prices, salaries, interest, rent, profit, saving, etc. (289)

Thus, revolutions do *not* change the terms upon which one theorizes "the economy". Socialism and capitalism are formally equivalent. Barone did not take this point further by asking whether or how a collectivist state could be as efficient as capitalist "anarchy". Given Pareto's skepticism that governments always cause inefficiency, and the provincial milieu coined by personal tensions between bourgeois Walras and reactionary Pareto, this little quarrel could not break out into a battle about the big system – the Russian reality of which was still to come.

The actual debate took off in the intellectual milieu of Vienna after WWI. There, in a rather contingent way, a mixture of ideas and traditions came together in such way that positivism took on shades of red – which is the virulent joint of the debate (see Caldwell 2004: 100f, and for an illustrative description from the point of view of Oskar Morgenstern, Leonard 2008). Austromarxism was big in the "first republic" of the interwar period. Otto Bauer, Max Adler, and, somewhat aside, Rudolf Hilferding pushed Marxism in academia. But austromarxists neither participated in the hype of positivism, nor referred to Walras. Walras's GET became known in Vienna, rather, through Joseph Schumpeter and his 1908 *Wesen und Hauptinhalt der theoretischen Nationalökonomie.* Schumpeter presented Walras' GET as a benchmark for scientific authority, though not because of political reasons. He knew that Walras did not express his political intuitions in his Pure Theory, since he also read his sociological writings. Schumpeter was appealed by Walras' rigidity, and disassociated Walras from his political intuitions. As a result, Walras's socialist ideas never found inroads into the calculation debate.

> (S)o far as pure theory is concerned, Walras is in my opinion the greatest of all economists. (…) Unfortunately, Walras himself attached as much importance to his questionable philosophies about social justice, his land-nationalization scheme, his projects about monetary management, and other things that have nothing to do with his superb achievement in pure theory (1954: 827f.).

The first, then, who gave positivism its fully red color was the political philosopher of science Otto Neurath (1882-1945). "Of all attempt at creating a strictly scientific unmetaphysical phsyicalist sociology, Marxism is the most complete", he wrote about the "Scientific Content of History and Political Economy" (Neurath 1973 [1931]: 349). Without reference to Walras, the experiences of economic planning in WWI made him believe that also in peacetime socialist planning is the way to go (in Boettke 2000, I). Neurath actually argued for the replacement of money with an "economy in kind", as also Max Weber discussed earlier. Instead of money, one could count in "real units", the nature of which is statistically determined in accordance with the "material" needs of people – do we hear the echo of Aristotle's belief of just prices "in accordance to needs"? O'Neill has made a strong case for reviving Neurath's argument in that it presupposed commensurability, which is a matter of individual rationality (1996). But the debate did not continue on this level. For nobody else would touch the distribution of consumer goods, and thus consumer sovereignty, so bluntly.

Such claims, and certainly the actual practice of socialist planning in Russia provoked liberal economists to draw a clear political line between economic theory and socialism. It particularly provoked the young Privatdozent Ludwig van Mises (1881-1973) to write in 1920 what some call the centerpiece of the debate: "Economic Calculation in the Socialist Commonwealth" (in Boettke 2000, II). What later would come to be known as Mises' (praxeological) apriorism, finds here its first political expression. He made a simple theoretical point: "Where there is no free market, there is no pricing mechanism; without a pricing mechanism, there is no economic calculation" (Ibid.: 111). Without a private ownership economy one cannot possibly speak of efficiency because there is no ground on which one could assess it. Even if one allows for a consumer market, money does not play the same role in a socialist economy. Economic planning under collective ownership is necessarily arbitrary. "There is only groping in dark. Socialism is the abolition of rational economy" (Ibid.: 110). Rationality of "the economy" is preserved to the "anarchy" of free markets.

> Without economic calculation there can be no economy (…) Historically, human rationality is a development of economic life. Could it then obtain when divorced therefrom? (…) [I]n place of the economy of the "anarchic" method of production [capitalism], recourse will be had to the senseless output of an absurd apparatus. The wheels will turn, but will run to no effect (Ibid.: 105-6).

This was perhaps the only time in the history of economics that "the economy", the market and liberal policies were explicitly equated – an equation that was always both operational and fatal for claiming scientific authority.

Next to this aprioristic argument, Mises also referred to two auxiliary points: complexity and incentives. He spoke about the "longer roundabout processes of capitalistic production"(Ibid.: 101), which are too complex to calculate – though in a single oikos, he admits, it is possible. Moreover, without being paid according to productivity workers would not care about efficiency, that is to say, they would be lazy. These arguments are weaker in that they refer to the practicality, not principal possibility of calculation. For who knows what happens to technology? And who knows if workers in socialism are happy to work efficiently without being paid according to their productivity. But the main point remained: no authority can mimic the unique rationality of "the economy". This rationality simply does not appear without the freedom of competition. And so Mises closes by saying that he will not deter "culture socialists" who aim at the "dissolution of the most frightful of all barbarisms – capitalist rationality" (Ibid: 130).

During the 1920s, Mises' argument was by and large accepted in Vienna. The contributions during this time, however, showed great attention to the practicality as well as reality of socialist planning. Boris Brutzkus gave a thorough account of why the NEP, and the Five Year Plans were doomed to fail (Boettke 2000, III). In 1935, Hayek compiled continental contributions in an English edition (Boettke 2000, II). There he referred to the complexity of the solution of "hundreds of thousands (…) simultaneous differential equations, a task which, with any means known at present, could not be carried out in lifetime." (Ibid.: 212). Hayek's opponents would later continuously quote this statement as well as Lionel Robbins, who argued similarly in his analysis of *The Great Depression*. There, Robbins showed more clearly the politics behind his a-political 1932 Essay (in Boettke 2000, V). Attention thus moved away from Mises' apriorist argument – or at least so it seemed for its opponents.

Let me keep track of Walras' model. Its actual revival happened in Vienna during the same time in the 1930s, but not in the context of the calculation debate. Mises was reading Carl Menger's economics, not Walras'. The revival of Walras happened in Karl Menger's mathematical colloquium (Karl was the son of Carl). There, economists like Oskar Morgenstern, Abraham Wald, and Karl Schlesinger were reading Cassel's reformulation of Walras (1932 [1918]), and specifically the critique of Frederik Zeuthen and Hans Neisser that there may be negative prices. Wald presented a first proof of the existence of an equilibrium of Cassel's equations, and in 1937 von Neumann's path-breaking topological proof of an equilibrium in a growth model was published in Menger's proceedings (see e.g. Punzo 1991). Although Walras stood in the spotlight of this group, their mathematical spirit dominated positivist aspirations – and thus the possible political ambitions they had. Menger's group was less expressive of its politics than the rest of Vienna. The rather religious Abraham Wald hardly has thought of this proof as an expression of his political inclinations. With the *Anschluss* of Austria in 1938 the colloquium had to give up. On that day, Schlesinger, who brought up the problem of existence, committed suicide while others celebrated.

The times on the Continent were too virulent, such that the debate could have continued on Viennese parquet. The quarrels continued in the 1930s on safer ground in the U.S. There, Mises' argument about the lack of method to determine an efficient allocation in socialism was seriously challenged. In Walras' terms, to whom reference was made more frequently, the *tatonnement* process to arrive at an equilibrium was tackled. Also the notion of "solving equations" moved to the fore. The contributions – this is still one of the contested points – took place somewhere in-between Mises' aprioristic and empirical arguments. More important, the line between socialists and opponents was less clearly drawn than on the continent. Rather than a debate on the theoretical foundations of politics, one negotiated the political meaning of theory. This shift of debate was accompanied by several confusions regarding the level of argument. Accusations and cross-accusations of misunderstandings widened the gap between the theory discussed and the implications drawn.

Already the first U.S. contribution is stunning in this respect. Frederick Taylor was a liberal economist, but argued in his Presidential Address of the AEA – as though a mere theoretical exercise – for the possibility of socialist planning. On the base of evaluation tables of primary factors, government can plan production as efficient as in a free market. Moreover, government has the additional tool of distributing income, which it could apply freely according to both productivity and needs. In 1933, the socialist H.D. Dickinson provoked with the claim that the plans of the Superior Economic Council, as he called it, can perform "at least as well as, if not better than, under capitalism" (in Boettke, IV: 34). Dickinson, as later elaborated by Oskar Lange, argued for a trial and error procedure to arrive at equilibrium prices. One may even start with randomly chosen prices, he argued. Dickinson also referred to the issue of statistically determining demand curves, which would come to be the central theme around which the econometrics society was established. A reply by Maurice Dobb, and a rejoinder by Abba Lerner followed, discussing mainly the terms of the discussion. The nature and place of consumer sovereignty also became contested after Dobb's article. He brought up the possibility that the equilibrium may not be unique. And, to mention another issue, the role of advertisement as a source of inefficiency played a role, too.

Re-reading these early U.S. contributions one can observe a rising awareness about the theoretical problem alongside a rising confusion about its implications. On the one hand, the theoretical discussion gained sophistication, though on the other the polemic about each other's positions increased equally. There was a clear gap between the sober tone of presenting the theory and the polemics that surrounds it. As compared to the scholarly essay of Mises in which theoretical argument and theoretical interest were the same, there was an increasing mismatch of tone and core of the debate. Although the marginalist principles that characterize an equilibrium (equal marginal productivity of all factors) gained sharpness, the dividing line between "the elements which are common to both societies from those which belong only to one", as Lerner puts it in 1934, was blurred by polemics (in Boettke 2000, VI: 48).

What was actually negotiated was less the theoretical, or practical, possibility of scientific planning. The very relationship of economic theory and political meaning became problematic. The question moved ever farther away from how to manage collective production to a *corroboration* of the marginalist principle of efficiency. Lerner, for example, argued that even if dictators determine consumption a market mechanism is necessary: therefore economic theory does apply to socialism. Along the same lines, Frank Knight in 1936 argued that marginal economics has the same function in socialism and competitive individualism:

> [T]he problem[s] of collectivism are not problems of economic theory, but political problems, and (…) the economic theorists, as such, has little or nothing to say about them. (…) For the principles of marginalism are the logical, mathematical, and hence universal, principles of economy (in Boettke 2000, IV: 9f.).

And Lerner began writing more playful about the political issue. He wrote that the socialist authorities could conceive of Dickinson's suggestion as a "subtle scheme of sabotage for building the socialist society on rotten foundations" (Ibid.: 48).

Given this depolitization of the debate, the actual issue at that stage was a negotiation of the economists' ethos in relation to politics. This locus of argument was most explicit in Paul Sweezy's essay of 1936, "The Economist in a Socialist Economy". He argued that the economist is "likely to acquire unprecedented usefulness and prestige" (422) and will "gain heavily from the victory of socialism" (426), at least as compared to the invisible hand economist in capitalism. Sweezy pointed to precisely that problem which I have observed at the very initiation of economic science between nobody's interests and special interests:

> This very concern with the public interest has left them [economists] to a large extent without a job. (…) Either the pursuit of private interests actually is made to serve the public interest through the working of competition, in which case is nothing left for the economist but to pronounce a solemn Amen, or it is not, and the economist's advice is necessarily ignored (425).

Only in socialism, therefore, economists could make a difference. For the same reason Hayek believed that science in its attempt at being relevant is biased toward socialism (1949). Socialism is more able to satisfy intellectual needs than liberalism. But Sweezy and Hayek did not anticipate that the spirit of social(ist) engineering was about to merge with the liberal doctrines of the west – the scientific engineering of liberty that was about to come.

As convoluted as the debate was, by the time it came to Oskar Lange's two publications in the *Review of Economic Studies* in 1936 – reprinted with Taylor's speech as the *Economic Theory of Socialism* (1938) – confusions calmed, and the ball was on the socialist's side. Lange's two essays were perhaps the only contribution to the debate that indeed nourished the political belief in the feasibility, rather than merely theoretical possibility, of economic design – though he neither added new ideas, nor was his model ever put into practice. Yet Lange was celebrated as the pioneer of actual socialist planning. This may be due to his astonishing biography. After emigrating from Poland, taking a chair in Chicago, and being naturalized as U.S. citizen, Stalin invited him for economic advice (see Steele 1996: 153ff).

Decisive for Lange's success, methodologically speaking, was that he presented the Walrasian framework as an actual description of "the economy". Lange referred to Walras as the father of his scheme, popularized the view that Barone has anticipated Mises on theoretical grounds, and believed that he himself has proven the practicality of calculation. He provided a scheme how the Central Planning Board can simulate the market for factor prices. Crucial for him was the characterization of factor prices as "terms on which alternatives are offered" (in Boettke 2000, IV: 116), an "index of alternatives" (117), which warrants the formal equivalence of prices in both political systems even under absence of competition. The only rule of the Central Planning Board is to alter prices like a Walrasian auctioneer without interfering in the choice of quantities. Then, according to Lange, the market is simulated to such extent that Mises' groping in the dark comes to an end. Shortages and surpluses could be observed, and via the trial-and-error process equilibrated. The simulated factor market is "surrounded" by the consumer market, and therefore dependent of and determined by the equilibrium in this market. Then socialist planning can even perform better than a capitalist society because there would be no inefficiencies due to monopolies, and no crises due to business cycles. Both refer to genuine Marxian characterizations of capitalism. Monopolies and crises were associated with capitalism, and not, as they are today, with "market failures". I come back to this fundamental change of meaning in a moment.

After the success of Lange's model the debate calmed in the face of WWII. Mises was of the days past. Lange had such success that even Schumpeter believed Mises to be defeated (1994: 172 ff). But on which grounds did Lange refuse Mises? On the theoretical possibility, or practical feasibility? Was the problem that of complexity, or that of conceivability? A later remark of Lange that is often quoted suggests that he dealt with complexity since he conceived of the computer as a substitute for the tatonnement process: "The market process with its cumbersome tatonnements appears old-fashioned. Indeed, it may be considered as a computing device of the pre-electronic age" (in Mirowski 2001: 236). In this regard, Mirowski sees in the debate a crypto discussion about economics that merges with technology – cyborg economics. Thus, was Mises' argument out of the game?

This brings us to the strike of Hayek that would separate the spirits of economic science for decades to come. Hayek was the last to challenge the association of positive economics and socialism. Between 1942 and 1944 he published three essays on "Scientism and the Study of Society" – reprinted in 1952 as *The Counter-Revolution of Science: Studies on the Abuse of Reason*. When Hayek characterized scientism as the "mechanical and uncritical application of habits of thought to fields different from those which they have been formed" (1955: 16), it clearly echoes Mises' point that rationalism in "the economy" is restricted to a liberal society. And when he compared economists' scientism with Stalin's "engineers of the soul", he must have also had Lange, Dickinson, and others in mind (Ibid.: 94).

Hayek revived Mises' aprioristic argument also on the level of a different conception of the market, which some celebrate as his most genuine contribution. In his essay "The Use of Knowledge in Society" (1945), Hayek presented an *informational* interpretation of the market that materializes the apriorism of Mises in an actual alternative "paradigm" beyond the Walrasian equilibrium model. The market is not, as it was conceived between 1930 and 1970, the allocation of resources (what the Central Planning Board could simulate), but it utilizes "knowledge" most efficiently. And this cannot be a matter of technology because this knowledge, so the Austrian tenet, is *subjective*, and thus evasive of empirical determination. The knowledge utilized in markets is situational, contextual, and adaptive to new situations. With this argument of the informational efficiency of the market Hayek fleshed out Mises' apriorism with the idea of dispersed and subjective knowledge. For the challenge of Mises was not that it is not possible to solve the equations, but to formulate them in the first place, as Lavoie has re-interpreted the debate (in Boettke 2000, VI: 183). While the problem of resource-efficiency could still be conceived of as "objective", the privatization of knowledge cut off modern scientific optimism at its source. At this juncture, Michael Polanyi, who coined the term 'tacit (read: not scientific) knowledge', joined the Mont Pelerin gang, while his brother Karl Polanyi had to commute between Canada and New York because of McCarthy (Mirowski 2004: 71 ff).

Hayek's argument implied a second fundamental critique of Lange's model: that it was static – which reversed the association of socialism and history that was vital at the beginning of this century of high modernism. The actual virtue of the market, according to Hayek, is not that it brings about an efficient result, but that it brings about results efficiently – a dynamic process. "Competition, to Hayek, is a verb, a noun to Lange," Boettke comments (in Boettke 2000, I: 17). Although for Hayek competition does describe a specific behavior, his new conception moved economics away from the question of value that haunted modern economics in the century considered in this chapter. With this novel conception of the market Hayek set the scene for most Austrian economics after 1945. But he also set the scene for the paradigm shift from resource allocation to information processing that happened *after* the formalist revolution *in* science.

In the rough-and-tumble of the war, Hayek's challenge found no soil. It did not inhibit scientific optimism. What happened was rather that scientific optimism was disassociated from political systems. WWII was the beginning of Big Science. The place where the ethos of social engineers that Lange represented and Hayek challenged could prosper beyond its ideological connotations was the Cowles Commission in Chicago. Lange was involved in this group together with his student Don Patinkin, and others such as Leonid Hurwicz, Trygve Haavelmo, Tjalling Koopmans, and Lawrence Klein. As their names suggest, many of them were war-émigrés from the continent. Officially, this group took over the empiricist spirit of their founder Alfred Cowles, and his support for econometrics – the econometrics society was launched in 1930. The socialist calculation debate was certainly in the back of every Cowlesmen's head, though the militaries' money in their pocket. The innovation of linear programming, for example, would have been inconceivable without the background of Lange's model, but was then advanced in the context of the design of weaponry, above all by George Dantzig. This disassociation of mathematical tools and politics was certainly fostered by the new vision of the computer. Planning became programming, and calculation became computation, which both easily passed political muster. At the occasion of refereeing *The Road to Serfdom*, Marschak distinguished between good and bad planning – he doing good planning, and Hayek arguing correctly against bad planning (see Mirowski 2001: 245). "This interesting turn in the discussion", Koopmans commented this depolitization, "shows, it seems to me, that the earlier discussions [Hayek, Lange] had been concerned too much with absolute institutional categories encompassing the entire economy" (1951: 457).

Cowles research program was, moreover, watered down by the "neo-classical" adaptation of Keynes who was more presentable in the western milieu than economists who, like Lange, travelled up and down between Stalin and Roosevelt. Socialism was not written on the flags of Cowles but their motto was *Science and Measurement* – which, for Hayek, would amount to precisely the same. One could also view this group independently of the debate, for example along the rise of national income accounts conventions. The separation of economic science and politics culminates here in the spirit of technocratic engineering that appears beyond any philosophical quarrels that haunted the entire century of high modernism. With Cowles, the contestability of the ethos of economists disappears behind technical sophistication. Debreu later would survey the history of Cowles between 1930 and 1980 ignoring its political involvement altogether (1983c).

The biography of Jacob Marschak, director of research of Cowles between 1942 and 1948 tells the peculiar passage from economics during the war to economics after the war (Cherrier 2008). Born in the Ukraine, he actually made the transition from post-revolutionary planning, to Berlin, where he contributed to the debate with a pro-socialist argument (1924), and further on – with stops at Kiel, Oxford, and New York – to Chicago in order to support the statistical branch of Cowles since 1942. After the war he was one of the first who contributed to the application of the same scientific rigor to the new rise of the information paradigm that Hayek introduced as an alternative to scientism. Hayek's anti-scientism was forgotten when economists like Hurwicz launched the research program of "mechanism design" which brought back the question of informational decentralization posed by Hayek, but in a highly scientistic fashion. After these developments, Hayek and von Neumann understood each other immediately when meeting at a party (in Mirowski 2001: 238). This disassociation of scientific authority and socialist ideologies represent the miracle of years surrounding 1945. The formalist revolution was operational for it. It made possible to reverse the motives that led to it, which will occupy great parts of the following part.

In the immediate post-war years until the early 1950s Cowles still shimmered reddish, although economists like Oskar Lange already left the country by 1945. The tensions between Cowles and Friedman, of course, root there (Mirowski 2001: 241 ff.). Koopmans described the spirit of social engineering in Cowles with the following words. Cowlesmen shared

> a strong sense of mission and of standing together in the early postwar years of the Commission (…) With Klein, Hurwicz and others we battled for simultaneous equations and for the idea of econometrically guided policy, in the annual meetings of the professional societies and our skirmishness with the National Bureau, as if the future of the country depends on it! (in Mirowski 2001: 244).

The young Lawrence Klein shared the feeling. Against Samuelson's advice he moved as a post-doc from MIT to Cowles, where he stayed from 1944 to 1947.

> A truly exceptional group of people was assembled in Chicago during the late 1940s. I doubt that such a group could ever be put together again in economics (…) In the field of postwar planning we imagined that we had the well-being of the economy right in the palms of our hand (in Breit, Spencer 1995: 23 f.).

This strong sense of mission, however, had to fade away in the following years. When in 1948 Koopmans took over the place of Marschak as Research Director of Cowles, he made a drastic turn from econometrics to theory - a point that I will pick up again in the following part. Since then there were no longer any real encounters of market socialism and liberal critiques. Cowlesmen were too busy with the new technologies. Willy-nilly, they lost their political expressiveness. Thus, nobody in the 1950s obeyed Hayek's call against scientism. Science proliferated as never before – independent but still in the context of western capitalism. Hayek may have won the debate in ideological terms, but he lost in terms of science. The open violence that has been done in favor of freedom in the two, or three decades after the war did not obey the spirit of Hayek's *Road to Serfdom* (1944) – at least if we think of the controlling apparatus that McCarthy and Macy envisioned in order to back up market freedom.

A last publication of the debate needs to be mentioned: Abba Lerner's *Economics of Control* (1944). Cowlesmen were reading it eagerly. Though it was a technical book at all, it incorporated the entire Walrasian framework. Every student of economics today is well equipped to read this book. Lerner opens the book by saying that "liberalism and socialism can be reconciled in welfare economics" (4). With Lerner, the socialist enthusiasm for scientific planning boils down to this sub-discipline of economics. A controlled economy is opposed to laissez-faire politics without implying collectivism. Lerner, as Lange, thus still associates the social miseries of the culture of capitalism with the "uncontrolled economy", which shows he is clearly a child of the century that I have considered in this chapter. But for Lerner control is no longer a matter of substituting the market, but quelling the market. In Lerner "market socialism" inconspicuously passed over into, yes, the "social market" – that became *the* motto of (ordo)liberalism in Europe after the war. While once the question was whether socialist politics could be as efficient as the market, now the question was how "social politics" could assure the efficiency of the market. While once socialist politics was meant to replace the culture of capitalism, now "social politics" means to correct market failures. Market efficiency, until today, is something to *become*.

In this way, Lerner bequeathed to the discipline of economics the theoretical remnants of the socialist calculation debate: the theorems of welfare economics, as students of economics learn them today. The textbook versions of these theorems say that a competitive equilibrium is Pareto efficient, and that so-called lump-sum transfers can realize any Pareto efficient allocation. How far are these theorems removed from the days when Bukharin was citing Mises, or Lange advised Stalin! Yet they spoke about the same theory.

In 1983, Debreu would resume Oskar Lange's contribution to mathematical economics at Cowles without even alluding to its political meaning:

> During the first years of the Cowles Commission in Chicago [since 1939], Lange was also working on 'The Foundations of Welfare Economics,' (…) Lange studied the characterization of the Pareto optima of an economic system by means of differential calculus, a problem that Maurice Allais was independently considering at about the same time in France (…) Lange's and Allais's contributions brought a long phase in the development of the two basic theorems of welfare economics close to its conclusions. They were to influence the reexamination of those theorems by means of convex analysis in the early fifties (Debreu 1983c).

This "long phase" I have just surveyed. What Debreu must have meant with "conclusions" was that GET ceased being associated with politics, and began being associated with mathematical sophistication. The socialist calculation debate, as Debreu implicitly acknowledged, remained unresolved. Instead, it experienced a rupture – the rupture of 1945.

The socialist calculation debate has no clear winner. Maybe the retreat was on the socialist's side since it never tackled the aprioristic argument at its roots, as Lavoie argued (in Boettke 2000, VI). But the retreat also may have been on the liberal side, since it could not maintain the scientific optimism that stamped the spirit after 1945. Perhaps misunderstandings prevailed, as O'Neill suggested after retrieving Neurath. He concluded that it was "Lange who is closest to Mises, and Hayek who shared most with Neurath" (1996: 439). Mirowski, instead, interprets Arrow's Impossibility Theorem of the early 1950s as the actual response to Hayek (2001: 302 ff.). I may conclude as follows: Considering the further passage of scientific optimism, socialists won; but considering the ideological setup, Mises won. The socialist side did push the spirit of social engineering and the idea that rationality can be learned, if not by agents, then at least by the central authority. But these authorities were all but socialist; they were rather western governments securing freedom in markets.

More important than the resolution of the issue was thus the *separation* that took place during the debate – the separation that describes the entire century of high modernism in economics. Designed to give a particular theory a reddish color, it ended up corroborating that the color does not matter for economic theory. Designed in order to lay open the hidden hands that underlie the invisible hand, it ended up hiding them all the more. What remained after the debate was an unresolved contest about the meaning of economic theory, as well as a half-opened box of mathematical sophistication for the sake of econometric guesswork. Note that the political associations of theory are contingent to the extent that they require scholarship, including the possibility of being expressive of one's intellectual culture. But this was no longer possible after 1945.

None of the contributions made to the calculation debate contributed directly to the scientification of economics. It was not by virtue of a *solution* to the battle of ideologies that scientification advanced between 1850 and 1950. Instead, it happened by means of going beyond it. In the same sense as early modern economists accomplished to arrive at a level beyond the imposition of mercenary motives, modern economists accomplished to arrive at a level beyond the issue of socialism vs. capitalism. The theoretical perception of "the economy" (manifest in GET) gained most scientific authority when it became independent of all its possible interpretations.

I rather do not attempt to summarize this century of high modernism in economics. I obviously could only present some basic remarks that a thorough social history of scientification has to follow up. If one believes in an expressive life of economists, one surely finds some equivalent, precursor or some association with this period between 1848 and 1945 – but not later. For 1945 was the end of the social history of the ethos of economists, as I attempt to show in the following chapter.

(4) Today, since 1945 – Late Modernism

The year 1945 represented a radical reconfiguration of the modern triad of science, technology, and economic growth – in particular insofar as it was associated with the liberation of man: the absence of authority, the control of means, and the liberation from needs. 1945 also represented the rise of two intellectual cultures on the continent and in the Anglo-Saxon world that reacted rather differently to that reconfiguration. The difference was that on the continent, at least for its intellectuals moving from modernism to post-modernism, 1945 meant the *end* of the belief in such triad, while in the Anglo-Saxon world moving from liberalism to neoliberalism it meant a *success* of this triad. The continent was committed to a new anthropodicy, which questioned how one possibly could still think humankind in light of the new imperative of "Never Forget!" The Anglo-Saxon world, instead, experienced a great push forward, a renewed belief in the promises of a *scientifically engineered liberty*. Most of the events since 1945, including 1989 and 2001, seem to confirm this authoritative claim to the meaning of liberty. The great puzzle from the point of view of the continent, however, is how the scientific optimism that stem from WWII could continue through the Cold War, and did not find a clear-cut end at 6th or at the latest at the 9th of August 1945.

The Secrecy of the Engineering of Liberty and the Formalist Revolution

The years after 1945 in the U.S. were the years of Big Science. With united forces, government and scientists from different promising disciplines – psychiatrists, mathematicians, physicians, information scientists, biologists, and neuroscientists, etc. – worked together at one goal, namely to design the new man. The military was one of the most generous departments in providing funds. Big Science was militarized science – the origin of corporate sponsorship of science, according to Mirowski (2001). Apart from the hope for new weaponry, the dream of a new man was to free mankind from its inclination to totalitarianism. Some scientists expected to find this inclination as a piece in the brain, others hidden in the causal feedback-loops of the social system. But most envisioned new man as the "age of communication and control" – Norbert Wiener's motto of cybernetics. The name John von Neumann needs to be dropped, too. Google "Macy Conferences", or "Ratio Club" to get a feeling for the scientific optimism in the postwar years. By Means of Science: Liberty! – so the new clarion call, which some economists sung along.

For a German European, like me, what is most astonishing about this clarion call is the continuity of social engineering during and after WWII. Inconspicuously, social engineering switched its connotations from socialist politics to western liberty. Between the 1930s and 1970s there was a continuous proliferation of technological innovations in economics. After the cultivation of real analysis waves of hope have been caused by linear programming, measure theory, simplex algorithms, nonstandard analysis, operations research, game theory, mechanism design, and simulation techniques – methods that are associated with names such as Tjalling Koopmans, Leonid Kantorovich, Jacob Marschak, John von Neumann, George Dantzig, Herbert Scarf, Leonid Hurwicz, Herbert Simon, and Kenneth Arrow. Their research stamped the years during the war in the same fashion as it stamped the years after the war. The same technocratic spirit continues today above all in complexity science associated with names such as Steven Durlauf, Lawrence Blume, Brian Arthur, Doyne Farmer. At the end of the cold war, economists only had to move a couple of miles from Los Alamos to Santa Fe in order to give full expression of their technological enthusiasm in a neoliberal world.

The changing connotation of technologies in economics is one of the most intricate episodes of the history of economics science. The technological innovations are in a seemingly contingent relation with the events that determined their social meaning. The political and the theoretical output of technology are difficult to link. To spell out these links is to write archive history, while ignoring the history that came to be represented in textbooks. One needs to dig out the archives of RAND, look for some hints in Göttingen, go down into the cellars of the *Institute for Advanced Studies*, trace how people moved from Los Alamos to Santa Fe, or ask the CIA Officer who may have heard what von Neumann bubbled when guarding his door before the genius passed away.

The general college teacher remained ignorant about these flights of science. Until the 1970s, the bulk of economists were busy with soft Keynesian engineering, at most with input-output analysis. There was only a small elite of scientists who were directly engaged in the mission of utilizing science for the engineering of freedom. The bulk of the profession was excluded from such business. *Big Science* was to a great extent *secret science*. Hence this history, exciting as it is, is only of limited relevance for the present social history of scientification. Secret history represents the underworld of the social history of scientification. Not more.

The link between technology and politics in economics is so intricate because it is difficult to squeeze out an intentional link between abstract theoretical innovations and concrete political design. How could the military possibly believe, for example, that the mathematics of "saddle point existence theorems" could help in case of an atomic strike – a belief it seemed to show when it funded Debreu's research for RAND (1952)? For the present plot the surprising fact is that such technocratic vision of science was no longer associated with the socialist pretension for planning as before in the 1930s. The social history of scientific engineering from the 1930s to the 1970s is interesting in that it describes a full turn of the political orientation, for which technology was used and designed for. While in the 1930s social engineering was associated with socialist planning, in the 1950s and 1960s it was associated with the engineering of market liberty.

Planning in the "age of communication and control" lost all totalitarian connotations. In some cases, this loss was not even accompanied by a change of the technical tools. The passage from calculation to computation partially meant not more than a change of names: linear programming, for example, was discussed by Jacob Marschak in the context of getting Oskar Lange's model of a socialist economy going. Later Koopmans mobilized the same technique under the name of "activity analysis" in order to model an efficient market economy of the U.S. Both referred to the same mathematics. But the former was politically suspicious, and the latter easily passed political muster (see Mirowski 2001: 258 f.).

This reversal was also not the result of an open renegotiation of the meaning of technology. In an intellectual milieu that was coined by such phenomena as the Smith Act – forbidding revolutions by law – and of course McCarthyism – forbidding even thinking about revolutions – how could debates on the political meaning of scientificity possibly be carried on? How could one possibly carry on *any* of the ideological battles that had moved economists' minds the century before? As Herbert Simon commented on these years:

> By 1948, Communists and supposed Communists were being discovered under every rug (…) Any graduate of the University of Chicago, with its reputation for tolerance for campus radicals, was guaranteed a full field investigation before he could obtain a security clearance (in Mirowski 2001: 246).

Thus, there was no eminent economist in the ideologically tense years after WWII who did not write science on his forehead giving the impression that all battles of ideology had been already decided – at least in the epistemic departments of the Truman-Eisenhower-Kennedys. No discussion. Those who still contributed to the political battle about science, such as Hayek, were expressively critical about the idea of engineering liberty (Hayek 1949). They pointed to the apparent contradiction of the new scientific clarion call and argued openly for the political irrelevance of economics *for the sake of freedom*.

The decisive question for the present social history of the economists' ethos is thus: Engineering liberty? Why did this not evoke open quarrels? Why did it not undermine economists' scientific stance? Were both of Foucault's two hands invisible? Mirowski suggested that military secrecy made it possible.

> The older machine dreams of a socialist like Oskar Lange were simply nowhere on the radar screen in the immediate postwar America, fearful as it was of a Red under every bed. However, there was one place where comprehensive planning was not only being put into practice on a daily basis in postwar America but, even better, where dark suspicions about patriotism and aspersions about foreign-sounding surnames were kept at bay, if not altogether banished. Whatever the wider doubts about the intellectual coherence of 'market socialism', the American military was one place where unquestioned adherence to the virtues of the market cohabited cheerfully with the most vaunting ambitions of centralized command and control, without ever provoking any hand-wringing about conceptual consistency or soul-searching over freedom (Mirowski 2001: 255 f.).

Military secrecy may have indeed institutionally enabled technological innovations in economics. But it does not explain what these innovations did to the profession, which is what this chapter will track down to today.

The answer that I suggest is that the formal character of technological means made it possible to disassociate technology from political determination (see e.g. Goodwin 1998). Only in a demanding, if not absorbing and at the same time unintelligible science political worries do not overload economists' practices. One could practice economic science as though it was not a political affair. In other words, *only* a seemingly ideologically free science could be utilized in a conflict so ideologically laden as the decades of the Cold War. In an atmosphere as tense as in the 1950s, economic science needed to appear free from ideology, but nevertheless, if it comes to its "application" had to play out in support of the western front. This is the key to understand the efforts put into the scientification of U.S. economics in the decades after WW II. Precisely this need for authority beyond politics was met by the formalist efforts of the *neo*-Walrasian community. The meaning of the *neo* in neo-Walrasian is thus clear: Walrasian economics beyond the discussion of its political meaning –that is, Walrasian economics axiomatized. I will discuss this episode in the following part extensively.

In other words, the formalist revolution carried out by the neo-Walrasians in the 1950s and 1960s had an enabling, if not conditional, function for the scientific engineering of liberty. Formalist economics kept the tension inherent in this mission low. It kept the two Foucaultian hands that give and take freedom from touching each other: the axiomatic separation of meaning and structure did precisely that! Mirowski and Sent acknowledged this auxiliary role of the split between "pure" and "applied" when claiming the following:

> The idea that there was some necessary but unproductive form of scientific research that required state funding for its very existence, and that the economic growth of the nation would suffer in its absence, whereas applied R&D could be safely left to the corporate sector to organize (…) provided the ideal cover for the absence of accountability of military science planning (2002: 22)

As a consequence of this separation, however, we can observe a rising gap between the perceptions of economics within the discipline as opposed to the perception of economics in public, on which I have concluded the last part. The public came to perceive economists as market engineers of efficiency. But for the economists themselves, who were by and large excluded from the secrecy in some elite institutes, the two decades after the war were like a traumatic sleep of suppressed discussions. Because those who occupied the core of the profession could barely afford having contests over the meaning of economics.

Certainly, there were some disagreements between Friedmanians and Samuelsonians. But they increasingly agreed on terms, and taught the same textbooks, and enjoyably separated themselves from the rest of economic talk. "Where Milton Friedman and I disagree," Samuelson defends the rank of economics as a science, "we are quick to be able to identify the source and texture of our disagreements in a way that non-economists cannot perceive" (1992: 237). Friedman shared Samuelson's notion of political incontestability of economics, as his following comment on his Chile-project makes clear:

> In spite of my profound disagreement with the authoritarian political system of Chile, I do not consider it as evil for an economist to render technical economic advice to the Chilean Government, and more than I would regard it as evil for a physician to give technical advice to the Chilean Government to help end a medical plague (in Letelier 1976: 45).

Could Samuelson not have said the same? Surly, Friedman and Samuelson had contests about truth, but no contests about the meaning of truth in economics. The same is true regarding the philosophical standards of economics. There never was an open renegotiation of such standards. During the 1950s and 1960s it was not necessary to sneer on the old literary style of economics. Only later in his age, Samuelson did so:

> Like Tobacco Road, the old economics was strewn with rusty monstrosities of logic inherited from the past, its soil generated few stalks of vigorous new science, and the correspondence between the terrain of the real world and the maps of economic textbooks and treatises was neither smooth nor even one-to-one (in Breit and Spencer 1995: 59).

Such explicit statements were rare after the war. For those who pushed the new economics, such judgment was implicit in their commitment to rigor.

Those economists who still held strong positions about the meaning of their discipline, instead, left the center of the stage. Austrian economists did not claim a place in Chicago or MIT, but willingly moved to Mises' seminar in New York. And the Paul Sweezys left the economics departments, too, but unwillingly. There remained but one huge group of economists who continued to claim representation in economics: the Keynesian generation. By Keynesian generation I mean those economists, who were born between 1900 and 1930, were taught in Marshallian economics, socialized via the Great depression, gained literacy with Keynes' General Theory, went along the neo-Keynesian adaptations to some extent, but then understood when they had to teach Samuelson's *Economics* that they are out of the game. But then it was already too late. At the end of the 1960s it came to a *generational clash* of these with the new economists who represented the new core.

Since these new economists largely came from other disciplines (the Tinbergen-Koopmann-Neumanns), the literacy of the Keynesian generation did not help much in following what was going on at the top. The skills needed for being an economist changed drastically in the 1950s and 1960s. Both the so-called Keynesian macro-econometrics of economists like Lawrence Klein, and the neo-Walrasian hunt for rigorous micro-foundations (both represented by the Cowles Commission) crowded out the economic literacy of an entire generation. The expressive opportunities for the old generation of economists diminished drastically. Many who perceived themselves as economists, suddenly felt inadequate within the new institutions of economics. All the more since these institutions flourished as never before. In the postwar years the "oligarchic structure of the profession" in the U.S. triad of MIT-Chicago-Princeton, as Eagly has surveyed the discipline in 1974, came into being. Around 1970, economics gained the institutional profile which I have described in the first part. *The* manifestation of the success of scientification was that in 1969 economics received a mimicry for the "Nobel Prize" – the *Bank of Sweden Prize in Memory of Alfred Nobel*.

The rising gap between the profession and the core, the decreasing expressive opportunities, and the steady increase of the burden of responsibility to live up to the seizure of the institutions of economics caused evermore fret and uneasiness among economists. While I will deal with the two decades after the war extensively in the following chapter, in this chapter I take the uneasiness of the Keynesian generation as a starting point in order to argue that all responses to the post war situation did not affect the social ethos of economists. In this sense, the formalist revolution represents a preliminary end of my social history of scientification. The present situation of economists, I argue, can be described in terms of a historical eclipse that does not exceed the horizon of the formalist revolution. No theoretical innovation since the 1970s affected the ethos of economists. I already described the institutional unity of economics since the 1970s in the last part in light of economists' discursive situation. In this chapter I consider the same unity from the point of view of economists' *self-understanding*.

The Keynesian Uprising of the 1970s and its Phenomenological Confusions

At the beginning of the 1970s, economists shared a feeling of uneasiness that something went wrong. All what has been gained in the decades after the war perhaps came at too high costs: the relevance of economics. Whatever the secret allies of economists and the cold war, the shared sensation was that of detachment, a detachment from what economics was once supposed to be. For an entire generation it became increasingly difficult to conduct a scholarly life in the booming institutions of economics. The Keynesian generation felt particularly uneasy with the prevailing idea that came to dominate the scene in the 1960s: that economics has been always centered around one, and only one, question: the conditions under which a general equilibrium holds. After the formalist revolution settled down, leaving behind this monist theoretical core, the sensation was that of a "turn inward", as Heilbroner and Milberg put it (1995: 68). The desire for change grew evermore (for an extensive survey of the field in the 1960s, see Ruggles 1970).

As early as 1973 Frank Hahn, one of the represents of the new core, acknowledged the general discontent, but at the same time added his discontent about these discontents.

> Economists do not grow bitter gracefully. Many of them came to the subject hoping to do good and to be useful and find that they can do far less than they had expected. Many others with a theoretical bent find that they cannot now understand what the best minds in their subject are saying. (…) Looking at, but not often studying, the pages of some learned journals or Debreu's work, they all agree: 'This is not what I meant, this is not what I meant at all'. If they are of the right age they then write a presidential address or a lament. What I think is disturbing about so much of this literature is that it is so bad (322).

Hahn refers to the prominence of the Keynesian generation that rose up against the trends since 1945 in a row of Presidential Addresses of the AEA during the 1970s. Let me reread these speeches asking why they may have been, if not "bad", so perhaps shortsighted.

A Presidential Address is a unique occasion for an economist to express his or her worries about the state of the discipline. The speeches present to us a frank image of the self-understanding of an economist in the attempt to speak for the entire profession. The points the speeches in the 1970s raised roughly resemble the complaints that the French students would later put forth in their post-autistic economics petition of 2000. With these speeches, I thus return to the standard critique about economics with which I closed the last part. It was in these speeches that this standard critique was instituted. As a reversal of the economic suspicion, now the choir goes: Economics is Irrelevant!

Kenneth E. Boulding (1910-1993) made the beginning in 1968 with his speech *Economics as a Moral Science*. Boulding was a broad-minded economist, who in his early years first contributed to the neoclassical-Keynesian synthesis. Later he wanted to be known as an evolutionary economist with high-brown scientific ambitions, but without loosing the modernist faith in the moral flourishing of economic life. In his speech he pointed to just this lack of normativity, particularly in welfare economics that is mislead by the spirit of Pareto efficiency. He stood up against the belief that everything that goes beyond the consideration of equilibrium prizes is like 'sermonizing' on the 'just prize' – assessing correctly the historical scope of the changes that took place in the preceding decades. "Who, indeed," he said, "would want to exchange the delicate rationality of the theory of equilibrium price, for the unoperational vaporings of a 'just price' controversy" (1). Apparently without knowing what was going on in the epistemic head quarters of the Air Force, Boulding indeed took it for granted that economics is beyond the politics of its time: "Economics is a reconciler, it brings together the ideologies of East and West, (…) it is corrosive of ideologies and disputes that are not worth their costs" (11). And so it is no surprise that Boulding concluded that economists should face up to the economic suspicion that has been oppressed for at least three centuries.

> There is a widespread feeling that trade is somehow dirty, and that merchants are somewhat undesirable characters, and that especially the labor market is utterly despicable as constituting the application of the principle of prostitution to virtually all areas of human life. This sentiment is not something which economists can neglect (10).

Boulding presented his plea with reference to the values that are constitutive for the academic world of economists (2).

Wassily Leontief (1906-1999) continued in 1971 with his speech *Theoretical Assumptions and Nonobserved Facts*. Leontief was the inventor of input-output analysis, an inclusive framework to statistically determine an aggregate economy. It developed somewhat aside from the socialist history of GET since Leontief never shared the theoretical interest of Walrasians. His interest stem from the issue of statistically determining demand and supply curves, an issue that occupied the early econometricians, and later went to Koopmans' dogs of theory. Hence, Leontief complained about the imbalance between statistical techniques and the undeserved recognition of mathematical economics. He addressed the misleading nature of mathematical economics that is too easily equated with scientificity, though actually being void of any reference. "Uncritical enthusiasm for mathematical formulation tends often to conceal the ephemeral substantive content of the argument behind the formidable front of algebraic signs" (2). Resembling Husserl's "garb of ideas", he consequently stated a tendency to forget empirical reference when practicing mathematics: "By the time it comes to interpretation of the substantive conclusions, the assumptions on which the model has been based are easily forgotten" (2). Leontief nonetheless believed that a balance of theory and statistics represents science proper. But he did not react to the socialist connotations that phrases like the empirical 'mapping of the economy in all its many dimensions' (7) had before the war.

John Kenneth Galbraith (1908-2006), an economist again of another kind, added his share to the sensation of crisis in 1973 with his speech *Power and the Useful Economist*. Galbraith was most known for one of the most popular books in postwar social criticism, *The Affluent Society* (1958). In his speech, he pointed to the exclusion of the industrial and political power in neoclassical market theory (or, "neo-Keynesian", as he called it). "The business firm is subordinate to the instruction of the market and, thereby, to the individual or household. The state is subordinate to the instruction of the citizen" (2). Thus, nobody actually exerts power – a politically incorrect assumption. Galbraith thus identified the "hidden" second hand that takes freedom in order to support the first "invisible" hand that provides freedom. Examples concerning the (non)developing countries are ready at hand in his speech.

In this way, Galbraith acknowledged the twist that happened to the invisible hand between Smith and Arrow. While in Smith it correlated with a particular, namely rival behavior (of which the pursuit of gaining market power is part of), the concept of competition later in GET meant the absence of market power. And so Galbraith says, "eliding power – in making economics a nonpolitical subject – neoclassical theory, by the same process, destroys its relation with the real world" (2). Note, however, that he is far from appealing to scientific realism. As though he was in Foucault's lecture, he writes: "Economics, so long as it is thus taught, becomes (…) a part of an arrangement by which the citizen or student is kept from seeing how he is, or will be, governed" (6). Reality, for Galbraith, is a matter of politics, not of truth-seeking. And with the same move he brings us back to the economist as suspect – as in the times of sermonizing about just prices: "If the state is the executive committee of the great corporation and the planning system, it is partly because neoclassical economics is its instrument for neutralizing suspicion that this is so" (11). Galbraith, too, not only made a theoretical case, but also demanded a new disciplinary policy based on more pluralism in economic science. He anticipated the French movement when saying: "Perhaps there are limits to what the young will accept" (11).

Robert A. Gordon (1908-1978), in 1976 (*Rigor and Relevance in a Changing Institutional Setting*), spoke out what many had already on the tip of their tongue: a direct trade-off between the commitment to science and the possibility of saying something of worth. Gordon was a Berkeley economist with a strong Keynesian sense for employment issues. He even cared for the quality of business education. His argument must have been a relief for many of his audience: "The mainstream of economic theory sacrifices far too much relevance in its insistent pursuit of ever increasing rigor" (10). Gordon too was explicit about the 'garb of ideas': "seduced by the siren of mathematical elegance" (12), the profession is unable to tackle the "really big questions about the economic aspects of society" (10). Such happens, he argued, when unemployment is assumed to be voluntary. Then 'relevance is considered irrelevant' (5). The train went off track since Robbins, according to Gordon, and runs clearly in the sand since GET. Gordon did not consider the conditions of committing oneself to the intellectual virtue of relevance; neither did he ask how it was *possible* that rigor came to substitute the concern for relevance. But he certainly has his point when quoting Stigler for equating relevance and 'relevance to economic theory' (2). In Gordon's view, sensibility for historical institutions would bring economics back on track, asking the really relevant question: "What is the future of capitalism"? (12) Will it break down? Or will it lead us to heaven? But these questions are too hot for the shrine of science. No discussion.

I could extend the list of countless other speeches held in other societies that adopted a similar tone. Let me only mention some highlights. As early as 1964 James Buchanan, in the spirit of Hayekian scholarship, mourned the "lack of identification" of economists. However, the cause of this disease he discussed in terms of the scope of economic theory. In 1972 Ward wrote a first full monograph on *What's Wrong with Economics*, applying Kuhnean standards to economics, but showing that it fails to respond to anomalies. Only some, such as Heller (1975), attempted to balance out the pessimism, and moderately asked *What is Right with Economics*? Another direct critique of GET worth mentioning, since it became like the refrain of most complaints in the years to come, is Nicholas Kaldor's *Irrelevance of Equilibrium Economics* (1972). Pointing to the difference of the axiomatic and the scientific method, the exclusion of increasing returns (thus monopolies), cumulative change, among other points, he called for a "major act of demolition" of GET (1240). Another "hit" the reader may remember was Leijvonhofvued's "Life Among the Econ" (1973) that ridiculed the closure of economists' tribe. Since the 1970s, there is a clear genre of anti-economic monographs continuing to lament *The Irrelevance of Conventional Economics* (Balogh 1982), *Debunking Economics* (Keen 2002) basically with the same arguments as mentioned, and ceaseless questioning: *What's Wrong with Formalization in Economics* (Woo 1986). As in the mid-19th century, there was a clear perception of a crisis of economics. But this time it came from within the profession.

In this choir of laments, the profession agreed with those who came to be excluded from economic science for political reasons, that is, those who suffered greatly under McCarthyism, such as Paul Sweezy (1910-2004). Yet when the profession pled for more pluralism, economists like Sweezy were hardly included. Economic science, Sweezy argued in 1972,

> has concerned itself with smaller and decreasingly significant questions, even judging magnitude and significance by its own standards. To compensate for this trivialization of content, it has paid increasing attention to elaborating and refining its techniques. The consequence is that today we often find a truly stupefying gap between the questions posed and the techniques employed to answer them (1972: 63)

Sweezy continues to illustrate this lost sense of intellectual propriety quoting Gerard Debreu. Together with his neo-Walrasian followers, Debreu indeed came to be known as the personification and bogeyman of mathematical economics, and thus of the irrelevance of economics.

At first glance, these speeches had full success. There is hardly any economist left who would not accord with the plea that mathematical economics is not All There Is. Neo-Walrasian economics is the economics of yesterday. Before I turn to assess that, let me first identify a crucial phenomenological confusion in these speeches concerning the *locus of critique*. Was the object of attack really a particular theory and particular methodological convictions? In all speeches the argument of economics being irrelevant was indeed meant to redirect theory and method. Then, as soon as theory and method allow for more value judgments, more empirical work, more consideration of power and institutions, economics will be back on track. This optimism that a simple change of attention could cure economic science, however, came with a sub-tone of pessimism when referring to the institutional context, and intellectual values that should enable this change of reflection.

Leontief, for example, was aware that not only the recognition of particular aspects of reality would be enough for a cure of the disease. He considered the fact that economists *already* knew that their theories are easily vilified. Economists, so he acknowledged "play the game with professional skills but have serious doubts about its rules" (1972: 1). Also others, as mentioned above, included as a locus of critique not merely the *scope and method* of theory, but also the social conditioning of science in that it may undermine insights into theoretical flaws. The actual problem, then, is not the wrong methodological value that informs the prevailing economic theory, but the *absence* of intellectual values that could inform these theories. Then, the insight in theoretical shortcomings has no effect. Only in this sense can one speak of a crisis of economics in the 1970s: the profession did recognize the shortcomings of its core theories, but also secretly had to acknowledge that there is hardly something to do about it. The emotions of the Keynesian uprising did not root in a contest about reality or the role of theory. Instead, they came from the lack of ethos of economists.

In Galbraith's speech, this transcendental sensibility is most apparent. Economics "offers no useful handle of grasping the economic problems that now beset the modern society. And these problems are obtrusive – they will not lie down and die as a favor to our profession" (1973: 2). Not reality *as it is*, but reality as being *obtrusive* is at stake in economics; not the wrong beliefs about reality, but the *sense* of reality. Galbraith thus had a clear perception – though no conception – of the transcendental character of the issue at hand. He did not want economics to be more realistic, but wanted "to urge the means by which we can re-associate ourselves with reality" (Ibid.: 2). And these means that could make economists sensible for the attraction of truth are not a matter of wrong beliefs, but a matter of the passions that inform intellectual activity: "[S]ince we will be in touch with real issues, and since issues that are real inspire passion, our life will, again, be pleasantly contentious, perhaps even usefully dangerous." (10) Economists lack inspiration, passion, the joy of contests, and the willingness to risk popularity. Galbraith shows most clearly that the crisis in the 1970s was not primarily a matter of which theory economists pursue, but a matter of the affective set-up of the economists' ethos as a condition for committing oneself to any theoretical virtue. Only this locus of critique granted the Keynesian generation its forces to rise up against the neo-Walrasian domination.

The arguments, in the mentioned speeches, pivot in-between "reality", "what matters", "obtrusiveness", the "inspiration" and "passion" of economists, and the wish to "be in touch with". It is this conflation for which a phenomenological critique of economic science is needed. Within a scientific attitude – and as such the addressees speak up in front of the *AEA* – this conflation cannot be resolved. For the economist as a scientist such pivoting is symptomatic. A scientist in its pursuit of being scientific can do nothing other than expressing his sensibility for relevance in terms of a feature of a theory. The prime place where self-critique takes place for a scientist, is the theory, not the theorist. The argument of irrelevance translates then straight into the terms of received philosophy of science: 'economic theory does not refer to the reality of the economic world'. In scientific attitude, "reality" and "what matters" are one and the same.

Although theory and method changed considerably since the 1970s, as I will argue in the following, the speeches remained ineffective insofar as they were formulated as a plea for a new particular and definite "unit of analysis", or "theoretical paradigm". If there is a phenomenology lesson 1.01 for economists, it is that relevance is not a feature of a theory in that it refers to the world. It is anything but a property of things lying around in the world that could be stated as any other property like "decreasing" or "increasing". All speeches touched upon, but hardly advanced to the point where the actual motivations of doing science could be explicitly reflected upon. In this sense the speeches as all standard critiques of post-war economic science, were phenomenologically naïve: conflating one's appeal to reality with the urge of doing relevant research.

Given this naivety, I may even call these speeches reactionary. For did they not implicitly confirm that economics possibly deserves the size of the institutions it gained through the formalist revolution? Did the speeches not confirm the uncontested belief in economic science as such? After all, Keynesians also profited from the scientification of economics, even if not in relative, but in absolute terms.

Mapping Economic Science Today (2010) under the Spell of the Formalist Revolution

Then, how about economics today? Has it changed? Were the speeches successful? Yes, economics has changed. Economists could partially regain the empirical grounds of their science by means of several theoretical innovations that since the mid 1970s took roots in the economics departments of Western universities. GET as the theoretical paradigm of microeconomics came to be replaced by game theory, econometrics is increasingly a required part of most core journals, and rationality is more contested than taken for granted. Even experimental methods became presentable again, and the profession intensified its relations to other disciplines such as psychology. MIT, for example, has an actual laboratory testing the efficiency of developmental policy. Moreover, heterodox economics has experienced a great revival. Associations like the *Association for Evolutionary Economics* (1972), or the *Association for Institutional Thought* (1976), and journals like the *Journal of Post Keynesian Economics* (1978), or the *Contributions to Political Economy* (1982) sprung newly out of the dry ground that the formalist revolution left behind. Heterodox schools re-politicized economics and provided considerable alternatives to the "mainstream" – apart from those in the *Union for Radical Political Economics*, who had to stay out. Did Leontief's, Galbraith's, Gordon's, and Boulding's dreams come true?

The laments of the 1970s, however, are not outmoded. To the contrary, booing economics has become intensified, professionalized, radicalized. The speeches of the 1970s have grown into an entire industry of anti-economic literature. There we continue reading the same points that have been made in the 1970s against mathematical economics – lack of normativity, modeling for its own sake, trade-off between rigor and relevance, lack of genuine empirical work, a-historicism etc. The need for complaining did not seem to be met by the mentioned theoretical innovations. One of those who provided new material for continued complaints in the 1980s and also 1990s was McCloskey's Rhetoric of Economics: "the mainstream of normal science in economics, I'm afraid, has become a boys' game in a sandbox. It has become silly" (1998 [1985]: 189). Heterodox economists, ahead of all the others, continue the tradition of lamenting: Wards book of 1972 (*What's Wrong with Economics?*), is updated in Fullbrook (2004) *A Guide to What's Wrong with Economics*, and Linder's *Anti-Samuelson* is updated with Stanford's (2008) *Economics for Everyone*. Also the teacher's petition of the French post-autistic movement repeats precisely what Gordon said, namely "the naïve and abusive conflation that is often made between scientificity and the use of mathematics" (Fullbrook 2003: 17). Their slogan of "post-autistic economics" did address – even if in an ad-hoc and youngish way – the intellectual sensibility of economist. Unfortunately, the movement has changed its motto recently into "real world economics" – another instance of phenomenological failure.

The student's movement is not the only case of this failure. There have hardly been any advances in terms of the phenomenological confusion in spite of the increased sensibility for the social reality of economics – professionalized by the Sociology of Scientific Knowledge. Take for example Arjo Klamer (2001). The causes of the disease he mentions are standard, putting yet more emphasis on modernism: occupation with representation, turn inwards, formalism, break with history, no tradition, self-referential, professionalization, departmentalization (Klamer 2001: 81 f.). As many others, Klamer believes the nub is that economists aim to prove to be bad anthropologists, and that all there is that went wrong is the misleading image of the economic agent as the maximizer of utility. MaxU amounts to the "disappearance of the human subject", "the loss of character" (93). MaxU is like "a man without qualities", no history, devoid of moral sentiments, "oblivious to the uncertainties and insecurities that plague anyone who has to make choices" (Ibid), and is "caught up in lifeless problem-solving exercises, and cannot tell us how to live" (94). "Take it away [the metaphor of Max U] and the work done by economists over the last sixty years is a big heap of insignificant mumblings" (Ibid). If everyone already behaves rational there is no need to improve: "Voila, the end of any social responsibility of economic science" (98). The phenomenological confusion is apparent. For what is at stake? Is it the modelling of agents, or the character of economists? Regarding the former, there is no economist who would not agree with Klamer. Regarding the latter, why should it be related with economic theory?

Heilbroner and Milberg, who I would grant to be closest to a phenomenological locus of critique when speaking about the 'loss of vision' (1995), commit this failure. Also they were seduced to think of this loss in terms of theories referring to realities: After Keynes, they write, "the mark of modern-day economics is its extraordinary indifference to this problem (…) of the connection between theory and reality" (1995: 3). There are only few moments in the commentary of economics that the phenomenological locus of critique shimmers through. Here for example, Eugene Meehan (1923-2002), in the first issue of *Methodus*, the informal precursor of the *Journal of Economic Methodology*: "An inadequacy in methods is only symptomatic of more fundamental disorders – confusion or error in the set of purposes taken as the basis of inquiry" (1989: 8).

The phenomenological confusion is crucial for the reproduction of the crisis of economics. It calms the emotions of discontent down to a novel "unite of analysis", a new "model", a new "paradigm", which may give hope, but ends up renewing the same dissatisfaction. Due to the phenomenological confusion, critique of economics cannot do anything other than create more visions of a better economic science, and therefore reinforce the uncontested belief in the virtual worthiness of economics. Only after this phenomenological confusion has been resolved can the tone of critique be advanced.

It is thus not surprising that some of those who contributed most rigorously to the laments of the 1980s now make us believe that things are going better. Some deem the crisis to be over. Most famously David Colander, who once argued that economics systematically induces cynicism, now argues that the profession has taken its lesson and is back on track.

Complexity theory, which I consider in a moment, should give us new hope. Others, like Heilbroner and Milberg, instead, remain skeptical about the empirical turn Colander celebrates:

> The retreat from theory ultimately leaves unresolved the crisis of vision that we describe in The Crisis of Vision in Modern Economic Thought. If anything, as widely different tendencies in economics vie for the mantle of pragmatism, it is likely that the question of vision will rise to the surface instead of looming in the background of economic discourse (Heilbroner and Milberg 2002).

With the two positions of Colander and Heilbroner in mind, let me go through the landscape of present-day economics as I have mapped it on the next page. What do economists believe makes their work relevant (second row)? What do they believe makes them epistemically more authoritative than the rest of economic talk (third row)? And what do they believe makes the work of other economists irrelevant (fourth row)? Thus, what describes the ethos of economists today, according to their self-understanding?

Rather than going through each single stream and school of thought, let me cut short such discussion with some simple observations that suggest economists are still in the grip of what happened during the formalist revolution. My comments partially summarize the couple of centuries of history of economics that I have drawn in the preceding chapters.

Needless to say, the inner borders as depicted are contested and crossed in all directions. There is a whole industry of writings that pull economic claims from one school to the other – who actually has the authority to talk about "uncertainty"? Austrians? Keynesians? Behavioral economists? Or even game theorists? Also the line between the orthodoxy and heterodoxy is contested. Think for example about Oliver Williamson, one of the main proponents of New Institutionalism, who made a career by becoming more and more orthodox, leaving the intuitions of old institutionalists behind. Or think of evolutionary game theorist Payton Young, who hardly anyone would doubt to belong to the mainstream. Or think of the experimental economist Vernon Smith who embraces the motto "institutions matter" (1989: 156). Should I thus embrace John Davis's conclusion that the orthodoxy is about to incorporate the heterodoxy (2007a, 2007b)? The clearest borders with the least exchange may be drawn around Austrian economics, on the one hand, and feminist and Marxian economics, on the other, although these borders are also contested. You need to read the *New Left Review* in order not to run into reference to the economic discipline on each page.

What then holds economics together? What identifies a theory as an economic theory?

(1) *Negative closure*. I depicted the present streams and schools of thought in such a way that all establish their identity in opposition to a school of thought, of which no economist is part. There is nobody who represents "standard economics" or the "orthodoxy". Insofar as this "standard" yet marks the borders of the discipline and the rest of economic talk, I may call this feature the *negative closure* of the discipline.

There certainly have always been standard references in the history of economic science – Smith, Mill, Marshall, Samuelson. Peculiar today, however, is that the standard reference is a straw man. There is "standard economic theory", "neoclassical economics", "orthodox economics", or the "mainstream" without anyone who is willing to defend it. The "standard" to which the orthodoxy refers, and the "orthodoxy", to which the heterodoxy refers are

Economic Science (2000)

Orthodox Economics

Stream of thought	What matters	Scientificity	"Standard Economics" irrelevant because
Game Theory (von Neumann, Aumann, Rubinstein)	Interdependence of actions, strategic behavior	Rigor	Complete competition, atomistic individual
Behavioral (informational, cognitive, neuro-) Economics (Kahnemann, Camerer, Tirole)	Cognitive processes, learning, etc.	Positivism	Perfect Information, perfect cognitive capacities, perfect rationality, perfect foresight…
Experimental Economics (V. Smith)	'what economic agents really do'	Empiricism	Not testable
Complexity Theory (B. Arthur, S. Durlauf)	How everything-is-connected-with-everything	technocratic empiricism	Deduction, Isolation

Heterodox Economics

School of thought	What "really" matters	Scholarship	"Orthodoxy" irrelevant because
Institutional Economics (Williams, North)	'Institutions constrain and enable economic performance'	Descriptive propriety, conceptualization	Markets not being an institution
Evolutionary Economics (Nelson and Winter, Young)	Dynamic development of economic character traits or genes	Technocratic historicism	a-historical, end-state conception of markets
Austrian Economics (L. Lachmann, P. Boettke)	Human action (free will, teleological, uncertain)	literacy in Austrian tradition	algorithmic choice, applicable to non-human events
Post-Keynesian Economics (P. Davidson)	Irrationality, short run, uncertainty	literacy and policy relevance	No contingent institutions (expectations, unemployment)
(Post)Marxian Economics (P. Sweezy, D. Ruccio)	class, power, ideologies, conflict	Materialism	Markets clear ideologies, power, conflict…
Feminist (J. Nelson)	Sex	Politics	Male

basically one and the same straw man. Since the "orthodoxy" that the heterodoxy refers to is somewhat non-existent, it is not surprising that some argued heterodoxy is a strategy to gain higher shares in the profession, as well as, that the orthodoxy is actually pluralistic (Davis 2007a). Another ironic consequence of this negative closure is that there are anti-economics textbooks that criticize economics on the same grounds as those who fly the flag of economic science highest. Regarding the received view on utility maximization, for example, Steve Keen's *Debunking Economics* (2002) goes well with the evaluation of what Colander would call the cutting-edge elite of economics (2004), such as Jean Tirole and Roland Benabou (2006). Both agree that MaxU is not All There Is. While the latter waits for the Nobel Prize, the former waits for the occasion to abolish it.

Although merely fictive, the bogeyman of the "neoclassical economist" is vital for the rhetoric of significance of all schools. No economic school establishes its importance in opposition to general economic talk outside the academic discourse. Would an outsider not expect basic statements such as, 'problematic about a globalized economy are not the greedy multinationals, but the information technology that allows them to be greedy', or the like? The conceptions of why economics is relevant do not stem from outside the discipline, but from within theory. "Standard economics", even if nobody believes in it, functions as the frame within which all economic theory can be presented as relevant. A *specter* of "neoclassicism" constitutes the discipline of economics today. Neoclassical economics is indeed, as Colander argued, dead in that this tag, whatever one associates with it, does not describe the practice of any economist (2000). But neoclassical economics is only half-dead since it lingers as the reference point for all theoretical innovations.

Another ironic consequence is that in a seemingly pluralistic science that is vivid in contesting its inner borders, many complain about the monism of the discipline. In 1992, Hodgson, Mäki, and McCloskey published a "plea for *pluralistic* and *rigorous* economics" in the AER (1992, *e.a.*). Among others, Franco Modigliani, Robert Axelrod, Brian Arthur, and Paul Samuelson signed the plea – who knows if more for the former or the latter reason. Another wave took place after the post-autistic petition, critically surveyed and examined by Davis and Sent (2006). Should it not make us skeptical that such pleas proliferate all the more the discipline attempts to break through the "neoclassical monism"?

At this point, I can come back to the *interpretive indifference* with which I closed the last part. Each school of thought is established as a different *interpretation* of what is wrong with the mainstream. Differences of schools, then, do not correspond with different world-views but with different interpretations of what the mainstream really requires. All schools thus presuppose that standard economics is constituted by referential truth-claims, that is, by a particular representation of reality. However, this is not the case. Interpretations in the so-called mainstream, as the preceding narrative suggested and the following part will make clear, are a *datum* of economic theory without affecting it. Just because the mainstream is independent of an actual perception of economic reality everyone can refer to it with a different interpretation. Neoclassical economics came into being, as I showed at length, because it brought economics *beyond* its meaning.

The disciplinary identity of economics is constituted by the common knowledge of what economic theory is *not* about, which nevertheless determines the frame of what economics can *possibly* be about. "Standard economics" is the common language of economics. Thus, outside economic science are those who do not use this language, that is, those who are not informed by formal training in economics. The discipline of economics is determined by the commitment to a language that stems from a theory no economist actually believes in.

Next to these ironies, the negative closure also entails a tragic moment. As every school is established by an interpretation of what is beyond interpretation, these schools themselves can be understood as an interpretation of, rather than an alternative to, standard theory. Expressive practices and the attempts to make a difference are therefore easily undermined. Maintain the theoretical "structure" and skip the pathos of the claim! – so the imperative of the "sophisticated mathematician" who tears away the differences of all schools of thought in a twinkling of an eye. Keynesian economics, historically speaking, was the first victim of this gesture. For each school of thought, one can read its alternative theory as a *defense* of mainstream theory. Such is the source of immunity in economics, above all to internal criticism. Being immune, one does not have to respond.

There is a certain awareness of this tragic moment in discussions of the heterodoxy, as here for example in Arnsperger and Varoufakis (2006): "There is nothing more frustrating for critics of neoclassical economics than the argument that neoclassical economics is a figment of their imagination". The two authors explain the common rejection and simultaneous success of neoclassical economics in that it can "hide" its ontological foundations in technicalities. The tragedy, for them, is that these technicalities are socially reinforced by the profession, so that the foundations always evade efficacious criticism. Arnsperger and Varoufakis, however, suppose that *there is* an actual substantive foundation to hide – rationality and equilibrium. They perceive these foundational concepts, as most other social philosophers today, as referential terms. Does an epistemological reflection on the "hidden" foundations really help escaping this tragic situation? Were the attempts to overcome "equilibrium" and "rationality" not pervasive since the 1970s? According to my idea of interpretive indifference the "foundations" of neoclassical economics can only be "hidden" because they are *not* of a referential kind. Precisely this constitutes the tragedy: theoretical alternatives can be easily incorporated in the mainstream via the *structure* of theory beyond referential claims. Structures cannot hide! They only can be imposed!

The rhetorical straw man of standard theory correlates exclusively with what is taught in *textbooks*. Let us not forget that since the postwar period, since Samuelson's *Economics*, the teaching of economics hardly changed. As the post-autistic economics movement stressed, economic teaching culture is still not yet informed by the theoretical changes that took place since the late 1970s (Colander 2005). The main access limit into the profession, created only about ten years ago, still bears a neo-Walrasian name: Mas-Colell et al. 1995. There is still no undergraduate teaching specialized on behavioral, experimental or institutional economics. If this happens, what could hold the profession together? According to my map: Nothing!

It may be this shared experience of having thirsted through Mas-Colell et al. that economists stick to the identity of economic science – comparable perhaps with the tradition of female circumcision. Thus, between the current fortress of economics and its dissolution, there is no more than the indoctrination most economists reject anyway – recall that I spoke of a light shove that is needed. Not more.

(2) *Lack of Scope and Method, and Advanced "Inverted" Imperialism.* I have argued above that the scope and method literature – which, recall, I pictured in the context of those who extended the scope of economic life to an explanation of consciousness, or at least to a mobilization of revolutions – ceased after the anti-philosophical interventions of Becker and Friedman. Indeed, the different branches of economics do not differ by different definitions of the scope of economics. Although, according to the "what matters" column, economists seem to hold different notions of economic life, there is no need to actually defend it. Economists today do not have to hold an expressive notion of the scope of economic science, or any concrete idea about the ontological properties of "the economy" in order to gain disciplinary identity. Economic theory is not based on the "distinctions between phenomena that are of chief economic importance" (Keynes 1999 [1890]: 75). Nobody will ever ask you until the press calls and wants to know why you got the Nobel Prize.

First year students, of course, want to know what economics is. The most popular classroom definition is still Robbins' 'relationship between ends and scarce means which have alternative uses'. Whatever this means, from the second class on, students understand that one cannot earn much credit in the definition business. In heterodox economics, there are neither competitive definitions of the economic realm. Robbins' definition functions here as a benchmark for considering *also* institutional dependency, *also* distributive issues, etc. – as though Robbins had merely narrowed the scope of economics rather than actually downplayed the very question of scope.

If one thinks of the scope of economics more formally, as that through which economics is discursively identified, it only can be its own straw man: the constituents of neoclassical economics as a "paradigm" – equilibrium and rationality. The omnipresent reference to these two notions represent the "turn inward" in economics insofar as economics became its own object, or better: a misunderstood past of economics became object of economics. Also commentators of economics discuss disciplinary changes in terms of these categories. Officially, rationality and equilibrium economics have been dissolving since the 1970s. Is it, as the negative closure suggests, not the other way around that *in* this dissolution, equilibrium and rationality are reconstituted as a "paradigm" in exactly the Kuhnean sense that its historical meaning is *not* discussed any longer but simply assumed – even if assumed to be wrong?

Closely related with the lacking perception of scope is the issue of economic imperialism. It is associated with Becker's understanding of rational choice (1976). Economic imperialism refers to economic explanations of phenomena traditionally beyond its scope, or better: "the intrusion of virtual markets into every nook and cranny of experience" (Mirowski 2004: 380) – utility maximization of having children, taking drugs, donating organs (Becker and Elias 2007), or also the utility maximization of democracy as compared to dictatorship (Acemoglu, Robinson 2006). Economic imperialism also refers to the framing of discussions in non-academic economic talk when politicians and other discourse makers adapt market language of efficiency and rationality. Economic imperialism became part of the popular image of economists.

Frey and Benz (2004) and Davis (2007a) argue that after the 1970s an *inversion* of this imperialism took place. Methods of other disciplines conquered economics. It is true that most methods in the orthodoxy have their origin not in economics, but are taken from other disciplines – as it has always been in the entire modern history of economic science. The main source is certainly the psychology department, but also computer- and neurosciences. Measuring brain activities when making decisions – which neurotransmitter is released when "being trusted" or "being suspected" – gave some economists the opportunity to actually place economics in science journals (McClure et al. 2004). Such phenomena lead Davis to speak of a turn towards pluralism in mainstream economics. Neoclassical economics is not only dead as describing theoretical practices, but is also dead "as a characterization of how mainstream economics is currently evolving" (Davis 2007a: 2).

We can, however, still observe very similar moves as Becker's in highly esteemed and rigorous behavioral economics, for example in Benabou's and Tirole's modeling of dignity as an "asset" (2006). How could one possibly count this as an input from social psychology? That the framework of rational choice misses fundamental features such as self-worth has been one of the traditional arguments against rational choice (see for example Shaun-Hargreaves 2001). What Benabour and Tirole do with this insight is to strip off its meaning, put it in a theoretical structure, play with their technologies, and see how the connotations in this play change and perhaps appeal in a reportable behavioral account of "dignity" – like playing with a kaleidoscope. Whether the critique of rational choice, social psychology, or whatever is written in whatever magazine, serves as the meaning-dispenser of this light play hardly matters. Calling this an example of reversed imperialism is like saying Becker's account of the household was an import from the "home economics" department that it actually has replaced ("home economics" department were professional schools for housewives).

Economic imperialism, moreover, could also be seen as *advanced* in the cases that seem to reverse it. While once in the 1950s Becker spoke of rational choice theory as the *method* of economic science beyond the traditional domain of economics, now rational choice becomes the *object* of economic science. Consider, for example, the object of neuroeconomics. One of its main results is that emotions matter for decision making, particularly in cases of ambiguous choice when there is "fear of the unknown", or in cases of strategic interactions when one feels "disgust" of being treated unfairly (Camerer, Loewenstein, Prelec 2004). What makes such claims and economic claim? Rational choice! Why should anyone ever have doubts about such a claim if not on the background of a particular interpretation of rational choice in that it excludes "emotions"? For someone who is not trained in economics and does not know of the centrality of rationality, he or she would not identify that as an economic claim.

As with Becker "rational choice" became method where it was once a basic property of a domain – once when economists knew what it means to actually pursue wealth – rational choice is now the *object* of economic science. If there is any definition of economic science today, I would suggest this: the science of the limits of rational choice and equilibrium theory. Such one could call *advanced reversed imperialism*, saying that other sciences, above all behavioral sciences, adopt "rationality" as the very *scope* of their science. Camerer, Loewenstein and Prelec indeed argue that neuroeconomics can serve as a ground for "unification across the social sciences" (2004: 573). I would even consider analytic philosophy of action as a case of this advanced reversed imperialism insofar as rational choice is there also the object of scrutiny. Note, however, that at that stage of imperialism there is no longer the need to refer to the discipline of economics: If economists use methods from other sciences to consider as an object what was once their own method, is this not a sufficient estrangement that should loosen the need to identify with the discipline?

Note also the *selection bias* regarding the sciences that are allowed to enter the discipline (Davis 2007a: 10f.). Economics, very successfully, includes those sciences that support its appearance of scientificity. Other disciplines find inroads into economics only with the analytic flavor of hard sciences – demarcationalist, exclusive sciences. Equally successfully, however, economists exclude economic discourses that do not support its scientificity, as great parts of sociology (think of Zygmunt Baumann, for example), anthropology (think of post-colonial studies), political science (think of Negri and Hardt), and of course the entire Marxian tradition (think of Slavoj Zizek). They all care about the present state of capitalism, and thus what Gordon has perceived as the really relevant question. At most these debates enter economics as a source of examples for showing, with sophisticated methods, the limits of rationality and equilibrium. If there was something to be called a "research program" of today's economics, this is it.

Let me only present one symptomatic statement of this research program by one of the leading behavioral economists, Mathew Rabin.

> (A)bandoning the view that hypothesis departing from rationality, self-interested, or other habitual assumptions are methodologically illicit can free us to evaluate these hypotheses with the same rigorous standards that our discipline, at its best, applies elsewhere (1998: 41).

The self-understanding of this economist underlines my remarks: *object* is rationality, *novelty* lies in the departure from it, and the *scientific pathos* that stems from it is rescued.

(3) *Historical dumbness*. Not to be able to reflect on one's historical roots, as not only Kuhn taught, is a clear sign of an advanced state of scientification. The most conspicuous benchmark of the ongoing effect of the formalist revolution is that economics is as history-blind as it became half a century ago. Whatever the speeches in the 1970s achieved, they did not cause a reflection on whether one should reconsider, for example, the ostracizing of historicists, or to reconsider the socialist calculation debate.

Economists are not only ignorant about their past, they are increasingly less interested in the analysis of the current state of their profession. As soon as one of the authorities I named in the map receives the Nobel Prize, the historical background of their work can be excusably skipped. References to previous work usually do not exceed the past decade. For this reason, even the analysis of the present streams of thought are left to historians, as shown by the recently established *Society for the Recent History of Economic Thought*.

Economics is forward looking – and thus possibly runs in circles without knowing. If there are historical cycles of paradigms from crisis, innovation to normal science, in economics these cycles are like seasons in hell, repeating the same over and over again. Every single row of the orthodoxy makes its own case: What exactly happened between Game Theory I (von Neumann and Morgenstern 2004 [1944]) and Game Theory II (Aumann 1985)? Who still considers the The Theory of Games of all who quote it apart from the historians? How come that "perfect competition and Nash equilibrium go so well together" if the latter supposedly replaces the former (Rizvi 1994: 11)? If game theory officially deals with agents' "power", why was it never clear "to what question is 'Nash equilibrium' the answer"? Did game theory not simply "rescue" the neo-Walrasian rigidity, yet run into similar formals limits (Ibid.: 6)? Second, behavioral economics: When announcing the cooperation of psychology and economics as for example Kahneman in his Nobel Lecture (2003), was it informed by any reflection of how and why psychology came to be excluded from economics – because, as I suggested, it was much too indiscreet for the weak nerves of any liberal? Third, experimental economics: Was it not necessary for the very theoretical abstraction of "the economy", at least for Mill, that experiments are *not* possible in economics? Running experiments, on the base of what kind of intuition of "the economy" does one make economic claims? Or is it rather rational choice theory that is tested? Last, complexity theory, which would require the most far-reaching historical reconsideration: What are the conditions of perceiving "the economy" as an "evolving, complex system"? Was there at any time an economist who did not perceive it as such? No, there was not. Otherwise one cannot even think of the very object "economy". Complexity is one of the oldest rhetorical moves to evade dealing with the concrete!

If I had to draw the line between heterodoxy and orthodoxy, it would be at this joint of historical contestability. The heterodoxy, and therein lies its potential, supports *scholarship* and *literacy* in their traditions. Some institutionalists read the classics of Veblen-Ayres-Clark. Some evolutionary economists read Schumpeter (but not Knies or Schmoller!). Post-Keynesians – as opposed to the Neo-Keynesians or New Keynesians – actually read Keynes. Austrians engage in extensive exegesis of the Menger-Mises-Machlups. And Marxians would cease being Marxians if they stopped reading Marx. For this reason heterodox economists tend to embrace historians. And for the same reason, historians tend to take actual positions within economics.

The line between orthodoxy and heterodoxy also appears as an ideological difference. Does the orthodoxy tend toward liberal, and the heterodoxy toward socialist aspirations? The question of ideology ran through the entire preceding social history of scientification. What is the status of ideology in economics today? Are there explicit links between economic theories and the epistemic claims of particular politics? Thomas Mayer, for example, argued that there are, but that they are not significant enough to explain their disagreements (2001). Of course, in Austrian and in Marxian economics the ideological dedication is obvious. Without an explicit political motive an Austrian is not Austrian, and a Marxian is not Marxian. But for just this reason both are at the edge of falling off of the list of economics. The lower you get on the map of the discipline the more difficult to score in any of its rankings. The higher you get the less economists are expressive of any political motivation. Certainly, one may say that public choice theory was intentionally designed in order to justify certain politics (Amadae 2003). It models political agents as market agents and thus undermines politics in that it is supposed to govern the market. Yet, the same theoretical intuition could be employed for an opposite politics to improve and thus enhance state activity. Even in the most obvious cases the link between theory and politics, at least in the orthodoxy, is never trivial. Between economics and its applications there is a gap that is not covered by the discipline. Others apply.

Then the question of ideology is not one of yes or no, but whether economic theories allow economists to reflect on the potential ideological utilization of their work and the effects they have on other discourses. The question of ideology today is no longer: Who Are You? – Arguing This! But: Do economic theories undermine the reflection on the motives that guide their work? In which sense can economists be made *socially responsible* for the use and effects of their work? This question delineates the horizon in which the following part is written.

(4) *Technocratic empiricism*. Around the turn of the millennium the commentary of economics began to adopt a new tone away from the critique initially set by the Keynesian generation. Some want to make us hope for a release in *complexity studies*. A new empiricism, so they claim, replaces the formalist revolution in that it doomed economics to the deduction from monist principles (rationality and equilibrium). Did economics come over the "paradigm" of formal equilibrium theorizing and moves toward a new theoretical identity? Above all Colander, one of the harshest critiques of economics in the 1990s, in recent years became supportive of complexity theory. It is worth quoting at length.

> [L]ooking at the profession today, I am convinced that it is quite different than it was in the mid-1980s, when Arjo and I first sat over drinks and lamented the state of the profession. The commitment to theorems and proofs has declined, and there is a much stronger empirical branch of economics. Natural experiments and instrumental variables are now central to an economist's training. Behavioral economics has advanced enormously (…); advanced time-series statistics, such as cointegrated structural VARs and calibration, are commonplace, where they were hardly known before. What were taken as requirements of research in the 1980s are no longer requirements in the 2000s; the holy trinity of greed, equilibrium, and rationality has been replaced by a looser trilogy of purposeful behavior, sustainability, and enlightened self-interest. (…) economics has changed and will continue to change, making it impossible to call the existing profession neoclassical any longer (2007: 15).

Colander is here in best society with Mas-Colell, who predicted the same twenty years earlier at a time when Colander booed loudest about the work of Mas-Colells:

> The axiomatic method is still all pervasive in economic theory but I would not venture to say that it will be so forever. I do not know what our subject will be like in 50 years, but I would not be astounded to find that the computer revolution would have changed the character of what we do and how we do it, or, for that matter, what the mathematicians do (Mas-Colell 1987: 323).

The computer revolution that Mas-Colell and Colander envision is on its way at the latest since the 1987 conference in Santa Fe. This conference gathered economists, physicists, biologists and computer sciences. Since then, so it is said, "the economy" comes increasingly seen as an "evolving, complex system" (Arthur, Durlauf, Lane 1997). In complexity theory economics is indeed finally liberated from the meaning of its principles. One can add to the technical model whatever specifications of heterogeneous agents, institutions, non-linear stochastic processes of evolution, simultaneous competitive and cooperative mechanisms, specific closed and open "networks" and what have you. As opposed to the deduction of claims from rigid principles, now highly dynamic processes are scrutinized: "emergence of properties" (irreducibilities), "positive feed-back loops" (mutual causation), "path-dependence", "lock-ins", and other non-linearities make some hearts beat faster. They give a fresh color to the *spontaneity* of "the economy". But "the economy" as what? As *Structure*. So, what is new? Does this new technocratic empiricism challenge the ethos of economists?

Colander, Holt, and Rosser seem to suggest so when speaking of a changing "face" of economics (2004). But do economists see others differently? And do others see economists differently? The new technocratic face indeed liberates economists from the chains of economic theory. Theory becomes secondary. It merely serves next to the data as one input in the technical model. The model, rather than the theory, is thus the focus of attention. The imperative is now to operationalize the limits of representation of formal models, to use these limits as a tool (Mirowski forthcoming). Theoretical assumptions are heuristic inputs in the model, not their ordering principle. Data and theory are equal components of a model that is not limited by its function of representing reality. Models do not represent reality, but they simulate it. Theory and reality are thus not strictly separate, but intertwined (the same counts for the Natural and the Social).

> New Millennium economists do not believe that they are testing a particular model which was deduced from first principles; instead they are simply looking for possible exploitable patterns in the data! (Colander 2000: 128).

Truth claims now move behind a more pragmatic practice of science. Software can randomly produce hypotheses, the meaning of which is up to the fantasy of the scientist. According to Colander, the novelty is the absence of a dominating structural principle of theorizing "the economy": "In 2050", he envisioned in 2000 "the belief of economists in derived analytical models has given way to a belief that the underlying reality is too complex to be understood with these sorts of models" (2000: 127). The practice of economics is decreasingly deductive, and comes closer to what scientists do in weather forecasting – von Neumann thought that whether forecasts are the chosen field for the use of computers.

It is here that my argument regarding positivism and formalism in economics finds its last touchstone. I downplayed empiricism already at the very institution of economics as a science. Constitutive for the perception of an object of "the economy" was the rhetorical diversion of the merchants, not the technology of William Petty. But did Thomas Mun and Nicholas Barbon, as well as every layman who listens to economists today not also believe that "the economy is a complex system"? Was "complexity" not always constitutive for the very perception of "the economy"? Did economists ever believe that market order can be derived from a unique principle of rational behavior? Was rationality ever understood as an anthropological truth? Was complexity not always crucial for the very establishment of the ethos of economists? Was complexity not first of all a way to evade the question: Who Are You - Arguing-This!

The questions I have to pose regarding the non-deductive technocratic empiricism in light of the preceding narrative are these: Do complexity studies move beyond the modern triad of science, technology, and growth? No. Do economists regain their literacy, that is, their capacity to express and to communicate? No. "Though the axiomatic method may become obsolete, we shall certainly not go back to literary economics" (Mas-Colell 1987: 324). Do economists come closer to their perception of economic life? No. Does complexity theory reverse the structuralist turn in economic writings? No. To the contrary.

Complexity theory brings the structuralist turn to the point where it leads into the dissolution of economics. Structuralism in economics never primarily meant that theory is structured by a *principle*, but that theory is *separate from meaning*. And precisely this was *the* virulent feature of the formalist revolution rather than its alleged basic beliefs, assumptions or principles. The axiomatic method shows just the same playfulness that is celebrated as the liberation from its deductive rigidity. Consider, for example, how Mas-Colell describes the axiomatic kaleidoscope of meaning.

> You have three interesting phenomena that give striking impression that something common is going on. You then succeed in building up a theory where the three phenomena are unified under four characterizing axioms (…). Now, as soon as you have four axioms this gives you 16 different combinations of axioms. You may not have thought of any of these 16 theories, and it may turn out that one or two of them are quite interesting, so that you have discovered something. But the danger is that the other 14 may be utterly uninteresting (…) It is an art to discern the interesting combinations of axioms to explore from the uninteresting one that should not be pursued (1987: 324)

A playful art – is this liberation not the same in complexity theory, even if Mas Colell does not speak of the play of empirical patterns? Is it not the same structure of "the economy" that was conceived 50 year ago and all the more in complexity theory? Complexity theory is a child of the formalist revolution. Period.

The positive lesson we can draw from complexity theory is the same as that of advanced imperialism. There is no reason to expect that complexity studies will reinforce the disciplinary identity of economics – as for example Davis suggested softly in association with a smooth historical cycle of unity and disunity of economics (2007a). Technocratic empiricism will not be an empowerment for economists. To the contrary, with the end of the unifying principles of equilibrium and rationality, the economist is *less* identifiable as an economist as before, *less* distinct from other sciences than before.

Only in this respect, I approve Colander's evocation of a turn in economics (2004). The chance that lies in complexity theory is that disciplinary border politics loose their grip and become redundant. The pretension of economics being a separate science could fall apart. And in this dissolution, as it will be fully comprehensible only after the next part, the empiricist turn stands actually in *continuity* with the formalist revolution. Both stand for the decreasing need of referring to the identity of economics when arguing something specific. The meteorologist can be just as good as an economist as any other person who knows the software. And the economy-forecast released by the MIT developmental lab will be taken just as serious as the weather forecast for the next weekend. In principle, one does not need any economic theory in order to make one's point. A vague intuition of what others perceive as economic life and the knowledge of software suffices. And this is the case not since 10 years, but since 60 years – since the formalist revolution.

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Whatever one believes and hopes about the discipline of economics regarding theory today, the preceding remarks suggest that economists' ethos is, phenomenologically speaking, in the same state as after the war. Just think of economics from the point of view of the entire last century, and compare the second half with the first half. Nothing in the second half recalls the weight of emotions that accompanied scientific optimism in the first half. The ethos of economists is no longer challenged. Even if economics heads straight beyond the neo-Walrasian paradigm of GET, rationality and mathematical rigor, it yet moves even farer beyond the ethos of political economists that still determined the professional ethos before the formalist revolution.

Negatively expressed, economics today is unified by the economics of the years after the war: *equilibrium* and *rationality* as a negative criterion of closure. Economic theory without any reference to equilibrium would fall short of theory. It would return to history (literacy), to instruction (technology), or to preaching (moralizing). Positively, the discipline is unified by means of the uncontested image of *scientificity* and the immense institutions it earned. Issues of legitimacy are out of the game.

And so I have arrived at a second preliminary formulation of my conclusion regarding the end of economic science. The open question to be posed is now: Why is it not possible that the events of the 1950s can still be reversed? What is the problem of *formalism* and the invisible hand if it is not a matter of theory? The problem of the formalist revolution, I introduce in the next transitional chapter, was *not* that it did something *to* economics that it does not deserve. Instead, it showed something *of* it.

(5) Taking Stock – Zooming In

Having gone through the intervention of economic science in modern European history, it is time to take stock. I have presented the basics of a social history of the scientification of economic writings. It culminates in a present situation that is framed by the formalist revolution of the 1950s. This revolution, represented by the work of the neo-Walrasian community, made an end to the scientification in the sense that the meaning of economic science lost greatly in contestability. Or better: the search for scientific authority no longer affects the social ethos of the economist. Although *what* economic science remains contested (for it determines the share a stream of thought earns in the discipline), the very existence of economic science is no longer at stake in these contests. Since the post war period economists occupy an uncontested place in western academia. There are many differences in economic theories and even differences regarding the underlying concept of scientificity – allowing within the orthodoxy for more or less empirical reference, and allowing within the heterodoxy for more or less politics and history. But these differences that we can observe as an aspect of the factual history of economic ideas mean less *to the economist*. Differences matter less. They no longer cause the same intellectual emotions, make the same demands, and need not to be played out against each other. It is no wonder that for some the struggle among economic schools appear like a strategic game for power. What else is there to be motivated by?

Thus, there is nothing to add to the ethos of economic scientists. Economic science came to be – too late to celebrate or to complain. The question now is no longer 'Which economic science?' but 'When does it disappear?'

Wrapping up Economists' Intervention in Modern History: Economic Suspicion, Formalism, and the Axiomatic Method, on the one Hand, and "the Economy", Invisible Hand, and General Equilibrium Theory on the other

In the course of the preceding narrative, one driving force of scientification appeared again and again: the economic suspicion. It functioned, broadly conceived, as the instituting moment and social engine of scientification. The main lesson is that economic science, yes, overcame this vulnerability. However, and this is perhaps the greatest historical arch I have made in this part, the tradition of suspecting economists had its draw-backs; I have drawn a *reversal* of the suspicious ethos of economists into the lament of their insignificance – a reversal from the suspicion of being motivated by a particular interest into the lament of not being motivated by any interest. Formalism is the drawback in the attempts of economic science freeing itself from ideology. Formalism is the invisible hand that "harmonizes" all disagreements and interests among economists by means of lowering the weight of what can be said.

Thanks to formalism, economists gained their historically awaited scientificity. Scientification in economics never took place by means of "discovery", or by means of innovations of methods that enable the "discovery" of certain truths. None of the virtues that are commonly associated with scientific claims of referential truth made the economic scientist. For this reason, I have downplayed all attempts to establish economics as a positive, objective, exact, or empirical science. To some extent, I have thus arrived at a similar point as in the last part where economics moves at the edge of its irrelevance. But in light of the reversal of the economic suspicion and the formalist tendency I now can say that *it has never been different*. It was always a *discreet* appearance that made the economic scientist, and it was always *contingent associations* that made science exploitable for particular positions.

Note, that I did not state a tendency to formalism in that economists took a particular philosophical position about their knowledge. Formalism accompanied the scientification in that it refers to lowering one's voice, or lowering the expressive tones of economic claims – lowering claims to notes, to corollaries, to remarks. Adopting this attitude in opposition to those who cannot stop preaching, and call for revolutions – such attitude made the economist.

Formalization in this phenomenological rather than philosophical sense has shown at the beginning, the middle, and at the end of my narrative. At the beginning, the *Urstiftung* of establishing a level of systematic knowledge came as a rhetorical gesture to appear beyond the imposition of mercenary motives. The first task of economists was to evade the question: Who Are You – Arguing This! After the ground had been prepared to avoid this question in mercantile times, political economists could achieve the status of scholars, though only in association with a specific political position: In the Name of Science: Liberty! To state socially beneficent results of mercenary motives disclosed academic scholarship, however, only if put against the protectionist interests of merchants – as if the economic suspicion was not really overcome.

In light of new social miseries of the 19th century, this association became implausible, inadequate, and challenged by the Marxian clarion call: By the Means of Science – Revolution! As a result, a unifying principle of economic life – constrained maximization – though first linked with a foundation in psychology, accompanied the rise of the economic profession at the beginning of the 20th century. This went at the cost of historians' and institutionalists' concern for the concrete, and ultimately also at the cost of finding an answer to the political meaning of science at all. Though the formalist revolution stood in continuity with the mathematical formalizations since the marginal revolution it represented a rupture of the contests about the political meaning of such formalizations. This I have illustrated mainly by the course of the socialist calculation debate. The axiomatization of GET moved the political interpretations of this theory in the background as though there was only one scientific imperative: For the Sake of Science: Calm Down!

At the end of economists' intervention – since the 1950s – in a time of high ideological pressure, the detachment of economics from its discursive environment substantiated. At last, economists can profit from scientific authority without making a particular claim. Today, economists respond to great extent to the misunderstandings they themselves have caused, or at most to the misunderstandings about their own history. In the formalist revolution the draw back of scientification became apparent: In a formal science, meaning no longer necessitates its explication. Meaning is ripped off its meaningfulness.

This formalist tendency that describes the theoretical experience, moreover, correlates with the cultivation of the theoretical perception of "the economy". "The economy" – it is ripe for a definition – is the *intentional correlate of intellectual discreetness*. It is, as it were, the extract of economic talk that lost its verve in thinking about economic life. Again, I have observed this moment at the beginning, the middle, and the end: the first perception arouse from the attempt to avoid referring to oneself – as though there was something else than selves in economic lives. Surrounded by the problem of trade, merchants could claim political representation because they referred to truths other than those of the clergy. A "phenomenal republic of interests" was perceived beyond specific economic interests – and thus a separation of the moral and social life.

In the century of high modernism, the separation of production and consumption as two elementary categories of political economy further advanced the theoretical perception of "the economy". Now the theoretical riddle of how "all that" is coordinated could replace the remnants of moral talk. The marginalist revolution, that subsumed production and consumption under one principle of constrained maximization helped in keeping distance from the moral notions of liberty on the one hand, and the material notions of labor on the other. It helped in avoiding reference to the dichotomy of socialism and capitalism. Allocation of resources appeared as a problem that even revolutionaries in whatever system, and whatever culture have to face at one point. At the end, the analytic core of GET – the determination of the price system – made it possible to discuss economic theory independent of its possible bestowal with meaning – that is, as a mathematical structure. The theoretical perception of "the economy" is thus freed from being bestowed by a notion of economic life.

What, then, happened from *the oikonomia* to "the economy"? Recall Husserl's phrase: "It is through the garb of ideas that we take for true being what is actually a method" (Hua VI.: 52, E.: 51). What was once a method that served a particular purpose – namely, to handle the economic suspicion – became substantiated in the theoretical perception of "the economy". The belief that "the economy" is an actual object, as it became more tangible through the preceding narrative, is based on a misunderstanding – the misunderstanding that talking about "the economy" is more than avoiding talk about something else. Talk about "the economy" – associated with whatever perceptions of wealth, resources, production, etc. – results from economists' discursive elevation – or better: diversion. This misunderstanding became apparent in the simultaneous success and rejection of the formalist revolution. In the formalist revolution the latent function of all advances in economic theorizing found their last manifestation: the search to go beyond economic life.

Formalism and the invisible hand (phenomenologically understood) were the two corner stones of the preceding narrative. The formalist revolution happened along a theory and a method in that both formalist discreetness and the perception of "the economy" merged: the *axiomatic method* as the separation of meaning and structure, and *general equilibrium theory* as the analytic core of invisible hand theorizing. The very fact that nobody today engages in such theory and method – although they reconfigured economics as no other theory or method before – shows that their influence cannot be reduced to their being a *particular* theory or method in a series of other available theories and methods. The axiomatization of GET had the character of a manifestation of economics rather than being an episode that has led economists astray for a moment by mathematicians and Walrasians. They show something elementary of the phenomenological constitution of modern economic science – they show something of the *experience of economic theorizing*. This manifest character will occupy us in the next part. In this chapter, I prepare the methodological ground for appreciating it.

I expect most readers to have difficulties accepting this emanation-like character of the formalist revolution. In order to appreciate it, the first obstacle to overcome is the belief that the link between GET as a theory and the axiomatic method as method is contingent. Could one not axiomatize all theories, and apply also other methods to GET? Today, even neuroeconomists speak of an "axiomatic approach" to their research (Caplin, Dean 2007). I associate them, however, *not* because the axiomatic method is the "suitable" method of GET. I associate them because, first, there is not any theoretical interest of GET that the axiomatic method could serve! And, second, because there is not any theoretical interest that the axiomatic method could serve! It is inherent in GET that it does *not* require a particular claim, and it is inherent in the axiomatic method that it does *not* require particular qualities of a theory. In the same sense as GET is independent of a perception of economic life, axioms are not representations of basic properties of that life. In precisely this negative sense they are associated. The axiomatic method could show something of GET – namely, that it does not (and never did) imply a particular economic claim. The attention that the axiomatization of GET received, and the negative closure it provides for today's economics *showed* that the engine of scientification of economic theory was to move economics beyond its possible meaning bestowal. Such is the entryway to the following part.

When associating GET and formalism – to close the preceding Big History – I do no more than what was obvious in mercantilist discourse: that theory is a matter of gaining ethos. To introduce a gap between the individual motives and the social result meant to divert from oneself. If later this distinction gave space to an analytic core of economic theory, it evermore covered this initiating sense-accomplishment of an ethos of economists. Thinking of "the economy" *demands* formalism. The axiomatic method is thus not the method proper, as though there was a purpose proper of invisible hand theorizing, but it is like its *symptom* that shows the self-defeating character of the theoretical perception of "the economy".

In this part, I have suggested that the phenomenological problem of economic science culminates in the formalization of the invisible hand. In the next part, I will show that with the formalist revolution this self-defeating character of modern economic science *as such* can be exhibited on the level of *theoretical experience*. The following methodological remarks disclose this locus of criticism.

The Transcendental Notion of the Life-World and the Manifest Character of the Formalist Revolution – to which Historians have Never Faced Up

At the present stage, the strong implications regarding the end of the social history of economics are not more than suggestive, based on Big (read: speculative) history. In order to be more expressive of the problematic of economic science, I need to make a methodological step regarding the notion of the *life-world* towards its full transcendental density. It will not provide conclusiveness to the fact of 'the end of economics', but, more important, it will increase the urgency to consider the possibility of such end.

In the first part, the notion of the life-world first gave rise to a description of economists' special world with the attempt to determine the actual significance of economics. Then, it gave rise to a (Big) social history of the scientification of economics with the attempt to keep track of the past significance of economics. In the following, it will give rise to the writing of a biography, or better: an *affective* history of the intellectual becoming of an economist with the attempt to exhibit the possible significance of economics. Now, I do not demand that economists are trivially situated within the world they share with others, as in the first part. I do not ask how economics forgot its being situated within the socio-historical world, as in the preceding part. But now I ask: what is the life-world of an economist *in its being forgotten*, that is, in the traces it leaves in experience. The guiding question is no longer what is the significance of economics, nor has it ever been, but can it possibly be significant? For after all, transcendentally speaking, only concrete and unique subjects can accomplish the meaning of economic science. Life-world is now the locus of the experiential traces after all intellectual efforts ceased being informed by their motives.

While in the first part life-world referred simply to the discursive milieu of economics, in the preceding it referred to the social history of the interest of economists, now life-world refers to the actual sense-accomplishing life of an economist as the condition of any interest – described in its deprival. Now the life-world is that world through and from which one can have an interest. The oblivion of the life-world, respectively, does neither refer to the closure of discourse, nor to the oblivion of the interests that historically initiated the entire project. It refers to the oblivion of having an interest at all – an interest that is not interesting, meaning that is not meaningful. With the formalist revolution the very conditions of the possibility of *any* interest in economics science are challenged and thus can become manifest in a lived experience: a *biography*. The subjective accomplishment of the formalist revolution, to use Husserl's methodological term proper, is the transcendental guiding threat (*Leitfaden*) into the phenomenological reduction to experience.

The methodological challenge, at this point, is certainly how one can talk about the possibility of economics in concrete terms of a biography? Are biographies not contingent and thus opposed to the necessity I intend? But note the change of intellectual value that accompanies the methodological step I invite to take. The intellectual value I adhered to was first the familiarity of a description, then the mind stretching of a historical reflection. Now it concerns the concreteness of "transcendental life". I use this somewhat dusty notion in order to seize upon the Husserlian twist to Kantian transcendentality. For Kant, it refers to the categories of cognition, which allow objects of experience to be matters of truth, at least as long as this experience is accompanied by the identity of a subject – the "I think". For Husserl, transcendentality is the title of the concrete accomplishing life which allows experience to be meaningful. Subjectivity is here not a matter of identity, but is both constituted by and constitutive for experience. Transcendentality concerns both the constitution of a horizon of experience *and* the constitution of subjectivity from this horizon. "I am the subject of my life, and the subject develops by living. (…) The Ego does not originally arise out of experience (...) but out of life" (Hua IV: 252, E.: 264). In my own words, experience is not, as it is in Kant, the faculty of conceiving and perceiving reality. It is "lived experience" that continuously evokes a subject of experience. What it means to be a subject of experience is itself a matter of experience – precisely this means to "be" a subject (see e.g. Fink 1995, Landgrebe 1982, Lee 1993). This notion of transcendentality, I presume, disarms those critical friends who conceive of subjects as the effects of their relations (for a discussion regarding life-writings in economics, see Forget 2002: 233).

Such a twist is commonly assigned to Husserl's move from static to genetic phenomenology. It provides the philosophical ground that allows me to speak of the experiential possibility of economics in terms of a biographical narrative: the self-constitution of transcendental subjectivity as a worldly being – a person. "As transcendental ego, after all, I am the same ego that in the worldly sphere is a human ego" (Hua VI: 268, E.: 264). To narrate a biography is to narrate a transcendental history of the becoming of an economist. The transcendental necessity that I claim with Husserl can only mean this: being intrigued by life.

It should not disturb, nor confuse that the style of a transcendental argument is here a biographical narrative, as opposed to, for example, the deduction of categories. A transcendental argument in phenomenology does not aim at generality. The reasoning I invite to is not an inference from one case to that of others. The concrete biography of an economist, in its concreteness, cannot be generalized. Rather, precisely in the uniqueness and contingency of one economist, I aim at intriguing other economists into a reflection on the possibility of the subjective accomplishment of economics. Generality, in other words, is void of transcendentality, and vice versa! The opposition of the contingency of individual lives and the necessity of ideas – as in the philosophy of science – or the opposition of individuals and the anonymity of superimposed social structures – as in science studies – does not apply to transcendental life. Most contributions in Forget and Weintraub (2008) discuss the methodology of biographies in terms of these dichotomies. The conduct of life, however, gains transcendental rank only in its "contingency", and in its struggle with anonymous structures. Transcendental analysis, as it is so fascinating and challenging of phenomenology, does not have to be carried out as an abstract philosophical discussion of categories, but can be carried out within the concrete reality of scientific practice.

So much for transcendental phenomenology. But why does the writing of the formalist revolution require such a subtle point of view? Is it not just one event before and after other events, preceded in some way, and echoed in another? To start, recall the card I already played at the beginning of the last part: *there is* a factual history of economics only since the formalist revolution; before, the formation of the history of economic thought was an essential part of every economic argument, essential for the very orientation in the discipline. How else was the historical erasure of the formalist revolution possible if not by virtue of its *self-evasive* character? To speak about it in factual terms is to miss the very nature of it.

As seen in terms of factual history, speaking about the formalist revolution is to speak about the *mathematization* of economics along mostly (not only) *general equilibrium theory*. In my preceding historical narrative, I did not refer to the history of mathematical economics. Such history had to include economists like A.N. Isnard (1748-1803), Nicolas-François Canard, (ca 1750-1833), Augustin Cournot (1801-1877), Arsène Dupuit (1804-66), Francis Ysidro Edgeworth (1845-1926), and all the other Walrasians hand in hand with the Paretians who were in dialogue with mathematical scientists like Poincare and Volterra (see e.g. Ingrao and Israel 1990: 31-87, Weintraub 2002: 9ff). In this history, one could even trace a notion of axiomatic mathematization. Cournot showed such interest when distinguishing algebraic representations from arbitrary functions:

> I propose to show in this essay that the solution of the general questions which arise from the theory of wealth, depends essentially no on elementary algebra, but on that branch of analysis which comprises arbitrary functions, which are merely restricted to satisfying certain conditions (1963 [1838]: 4).

Being interested in 'certain conditions' rather than algebraic representations, mathematical economists never relied heavily on referential truth claims. Until the formalist revolution, however, they could not set the tone of the discipline, in which most economists continued to believe in the referential truth of their abstractions.

Mathematization as a feature of theory is thus not the same as formalization as a feature of theoretical practice. Unique to the mathematics applied in the formalist revolution was not its degree of sophistication, but that it was *incontestable*. For this reason only do I call the formalist revolution a revolution. An account of the formalist revolution that assesses it in light of this preceding mathematization too easily loses sight of the accompanying loss of contestability. Formalism, rather than a philosophical program, neutralizes all those instances *through which* economists can respond to their motives. The actual virulence of formalism is not a matter of a feature of theory. Instead, it affects the responsivity of theoretical practices.

With the formalist revolution I can illustrate the phenomenological problem of various modes of formalism that one could distinguish on the level of theory – such as logical formalism of derivations, statistical formalism of error functions, geometric formalism of graphs, computational formalism of algorithms, moral formalism of utilitarianism, formalism of natural laws, of (national) accounting, etc. There are mathematical formalisms again of many kind, while the axiomatic method somewhat stands out not only for its sophistication, but for its *suggestive force* to transmit incontestability (which I describe in a moment). To the extent that the axiomatic method requires remaining inexpressive of meaning, it indeed cannot be called a "method" at all. Along the "axiomatic method" the latent problematic of all formalisms for the economist can be best illustrated. Although in the philosophy of science these formalisms are by far not the same, phenomenologically speaking, the problematic *for the theorist* distils in one tension: Formalism is the title of the difficulties of *taking a stance* or an *attitude* as an economist.

For the sake of clarity, let me compare my approach to the formalist revolution with some other prominent approaches of historians of economics. One of the most versed historians of mathematical economics, Roy Weintraub, warned the economist not to confuse the differences between "mathematization", "formalism", "axiomatization", "abstract", and "rigor" (2002: 72 ff.). He does so in order to disclose the space within which the historically contingent transfers from mathematics to economics (and back again) can be discussed (for the 'back again', see Kjeldsen 2007). Weintraub thus employs historical scrutiny in order to debunk the conflations caused by economists' belief "that mathematics is somehow there, and will always be there in but one shape and form" (2002: 3).

Historical scrutiny, according to my approach, means to account for the formalist revolution in that it *necessitated* this image of mathematics and thus its historical conflations. The little awareness among economists about the historical reality of the rise of mathematical economics is inherent to mathematical expressions as such. A historically and philosophically informed mathematician would soon loose the feeling for mathematics – for which the biography of Weintraub is the best example. For this reason Leibniz, for example, is known as a philosopher rather than as a scientist, and Newton as a scientist rather than as a philosopher.

When committing to historical scrutiny, Weintraub and others thus forgot that inherent to the mathematical experience is to remain inexpressive of its historical or philosophical meaning. It was necessary that economists remained ignorant about the history of mathematical economics if it was to be influential at all. Mathematical economics, as I already have suggested, was influential *because* economics could forget its historical past and philosophical contests. The history of mathematical economics, in particular its success, cannot be understood without considering how effectively it numbed economists' historical and philosophical awareness. In this sense, mathematical economics was manifest of the economists' need for scientificity. A factual history of the mathematization of economics that Weintraub has presented in his yet path-breaking work, possibly *reinforces* the disease it aims to cure.

Historians of economics never internalized that the history of the formalist revolution is a history suppressed by its very events. If there was a formalist revolution, it did not take place – or, less paradoxically, its history of sense is to hide its history of factual events. Even if in the last decades historians have shown a growing interest in the formalist revolution, the question of why this period is both fascinating and frightening at the same time was not seriously tackled. As a result, the most common strategy is to show how little mathematical economics contributed to the questions that moved economics before (Blaug 2003). This nostalgic move goes hand in hand with the rhetoric of most economists today when they ask: What is wrong with the axiomatization of GET? Why is it worthless? Hardly ever does one ask: How could it possibly happen? What made it attractive? What made it necessary? Such an account would also show to historians why the great bunch of their profession are still concerned with what is already "history" and thus officially "irrelevant" for present economics (Marcuzzo 2008).

For the same reason that the debunking of common historical conflations is not the target of my narrative, neither, then, is the exposing of what could be called the non-neutrality of mathematical tools. Historical scrutiny in the formalist revolution is often invested in order to show that its image of neutrality did not correspond with its practice. Mathematics was certainly not "merely a language" that says the same in a different or better way, as Samuelson proudly announced (1947). The informative account, for example, of Giocoli on how mathematical proof techniques affected the notion of equilibrium in economic theory, convincingly shows the non-neutrality of mathematical tools (2003). Yes, mathematical expressions did something to economics. But they did something to economics *as* that which appears beyond economics. If the neutral appearance was part of the motivation of the formalist revolution, then what does it account for to debunk it historically?

The same can be said about exposing hidden intentions surrounding the formalist revolution: What appears being solely for the purpose of mathematical rigor, was actually motivated by specific, mostly politically suspicious, interests in the context of WWII and later the Cold War. Exposing historians take over the role of those who asked: Who Are You – Claiming Scientific Authority! But if the economist, precisely due to the self-evasive character of the formalist revolution, is no longer sensible to this question, then exposing hidden interests appears to be no more than the writing of subplots.

Mirowski's narrative that tells the history of that period as the rise of the new paradigm of "information" is surely the paradigmatic case (2001). Although Mirowski does give an account under which social conditions economic science gained discursive power before and after 1945, in order to do so he had to downplay the impact of the formalist efforts – "Bourbaki would become a charm to ward off cyborgs" (2001: 394). But the neutrality of the formalist efforts, as I have suggested in the preceding part and will confirm in the next, was operational for *how* the new paradigm could sneak into economics, and thus crucial for what difference this new paradigm made *for the economist*. Exposing hidden interests may account for new theoretical paradigms. But it does not account for the meaning of this paradigm for the economist. Here lies the challenge for "another" history of the formalist revolution: it affected what economics possibly could mean for the economist.

When applying historical scrutiny to the formalist revolution we need to keep clearly in mind the rupture it represents in the *historicity* of economics, on the one hand, and its possible philosophical meaning, on the other. A mathematization of economics is not simply one event in the sequence of events that could constitute a tradition. It does not bestow economics with any historical meaning. The mathematization changes not only the relation of economics to its history, but the very possibility of relating to its history. "After" the formalist revolution, the practice of economics excludes the basic questions of history such as whether economics is coined by one and the same question from its beginning until today, or is an scattered field of different world-views. To put it in the words that Husserl reserved as the basic question of meaning: How did I get there? What brought me to it? What made me interested? What am I actually up to? In this sense, the formalist revolution touched at the coordinates of what economics *possibly could mean*.

Toward a Transcendental Phenomenology of the Mathematical Experience: The Rupture and Suggestive Force of Mathematics

My phenomenological discussion of the formalist revolution thus begin with the intuitively intelligible point of the a-historicity of formal expressions. After the two preceding expository parts, the reader should have a notion that "history" is not only a matter of a particular interest, a particular purpose, or even a particular method. A-historicism lies at the very bottom of the constitution of modern science.

The phenomenological problem of formal sciences can be described in terms of *forgetting* – literally, that is, not merely to 'neglect for particular reasons'. What is commonly conceived in the philosophy of science as an *abstraction* or generalization is actually an *oblivion*. While generalizations are only general as long as one retains in the impression of the particular, and is so able to "return" to it, in formalizations one neglects and unlearns to care for the particular, and at last also loses the very sense for it. What lets us forget is of a different phenomenological nature than the reasons that make us abstract from something. What lets us forget are "affective reasons", as Husserl would have said with a smile. Forgetting is being drawn into something without retaining in grasp what led to it. *This* is how economics became a mathematical science.

It is vital for the phenomenological character of the following part that forgetting is inherent in the mathematical experience. Formalism does not refer to a philosophical position. Accordingly, it is possible to actually defend a philosophy of formalism phenomenologically. Hilbert and Husserl went well along in Göttingen. The phenomenological problem of the mathematical experience is that it exerts a *suggestive force*, in particular to equate the intellectual virtue of rigor with the virtues of science. Regarding their affect, the passivity of mathematical cogency and scientific evidence are associated. This is to say that the equation of mathematics with science *was never made by anyone* (certainly not by Hilbert). Instead, it was always a *felt* coinciding. A coinciding (*Deckung*) in Husserl is a passive identification. There is an *affective* alliance of mathematics and science, which fades away as soon as it is defended philosophically.

The suggestive force of formalism says that formalization suffices the needs of science. Only when being in the impression of this force, only within the *experience* of mathematics, could the formalist revolution be successful in shaping the economist's mind: forgetting history as though it was fully represented in presence, and forgetting philosophy, as though all epistemic authority is incorporated in the cogency of a mathematical proof. This is indeed sufficient for a science, the intellectual need of which is merely to appear beyond suspicion. In this sense, the success of the formalist revolution was telling from economists' scientific needs.

While in the philosophy of science one can only cry out the difference of mathematics and science, a phenomenology of the mathematical experience can make understood what affects give rise to this misunderstanding. Only because there is first an affective association between mathematics and science, there is the possible misunderstanding of that mathematics is All There Is about science. Claiming mathematical science is just fine as long as it is accompanied by a philosophical awareness is question begging. It is true, as for example Backhouse argued, that the problem of mathematical economics is not formalism *per se* (1998). But what does it amount to requiring that "economists put sufficient effort into empirical work" (Ibid.: 1856), if it lies in the experience of formalist expressions *per se* not to do so. How come that there is the need to emphasize the preliminary character of mathematics all over again if there was no inherent appeal of the mathematical experience to clink off the rest of our intellectual life? Only for the reason of its suggestive force has the critique of mathematical sciences cried out for centuries: Life Cannot be Reduced to Numbers! And for just the same reason all mathematical scientists have warned, with no softer voice: Never Forget! Mathematics is merely preliminary – precisely as the never-ending warnings of the economic suspicion say: Do not forget, economic life is not All There Is!

Let me quote three of the figureheads in mathematical economics of the first half of the 20th century who accorded to that warning. First, Arthur Pigou, in 1941. He referred to the *ballistics* that mathematical economics cannot provide.

> We must indeed, when engaged in this type of investigation [mathematical] remember always that it is a second-rate affair, prolegomena to economics, not economics itself, not real ballistics. But, if we remember this, we are safeguarded against the danger Marshall feared (Pigou 1941: 278).

The *ballistics* points nicely to my notion of the weight of meaning that will be the actual object of critique in the next part. The success of the mathematization of economics shows that only the prolegomena to economics is capable of providing scientific authority.

Also Paul Samuelson acknowledged in his peculiarly eloquent fashion the suggestive power of mathematics, though he did profit from it greatly:

> The danger is (…) that you will overrate the method's power for good or evil. You may even become the prey of charlatans who say to you what Euler said to Diderot to get him to leave Catherine the Great's court: 'Sir, $(a + bV)/n = x$, hence God exists; reply!' And, like Diderot, you may slink away in shame. Or reacting against the episode, you may disbelieve the next mathematician who later comes along and gives you a true proof of the existence of the Deity (Samuelson 1952: 67).

Another instructive example is John von Neumann, one of the most forceful activists of mathematical economics in the decades surrounding 1945. When speaking in 1947 about the "nature of intellectual efforts in mathematics", he appealed to the empirical origin of mathematical practices though acknowledged their tendency to clink off this origin. He warned of "taking the immovable rigor of mathematics too much for granted (1961 [1947]: 6), and concluded his speech with the following words.

> As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from "reality", it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely *l'art pour l'art.* (…) there is a grave danger (…) that the stream, so far from its source, will separate into a multitude of insignificant branches (…). In other words, at a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration (Ibid.: 9).

We know how von Neumann has saved mathematics from its degeneration in Los Alamos. Therefore, neither could I write the affective history of the formalist revolution following von Neumann. About his affective life you rather consult the archive of the CIA.

Let me clarify my notion of the phenomenology of mathematical experience with reference to Husserl's account of Galileo's "mathematization of nature" (Hua VI: 18-59, E.: 21-59). The a-historicity of mathematics was central for Husserl's image of the sense-history of the scientific revolution. Mathematics, first of all, is a-historical in a trivial sense. Mathematical truths are independent of their "before" and "after". If someone doubts whether the sides of a right triangle have always been or will always relate like $a^2=b^2+c^2$, we have to doubt whether he or she understood it. Euclidean mathematics was "virtually" true even before Euclid formulated it. This virtuality of mathematics gave rise to the Platonic dream that mathematics is reaped off directly from the heavenly tree of eternal ideas rather than designed in light of a particular purpose. Those who hold a more pragmatic image of mathematics, instead, simply do not have the right *feeling* for it.

Hence the history of mathematics is of no interest for the mathematician. The incident of mathematical practice in the world is annihilated by this very practice. Every formulation of a mathematical statement proves that its very formulation does not add something to its validity. Outdated mathematics textbooks – which nevertheless exist – are the first you throw out of your shelf. For such reason mathematics can work as a suggestive image of the progress of science to the extent as progress, too, enables forgetting the problems of yesterday – because they are solved. Hence the rise and fall of modern scientific optimism is nothing but the rise and fall of the belief in a "mathematical science". While during high modernism of science this equation with mathematics prospered and nourished scientific optimism, today mathematics – which historians of mathematics oddly call "modern" – moved to its own department. Since WWII mathematicians deal with *structures* (later with categories), while scientists deal with *objects* (later with models). Mathematicians and scientists have shared the same feeling for most of their modern times. The question of the modernity of science comes down to the question whether they still share this feeling today.

It was this self-annihilating character of the mathematical experience, that fascinated Husserl. It represents one of the benchmarks of his phenomenological philosophy. Husserl, a trained mathematician, taught philosophy between 1901 and 1916 in Göttingen door to door with his colleague David Hilbert. He continued to be occupied with mathematics for his entire life. Mathematics was for him the last hallmark of a phenomenology of science that tries to overcome the positions of, and opposition of, psychologism and objectivism (see exegetically Lohmar 1991). He wanted to exhibit the nature of ideal objects (such as numbers) within an intentional analysis. While in his early years such effort supposedly provided a foundation for the formal sciences, in his later work, which informs the phenomenology at hand, he discussed mathematics along Galileo's hybris of the *mathesis universalis* – that is, the belief that mathematics is All There Is in the natural sciences.

Mathematics fascinated Husserl because the mathematical experience challenges the transcendental status of experience as such. The notion of experience itself was at stake because its constitutive role seems to be annihilated. Precisely as "death" in Heidegger or "the other" in Lévinas, the mathematical experience is of transcendental significance for the very notion of experience. How? In the *Ideas I* Husserl writes:

> There are *pure eidetic sciences*, such as pure logic, pure mathematics, and the pure theories of time, space, motion, and so forth. Throughout, in every step of their thinking, they are pure of all positings of matters of fact; or, equivalently: *in them no experience as experience*, that is, as a consciousness that seizes upon, or posits actuality, factual existence, can assume the function of grounding. Where experience functions in them, it does not *as* experience (Husserl 1988 (Hua III/1): 16).

The mathematical experience does not function as experience. It does not constitute, and is not "present" in, its theories. This is the phenomenological challenge of the "mathematical experience": it seems an oxymoron. Mathematics cannot be constituted as a proper "region" with a typical style of meaning, its own sense-modifications, and sense-sedimentations. Rather, mathematics describes an "emptying of meaning" (*Sinnentleerung*) (Hua VI: 49 ff., E.: 46 ff.).

There is no history of sense, but only a history of the deprivation of sense. The mathematical experience annihilates itself in its being accomplished. It takes place in its own blind spot. The mathematical experience is not manifest in its correlating object of experience, but withdraws from it – as though a feeling was of a different order than the feeling of it. Eric Livingston has put this self-evasive character of mathematics in the center of his anthropology of mathematics: "a proof is cultivated so as to realize the material proof as a disengaged version, or account, of that proof's lived work" (1986: 177). In his (slightly improvised) phenomenology of the mathematical proof, Rota has put the same gap in little harsher words:

> Every mathematical proof is a form of pretending. Nowhere in the sciences does one find as wide a gap as that between the written version of a mathematical result and the discourse that is required in order to understand the same result (Rota 1997: 189).

This self-evasive character of mathematics makes it phenomenologically *risky*. It risks a *rupture* of life in that its experience is absolutely singled out, and does not *demand* to be lived through, so that it could "flow into" (*einströmen*) the stream of consciousness. The mathematical experience does not bestow its object with the weight of meaningfulness. Mathematics interrupts the demand of meaning that makes all experiences lived experiences. "After" a mathematical result is stated, nothing needs to be added: QED. One can enjoy mathematics without being committed to an actual claim, without doing something in epistemic terms – precisely as the trader did not do anything in *oikonomic* terms.

So what do mathematicians? How does mathematical experience bring about a subject of a mathematician? How is an intellectual act experienced if it constitutes only in absence? Or, in terms of my principle notion: To what is the mathematical experience *responsive*? The gap between the mathematical experience and the nature of its correlate – mathematics – challenges the transcendental concern of phenomenology for the sensibility of intellectual life. To bridge this gap, and thus to make understood mathematics from its practices, is the task of a phenomenology of the mathematical experience. It positively exhibits the experiential constitution of mathematics *in* its deprivation. Such I will do in the following part, to write a history of the absent, but nevertheless constitutive experience of mathematics: the history of the *affective traces* of the silence of meaning in mathematics. The phenomenological riddle to be answered is this: being beyond the demand of meaning, mathematics *liberates* intellectual life from sensible life – and yet liberates *to* an experience that is capable of nourishing scientific optimism? How? And at what costs?

Husserl sketched a similar history in his famous supplement to *The Crisis* on the origin of geometry (Hua VI.: 378 ff., E.: 353 ff.). He was interested in the miracle of the institution of geometry (*Urstiftung*), an event that initiated a tradition by means of anticipating what possibly could follow. Geometry could only institute a tradition by hiding itself as an origin. The tradition of geometry is thus the supplement of its validity. This twist of a constitution that constitutes by means of the evasion of its origin also fascinated Derrida when reading Husserl with and against himself (1989). In a nutshell, Derrida showed how Husserl's project of historizing transcendentality remains equally in what he calls the metaphysics of presence. Derrida, like me, was skeptical about digging out an origin of science that could renew the project of European reason. My notion of the history of the affective traces is informed by his critique, but nevertheless does not give up the claim of a phenomenological "exhibition" (*Ausweis*) for the sake of a critical account of modern economic science.

I thus adopt Husserl's problem of the mathematical experience for the case of the mathematization of economics as follows: Since the theoretical experience of the formalization of economics is not expressive of its motives, the history of mathematical economics can only be written within the *trace* it leaves for the character of the mathematician. The experience of mathematical economics does not *enter*, nor does it flow into the intentional correlate of economic theory. Theory does not tell from its sense-history through which it can be understood. The mathematical act remains in itself – that is, in its affective weight before it could bestow its correlate with meaning, and before it could become part of the hermeneutic play of meaning. The mathematization of GET is thus no history made up by *events*, but it is the history that leaves traces, an *affective* history.

There is thus "another", a secret history of the formalist revolution. Only through this history the success of this revolution beyond the theories it informed and beyond the theories that came as an alternative to it can be assessed. With this history I respond to the standard critique of the irrelevance of formalism. I advance this critique by a genealogy of the mathematical experience in economics that circles around a *besetting*, or *infestation*: the scientific motive in economics turns against the economist. The intuition of this infestation is simply that economists become incapable of making sense of themselves, and cannot grow into a fully-fledged self-perception as an economist. In more "therapeutic" terms, I write, as it were, the history of the symptoms of a science that forgot its possible meaning – the life-world.

To sum up these preliminary remarks about the phenomenology of the mathematical experience, the problem is that a *formal theory qua being formal cannot reflect the motivation that gives rise to it.* In other words, theoretical experiences do no longer bring a subject of theorizing into play. If the affective weight of an act does no longer enter the meaning of the objects of experience, there remains an affective history that turns against the object of experience. The subject that is evoked by mathematical economics is not one that conducts an intellectual life, but relates like a parasite to its own work. This estrangement describes the affective constitution of economic science. It starts with the loss of weight of an economic claim, a liberation from the economic suspicion, a liberation from being blamed to be responsible, and ends in the feeling of elevation above messy economic life, which is actually a protective gesture that demands disinterest, resulting in nothing but the loss of the belief in saying something of worth – in cynicism. Such is the genetic code of economic science.

I will tell this affective genealogy in terms of the intellectual biography of one character that appears entirely uninteresting in terms of the history of hidden motives: Gerard Debreu. With Gerard Debreu the phenomenological contradiction of economic science can be exhibited – *with* Debreu, *against* Debreu, as well as by *protecting* Debreu.

Zooming In: Gerard Debreu

Why Gerard Debreu? Among economists he is known as somewhat illusive and double. He cast a "bright shadow" on post-war economics. Debreu is known by all who had to learn his proof of the existence of a competitive equilibrium at graduate school, cursed by all who want to say more than $x \in X$, yet he is crowned with the Nobel Prize for economics. Although economists are trained in Debreu's proof, they hardly read the 1954 article, nor his book of 1959: *Theory of Value* – noteworthy, the last book in economics until today crowned with such title. Some economists may associate Debreu's work on GET with the initiating metaphor of economic science – the invisible hand – although the study of the actual text of Smith is not conceived as part of economic theory. Even if full-heartedly rejected or belittled as outmoded, economists profited from Debreu since he pushed the belief in, and fostered the reputation of economics as an incontestable science. Economics today can be sold for a higher price than before Debreu, although economic battles seem less heated than in the days of, for example, the socialist calculation debate. Last, although the entire effort of Debreu is motivated and becomes intelligible only by his teaching in a *particular* and rather obscure school in mathematics – "Bourbaki" – there are but a few economists who ever heard of that name, and even less who have read it. Debreu did, word for word, and everything he said about his work we find word for word in Bourbaki. Debreu is like an anti-hero of contemporary economics, an icon of its closure.

Debreu may be an icon, but he is certainly *not* a representative of a typical economist. To the contrary, he is an outstanding character in many senses; outstanding for he regarded the period of the formalist revolution as a phase that "had no precedent, and it will have no successor" (1991b: 1); outstanding because he thought of his work as having "freed researchers from the necessity of questioning the work of their predecessors" (1983: 99); outstanding because there is, compared to his work, little interest in his person among economists; outstanding for being often quoted but hardly read; outstanding because he was the only who actually *defended* a methodology that derives all its value from the rigorous distinction of mathematical "form" and economic "content" – thus the only actual formalist in economics; and ultimately outstanding because he is the unique person who received the *Bank of Sweden Prize* for economics without ever perceiving himself as an economist! This is the *scandal* that I will trace in the following part. In the person of Gerard Debreu the phenomenological contradiction of economic science became apparent.

Debreu's illusive status is reflected by the way others have assessed his influence. "Although there had been quantum leaps of mathematical sophistication before in the history of economics, there had never been anything like this" (2002: 114), as Weintraub emphasized his role as compared to mathematical economics between Cournot and Hicks. Hildenbrand called his work "scientific contributions in the most honest way possible", and Samuelson "a unpretentious no-nonsense approach" (in Weintraub 2002: 113). His colleague at Berkeley Oliver Williamson, the ambitious neo-institutionalist said, "I always marveled at Gerard's quiet, kind and inclusive ways – an example being his insistence on referring to me as an 'economic theorist', my protests to the contrary not withstanding" (in Anderson 2005). Debreu was right to insist, since as Varian wrote after the Nobel Prize, "not only have Debreu's works contributed to mathematical economics; they have contributed to the science of economics as a whole" (1984: 4).

Debreu's illusive status is also reflected by the way others have discussed his work. Roy Weintraub, who until today presented the most complete image of Debreu's contribution to the formalist revolution, relied on a distinction of the historian of mathematics Leo Corry between the "body" and the "image" of mathematics (2002). Debreu's influence on the body of economics, from the point of view of today, could be called zero. But it is easy to underestimate his influence on the image of economics including all "secondary" features like method, style, standards, institutions, and professional ethos of economists. The broader the view the greater, yet more subtle is his influence.

Concerning the body of economic theory, on the one hand, Debreu can be identified with the end of GET – the full articulation of the theoretical perception of "the economy". This "theory" has mirrored most analytic advancements in economic theory before Debreu, while after Debreu most theoretical innovations came as alternatives to GET. In this search for alternatives, Debreu became the straw man of orthodox and chiefly heterodox market theorists when speaking about the insignificance of economic theory. Concerning the "image" of economics, on the other hand, I need to include Debreu in the row of those economists who coined the post-war institutions of economics. Did Debreu, in whatever murky fashion, not contribute to the immense growth and social status of the economic profession, its epistemic dominance in other economic discourses, and its participation in the ongoing hype of (Big) science? In short, concerning the body of economics, Debreu's influence can be easily belittled. But concerning the image easily undervalued.

This ambiguity of Debreu's status is my starting point for writing his biography. Only within this tension can the manifest character of his intellectual life for economics as such be exhibited. The master key to this ambiguity, how could it be different, is a *separation*: the separation of *structure* and *meaning* of economic theory. It represents the core of the axiomatic method that Debreu inherited from Nicolas Bourbaki (rather than from David Hilbert). This separation is vital for the suggestive force of mathematics that motivated all of Debreu's writings. Meaning ceases to be a source for the intellectual practices of economic theory. The axiomatic method transforms theory into something that is detached from its motives.

The suggestive force of Debreu's work accounts for the philosophical miracle of the formalist revolution: the identification of mathematics and scientificity. In a broader perspective, it also provides hints for the understanding of the political miracle of the formalist revolution: the changing connotations of economic theory from socialism to neoliberalism. This took place roughly in the two decades of the 1950s and 1960s. I repeat, both riddles can only be solved on an affective level. There was never any economist who actually defended mathematics as *the* philosophy of (neoliberal) economics – certainly not Debreu! In other words, the formalist revolution could only take place *without* a real revolutionist. It happened *to* the profession – as if guided by an invisible hand. During the formalist revolution scientific authority in economics gained an affective monument that is beyond the possible interests that motivate economists. In this sense, Debreu's success shows something of the affective constitution of the scientificity of economics: Economics earned the glory of science by feeling scientific, without anyone ever achieving it!

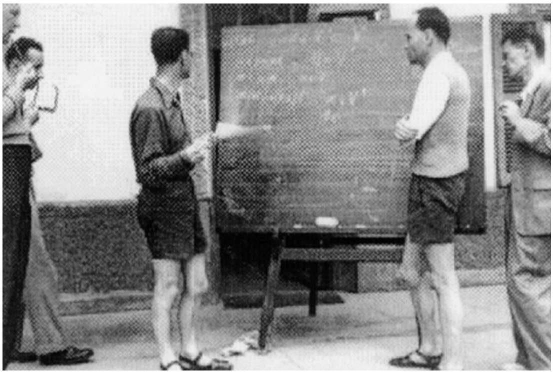
The following phenomenology of the intellectual life of Gerard Debreu is an *affective history* of the axiomatic separation of meaning and structure. Even if meaning and structure are separate, and theoretical acts no longer constitute economic meaning, there are affective traces of this separation. With the life of Debreu I am going to write the history of the phenomenological traces of the formalist revolution. It describes the affective labour involved in making the transition from Debreu's fascination with mathematics, to his struggle in deciding to leave it and enter economics, to the discreet posture that kept him working in economics. It culminates in the festive mood surrounding the Noble Prize in 1983 that meant nothing but a disaster to Debreu. The affective biography of Debreu thus runs from "Nicolas Bourbaki" to "Adam Smith" – from his youth at the *Ecole Normale* in Paris in the last years of WW II, where he was taught by Henri Cartan alias "Nicolas Bourbaki", to 1983 when he received the Bank of Sweden Prize in economics roughly for 'having proven the invisible hand of Adam Smith'.

Allow a last methodological remark in the attempt to connect to those still skeptical about my phenomenological approach. I am not interested in Gerard Debreu's person and life for its own sake. Debreu's intellectual life tells something about economic science as such. How? Do not other economists of the post-war decades tell more about the profession? Why should we be interested in a seemingly uninteresting person such as Debreu? Should I not rather look at the life of other, much more revealing economists of the postwar period who really invested great passions and great ideas into economics, and who tried to make global politics with it? Do not the Arrow-Neumann-Friedmans represent the high culture of the theoretical perception of "the economy"? How could the person who proved the existence of a general equilibrium possibly be important? Is it not irrelevant for this proof to know who made it?

Debreu indeed never made any politics with his proof. He made it precisely for this reason: being beyond politics. After he slipped into economics without really having decided on it, mathematical rigor was a way to avoid economists' high culture that surrounded him in Chicago of the 1950s. If he nevertheless affected this culture it is not because Debreu wanted to deceive the profession. Debreu has always been protective when being made responsible for particular economic claims. Formalism meant to him something he could hide behind. He did not want to make a statement about the scientificity of economics. His success thus happened against his own self-understanding. It happened to him. All meaning that became assigned to Debreu, therefore, does *not* stem from himself, but can count as a disciplinary reflex to his work. While other economists only tell about themselves, the discreet Debreu, who always avoided making an economic claim, shows something *of* economic science.

Nevertheless, I am not interested in debunking the misperceptions of his work that happened in the course of its reception. The experiential contradictions of "economic science" can be exhibited only *in* Debreu's life itself, in that *he* experienced the misunderstanding of "economic science" in an intimidating way. The profession's need for scientificity fell back on Debreu. Debreu has nothing to reveal about himself. But he reveals the existential problem of becoming an economic scientist. I am thus far from criticizing Debreu by showing the dirt under his clean dress shirt. I am rather making a case for protecting Debreu against the offences he had to face.

The biography of Gerard Debreu can be entitled the literary rank of a *transcendental parable* for economists. Παραβολη means to "walk aside", like Debreu. His life is an apologue in order to transmit a moral question, in an indirect, but nevertheless concrete way. Debreu's life consists of precisely the elements of a parable: a moral dilemma, a questionable decision, and the suffering of the consequences of this decision. Debreu's biography is a transcendental parable of the moral end of the ethos of the economic scientist.



Nicolas Bourbaki perhaps at their first summer meeting in Besse-en-Chandesse in July 1935.
Note the short trousers.

Part 3
Biography

Gerard Debreu with his long year friend Edmond Malinvaud
after being informed to receive the "Nobel" for economics. Note the housecoat.

Part 3
Biography
Gerard Debreu from Nicolas Bourbaki to Adam Smith

Gerard Debreu died on New Year's Eve 2004 at the age of 83. Some months later, in March 2005, Chantal Debreu de Soto, the daughter of Gerard Debreu, gave a Memorial talk for her father's colleagues at the University of Berkeley. For her, as a non-academic, it was probably not an easy task. She was speaking from a different perspective than her audience was used to.

> As I prepared the statements I wanted to make to you today I was acutely aware that each of us, myself included, only knew a very small part of the whole, complex, and intensely private man that my father was. So I speak to you now from the perspective of a daughter and what I knew of my father (2005).

The surprising thing about his daughter's speech was not that there was a different Debreu, a "father" Debreu. What was surprising, rather, was that Debreu, the father, was just the same as Debreu the colleague – namely, "private" even in his private life. In the same sense as Debreu kept his lives in family and academia apart, he remained discreet in both of them.

And so the image Chantal Debreu presented of her father was all too familiar to Debreu's colleagues at Berkeley. Debreu was an inconspicuous, introverted, quiet, soft-spoken, and protective person. He set high standards for himself, but never got preachy about them. His daughter described him as pedantic, "orderly, predictable", "aesthetically austere", and a "perfectionist", somewhat like "Phileas Fogg" (de Soto 2005). He liked to travel and liked good food, if only things were in order – like an "economic man".

> He would have calculated every probability beforehand and acted only if he figured the odds as favorable to his goals. Any travel or vacation with my dad were planned like expeditions to the top of Everest. Well ahead of time he had researched every historical and geographical point of interest, had booked the hotel for each night, and planned the days' activities in order to stop at the restaurants of interest along the way (Ibid.).

Debreu's home office seemed to his daughter like the ivory tower of a "mathematical Don Quixote", or of Professor Cosinus and his dog Spheroid, who were the heroes of the bedtime stories he used to read after coming home from Cowles. When she grew up, she found out to her surprise that her father did just the same as that droll Professor Cosinus: filling pages from upper left to bottom right, closing with *QED*. Her father did not at all appear to do things other than this, let alone what other fathers did.

> He seemed either to not have the needs other humans had or he managed never to show that he had those needs. Other people, other fathers got tired, got hungry, got thirsty, had a sweet tooth, sweat when it was hot, shivered when it was cold, got distracted when they worked on a difficult task. Other father's clothes got wrinkled or got food stains on them. Other fathers got sad, discouraged or angry. For the longest time I believed that my father never did any of these things. I believed he slept fully clothed in his dress shirt and the bow tie that was his trademark for so long, with his dress slacks, his leather belt, his watch and his dress socks (Ibid.).

As formal as Debreu's work was, as inconspicuous and discreet was his own person. Chantal Debreu could never be as close to her father as she may have wished to be. And this distance applies not only to his relationship with his environment, but even to his relationship with himself. As an anti-hero of economics, he was hardly the hero of his own life. Life happened to Debreu, as he showed by calling his autobiographical essay for *The American Economist* (1991b), "Random Walk and Life Philosophy". Here are his opening lines:

> As a particle performs a random walk in a high-dimensional space, an observer may discover a subspace in which the projection of its path approximates a straight line. The observer may then be tempted to anthropomorphize the particle, and to believe that it has a 'system which a person forms for the conduct of life' [Debreu quotes the "philosophy" entrance of a 1909 dictionary]. In an inversion of roles, a scientist or humanist who is asked to expound his life philosophy must feel inclined to identi[f]y with that particle if he is aware of the many chance events that shaped his career, and of the inchoate system that he formed for its conduct as it began (1991b: 3).

Thinking about his life, Debreu identifies with a particle in a high-dimensional space. I would like to take this metaphor seriously and relate it to the overall present project.

With the metaphor of a random walk, Debreu brings together three themes that I will consider simultaneously in his biography. First, Debreu shows his skepticism about the possibility of (empirical) sciences. Science tends to overstate as truth what is merely a theoretical projection of what is actually utterly random. Science cannot unravel an actual order of the world – which, in any case, may not exist. Second, this epistemic skepticism applies equally to the imperative of invisible hand theorizing: never "anthropomorphize" the market. This would amount to an imposed order on our individual economic lives which is not inherently ordered. About individuals one cannot say more than about particles, or, better, atoms – as the dominant metaphor for economists' "individualism" goes. Third, he himself identifies with this particle! He could not say any more about his life than what one could say about a random sequence of events. In other words, he did not live up to being the subject of his own life. Debreu did not develop a "narrative identity", and never lived up to a hermeneutic integrity of an author (Eakin 2008). Debreu could not be made responsible for what happened during the course of his life. Using the metaphor of a particle, it is as though Debreu wanted to apologize, to avoid responsibility for what happened to him.

In these three themes the general concerns of the Phenomenology of Economics reappear: formalism, the invisible hand, and the life-world of economists. The general motif that runs through these three themes and through the following biography of Debreu's intellectual life is that of the *suspicion of meaning*. Inadmissible in science, economics, and in the life of the economist is the very weight of meaning: transcendental discreetness.

Around the same time as Debreu's daughter held her memorial speech, I began to go deeper into Debreu's biography, since I understood his unique role in economics as compared to, for example, Kenneth Arrow. In these days I was reading about "Bourbaki". I thus never met Gerard Debreu in person. Tackling his intellectual biography nonetheless, I invite the reader to adopt a perspective that sees his work through his person – as his daughter did – as well as his person through his work – as his colleagues and other economists did.

The evidence I present about the life of Debreu that was rendered "invisible" in his work is based on the account of his daughter, who somehow in his place expressed the tensions of the encounter of a mathematician with economics. Much of the following reconstruction of his life depends on the last words of her speech, in which she speaks about the personal consequences of the Bank of Sweden Prize. In this Prize, the misunderstanding of Debreu's intellectual life culminates. For him the Prize was nothing but a disaster. It is this misunderstanding that reflects and concretizes the misunderstanding of economics as a science. My phenomenological account of the intellectual biography of Gerard Debreu is designed to make sense of the disaster of the Prize of 1983, which came for him and his family like a "thunderbolt out of the blue" (de Soto 2004):

> I don't think any of us in the family at the time recognized it [the Nobel Prize] for the disaster that it was to be. (…) It was from that time onward that I saw my father withdraw from us. He was unwilling for any of us to see him as less than he had been judged in that brief shining moment in Stockholm. He could not live up to the myth that had been created around him. We deprived him and he deprived himself of his humanity, of his right to be flawed (Ibid.).

An intellectual life, at the end of which Debreu "deprived himself of his humanity"? How?

(1) Debreu's Intellectual Initiation: Nicolas Bourbaki

Gerard Debreu was born 1921 in Calais as a son of an industrialist. In his youth he was mainly a good student. He quickly grew up into the rather strict and elitist educational system of France and headed straight toward an academic career. In high school he had already experienced the "austere beauty of mathematics" (1991b: 3). He participated in the *Concours General*, and won. The prize was a trip to French West Africa (Debreu 1983a, Bini and Bruni 1998).

During his time at Grand Ecole (1939-1941), France fell to the German occupation. The students were sent into the so-called Free Zone in order to save them from the tumult of the North. Debreu spent this time in the small town of Ambert. "The isolation of the Ambert novitiate often made it possible to forget that France was at war" (Debreu 1991b: 3). The young Gerard was good in math, interested in geometry, the sciences, and astrophysics in particular. In 1941 he entered *Ecole Normale Superior* in Paris run by the Nazis. He was 20 years old. Here my narrative begins. In the "superheated intellectual atmosphere" of this elitist school, Debreu's intellectual identity gained the shape it would maintain for the rest of his life.

> Entering the Ecole Normale Superior in the fall of 1941 meant another initiation. The three years during which I studied and lived at the Ecole Normale were rich in revelations. Nicolas Bourbaki was beginning to publish his Eléments de Mathématique, and his grandiose plan to reconstruct the entire edifice of mathematics commanded instant and total adhesion. Henri Cartan, who represented him at the Ecole Normale, influenced me as no other faculty member did. The new levels of abstraction and of purity to which the work of Bourbaki was raising mathematics had won a respect that was not to be withdrawn (Debreu 1991b: 3).

The encounter with Henri Cartan alias Bourbaki shaped Debreu's image of mathematics and impressed his intellectual ethos for the years to come, in particular insofar as Bourbaki 'commanded instant and total adhesion'. The influence goes so far that everything one could say about Bourbaki, as Weintraub correctly argued and I will strongly confirm, "applies with equal force to Gerard Debreu" (2002: 113).

Apart from these lines, curiously, Debreu for most parts of his life never promoted or even referred to a particular school of mathematics. He was never an outspoken "Bourbakist" in economics. This silence was crucial for both Debreu's self-image in economics and the creepy influence he had on the discipline. For Debreu there was no declaration of belief in a school to be made. For Debreu Bourbaki *is* mathematics. In his *Theory of Value*, *the* Bourbakian document in economics, he refers to his method merely with "*the* contemporary formalist school of mathematics" (1959: x, *e.a.*). And so the very name of Bourbaki did not enter economists' consciousness as the word "axiomatic" did, although economists were in full impact of Bourbakian values for at least 20 years. Economists, like Debreu, hardly discuss what kind of mathematics they use. With Debreu, economics became mathematized, not bourbakized. For economists, without knowing, mathematics *is* Bourbaki.

All the more is it urgent to ask what of Bourbaki was so fascinating for Debreu. The identification of Bourbaki with mathematics and the anonymity of "Bourbaki" as a collective, we shall see, were vital for the very program of Bourbaki. Even for Bourbaki, Bourbaki *is* mathematics. Although for them this identification meant to stand beyond science, it prepared the affective ground on which mathematics and scientificity could later be associated in economics. Commenting on Bourbaki will disclose these virulent intricacies of being a Bourbakian economist, which would otherwise be disguised by a cloud of sheer mathematical authority. Thus, what kind of image of mathematics did the young Debreu inherit from his teacher Henri Cartan? What kind of *mathematical experience* let him speak of his teacher in such lofty tones? Who was Bourbaki and what is it like Being Bourbaki?

The Liberation of Being Bourbaki – Listening to the Music of Reason

As Debreu cast a bright shadow on economics, so did Nicolas Bourbaki in the history of mathematics. In fact, he did not exist. Charles Denis Sauter Bourbaki (1816-1897), instead, was a French general of Greek ancestry fighting in the French-Prussian War of 1870. The virtual son of the general was used by a group of enthusiastic young French mathematicians as a pseudonym for their no-longer-so-secret society, founded in 1935 and beginning to publish their joint work since 1939, *The Elements of Mathematics* (1968 [1939]).

The founding members came all from *Ecole Normale* in Paris. Some would later become the most influential mathematicians of the 20th century: Jean Dieudonné (the dogmatic spokesman of Bourbaki), Claude Chevalley (anarchist and teacher of Alexander Grothendieck), Szolem Mandelbrojt (the uncle of the Mandelbrot Set fractal), Rene de Possel (who left shortly after and wrote a book on game theory as early as 1936), Jean Delsarte (also celebrated), Andre Weil (perhaps the most well-known founding member, who, after the war, held a chair in Chicago and during the war was fiercely criticized for the apolitical project by his sister and philosopher Simone Weil), and Henri Cartan, Debreu's teacher and oldest member of the collective. Membership could be transferred to succeeding generations. Among later members, other great mathematicians have been Alexander Grothendieck and Samuel Eilenberg who both were involved in the foundation of category theory. While there are still some leftovers of the group at the Ecole Normale today (you can send "him" an e-mail: bourbaki@dma.ens.fr), they were influential in mathematics primarily from the 1940's until the 1960's, the period of Debreu's active intellectual life.

At their beginnings, the group tried to keep the list of members secret. Neither did they give an account of their name, so that "Bourbaki" was soon surrounded by mysticism. We only know from later interviews and the minutes of their meetings about the rather peculiar social

Le général Bourbaki, commandant de la garde impériale, puis de l'armée de l'Est. (Musée de l'armée, Paris).

Charles Denis Sautier Bourbaki and Nicolas Bourbaki

Nicolas Bourbaki was meant to be Nicolas Bourbaki. The collective did not give an account of their name. When in a review of their work it was explained that Bourbaki was a collective, "Bourbaki" replied denouncing the deprivation of "his" right to exist. There are yet several myths surrounding the origin of their name. One that Claude Chevalley later blabbed was the following (in Guedj 1985: 19). As an initiation of first year mathematics students at the *Ecole Normale* a fake famous mathematician delivers a lecture in which the theorems, all wrong, have the names of famous generals. Since Bourbaki was a general of the French German war, other commentators have associated the name with the competition that the group may have perceived with Germany. Germany was ahead in the formal sciences in those days – though the general Bourbaki was not that successful. Furthermore, the Greek ancestry of Bourbaki might be associated with the Euclidean initiation of mathematics that the group wanted to succeed, or even supersede (see also Dieudonné 1970: 134).

There exists a relatively developed body of literature commenting on the history of the group. One finds first-hand information about the beginnings of the group in an interview with Chevalley in Guedj (1985). Dieudonné (1970) presents some accounts of the motivations and social dynamics of the group that may only apply to his perspective. Regarding the proto-meetings of the group in the Parisian milieu see Beaulieu (1993). Standard reference regarding their place in the history of mathematics goes to Leo Corry (1992, 1997). There is also a growing literature on Bourbaki in popular history, such as Aczel (2006). Aubin (1997) provides a fascinating account of how the notion of "structure" in Bourbaki "was in the air" in France during the 1930's and 1940's. In particular, he associates Bourbaki with the structuralism of Saussure and Levi-Strauss, and even with the concept of structure in Althusser (!). He also refers to the so-called "potential literature" of a poetry group called "Oulipo" that wrote Bourbakian poems. Also for Michel Serres, who worked in his youth on Bourbaki, there is a close link of the structuralism of Bourbaki and Saussure (1995: 35).

Bourbaki's influence on the discipline of mathematics was to dissolve the old classification of mathematics (analysis, differential calculus, geometry…), replaced with algebra, order, and topology as *the* basic mathematical structures. They thereby contributed to the separation of mathematics from the sciences. Like Debreu, the group won respect in the U.S. mainly after the war. Its influence lies, like in economics, less in research than in the teaching of mathematics. In research, Bourbakism soon was replaced by category theory, a slightly less hierarchical approach to mathematics. Some of their spirit was taken over by the *Bonner Arbeitstagung*. In other sciences like physics, it lost its appeal soon after the war. For the influence of Bourbaki on mathematics, see Corry 1992, 1997.

Despite the abundant literature on mathematics in economics, there is comparatively little work on Bourbaki in economics – which may be symptomatic of the commentary of economics. While Bourbaki is mentioned by many historians (as in Blaug 2003, Hands 1985, Ingrao and Israel 1990, and Giocoli 2003), there is only one article explicitly dedicated to Bourbaki in economics (Mirowski and Weintraub 1994). Mirowski (2001, 2008) mentions Bourbaki notably as a reference for an alternative narrative of contemporary economics than the one he told in terms of von Neumann.

life of the collective. The reasons why the group had chosen the name "Nicolas Bourbaki" are merely suggestive, but the very fact is programmatic. Just as one rhetorical strategy within the mercantile discourse was to present one's argument with a different name, Bourbaki has chosen a pseudonym in order not to let the search for personal fame defile their work. At least so Dieudonné said about the noble motives of the members (1970). Doing so, as we know, can also hide a base social identity that may conflict with the appearance of their work. Some commentators are quick in denouncing Bourbaki in such fashion: "The myth has the effect of bolstering Bourbaki's scientific authority and hiding arguments among the group" (Aubin 1937. 304). Such arguments took place for example between Rene de Possel and Andre Weil that made Possel leave Bourbaki early after it was founded. Yet for Bourbaki the choice of using a pseudonym was a symbol of their very program. As a first, but ultimately sufficient characterization of Bourbaki, I can say: Bourbaki *is* mathematics to the extent that mathematics does not need an author. Bourbaki is *mathematics without a mathematician.*

Despite or just thanks to the anonymity of the members, being part of the group meant a great deal to their intellectual life. They all showed an uncompromised commitment to the project. The group was held together by very close relationships and a "profound faith" in their mission, as Chevalley commented:

> [S]trong bonds of friendship existed between us, and when the problem of recruiting new members was raised we were all in agreement that this should be as such for their social manner as their mathematical ability (in Guidj 1985: 20).

Their meetings, certainly their annual summer meetings in the countryside at Besse-en-Chandesse have been repeatedly described as very vivacious and even anarchic (Dieudonné 1970, Beaulieau 1998). In order to arrive at an unanimously approved text (which took considerably longer than expected), such bonds were a condition of, and also reinforced by the harsh practice of criticism of the circulating drafts. The anonymity of their work helped to create a social atmosphere that was liberated from the usual norms of (academic) conduct. "We often disagreed, we often had big arguments – but we remained good friends", as Cartan remembered that time (in Beaulieau 1998).

All that, the "process of working out", to use Gadamer's expression, and thus all the "extremely animated arguments" (Dieudonné 1970) that were so important for the members' commitment to the project was not supposed to be present in the publication itself. Neither was the process of arriving at a result, nor any other commentary on style or other heuristics. The program of Bourbaki required a secret life of its members, which, in turn, fascinated them with this program. The anthropologist of mathematics Eric Livingston has made this gap between the appearance and the "embeddedness of mathematics within a surrounding culture" a principle of his writings (1999, 1986). It is the first striking aspect of Bourbaki I too want to take stock of: there was a huge gap between the dense social and affective circumstances under which their work was produced, on the one hand, and the appearance of their work being entirely free of this density, on the other. The most vivid experience of mathematics combined with the most rigid appearance of their work? How come?

The experience that drew the members into Bourbaki was a *liberation from* – first of all a liberation from being an mathematical author. Rather than each single member speaking out in his own name, they could *let mathematics speak for itself.* This absence of an author in mathematics is imperative insofar as one pursues the old Platonic dream of mathematics not being created for particular purposes, but discovered as forms in themselves. That there could be someone who first has to conceive these mathematical forms, or perhaps even has to interpret them, amounts to the same as a failure of their program. Let me quote another mathematician who shared this dream, Bertram Russel. He spoke of mathematics having "a supreme beauty cold and austere (…) without any appeal to our weaker nature (…) yet sublimely pure" (Russel 1981: 49). In this sublime reality of mathematics there is no place for an author who, in all of his or her intellectual weaknesses, has to stand for the work. Mathematics, if it stands at all, has to stand on its own feet. Every reference to an author, as well as any heuristics for the reader, would be an illegitimate anthropomorphism, and ought to be suppressed for the sake of the aloofness of the mathematical experience. A "bible in mathematics", as Chevalley called their writings, has no mundane author (Guedj 1085: 20). Bourbaki is mathematics without author, that is, mathematics that writes itself. The name "Bourbaki" is the formal representation, the placeholder of all possible contexts within which their work could have evolved. In the same sense that Bourbaki left authorship aside, Debreu would later silence his provenance from this mathematical shrine.

Mathematics in this Platonic vision is mathematics not by means of being achieved by someone in particular, but by means of the absence and impossibility of someone who could object. "That mathematicians come together and, in each other's presence, prove theorems, not simply to their own satisfaction, but for all provers for all time, provides the sustaining grounds of mathematical activity" (Livingston 1999: 885). Mathematics hides its social constitution and becomes the primordial manifestation of the force of intellectual compliance. For this reason, mathematics was located in Plato's ontological universe just one level below the ideas – like angels. The passivity of cogency made Bourbaki believe in the sublime reality of mathematics, or, better, "the internal life of mathematics" (Bourbaki 1950: 230). Mathematics is of a higher dignity than the rest of the world. Such is the experiential ground of the master trope of the axiomatic method: separation. To experience the force of intellectual cogency without being forced to a particular position describes the sublimity of following a proof step by step. Let me dwell upon this mathematical experience more thoroughly.

The fascination of Bourbaki lies in their *pathos*, in the literary sense of "suffering". Being Bourbaki is to suffer intellectual necessity. It is to experience intellectual forces before, and free from the evocation of an ethos. As long as ethos refers to the kind of problems one is responsive to, Bourbaki was indeed prior to an ethos. Bourbaki separated pathos from ethos in that their intellectual experience did not determine by any means their relation to an audience. The pathos of cogency is thus not a disposedness, or state of mind (*Befindlichkeit*) in Heidegger's sense, in that it would disclose a world in which one "finds" oneself. The affective force of necessity (rather than the active practices of discourse) was the exclusive source from which Bourbaki's work gained its dignity and which made its members believe in its gravity. This gravity weighs heavily on the biography that I am about to present.

The separation of pathos and ethos make the mathematical experience not of a discursive, but of an *aesthetic* kind – not only in the sense of beauty, but also in Kant's sense of sensibility. This aesthetic character resonates strongly in Dieudonné's description of mathematics as the "music of reason" (an expression that was first used by the English mathematician James Sylvester, 1814-1897). Not that Bourbaki wrote, but they listened fully immersed to the "music of reason". Reason is thus no longer a principle, a faculty, or activity of judgement, but becomes an object of experience. Reason is what the mathematician listens to, rhythm in which he thinks, and thus that which is sound and binding for the mathematician. The mathematical experience, or better: what is fascinating in its result, the proof, can be described in terms of the duration of time following a proof step by step. With this duration, one could depict the *impressive consciousness* of the mathematician that informs Bourbaki's (and Debreu's) belief in their project – a complementary description of the "lived-work of proving" which is the object of Livingston's anthropology of mathematics (1986). Let me thus exploit this metaphor of 'the music of reason' a little further and compare it with Husserl's remarks on listening to a melody.

When Husserl speaks about the constitution of the inner-temporal consciousness, he refers repeatedly to the example of the constitution of the object "melody". A melody, as opposed to a spatial object, is not something that is constituted as the identity, or better, the "coinciding" (*Deckung*) of several varying experiences – that is, in "adumbration" (*Abschattung*). A melody has no sides, no "shadows", around which we could "identify" an object as "the same". A melody is itself a temporal object, and only *in time* comes to a coinciding experience as "this melody". Husserl uses this perception of a temporal object as an example of the intuitable "temporal fringe" of consciousness. He speaks of an "animation of the moment" (Hua. X: 386), which is the "field" of passive synthesis: the tone we hear *is* the tone that follows the other. Without actively remembering all preceding tones at all moments, we still listen to "the melody". We do not hear tone (pause) for tone, as though the tone represented the score, but we hear "tone-for-tone", as though the entire melody were the same as the single tones we hear.

> I perceive a measure, a melody. I perceive it step by step, tone by tone. I hear and perceive continuously. Accordingly, there exists an enduring, temporally extended act of perceiving. *What* do I perceive? The first tone sounds. I hear this tone. But I do not hear merely its quality in a timeless point. The tone *endures* and in the course of its duration swells in intensity in this way or that, and so on. And then the second tone follows. I continue to hear, and now I heat it. The consciousness of the preceding tone is not erased, however. I can surely observe, "see," that I still keep my intention directed towards the first tone while the second is "actually sounding", is "actually" being perceived. And so it continues. (Hua X: 167, E.: 171).

Listening to a melody, we can "see" it tone by tone. There is an affective, or better *hyletic* extension (a retension and protension, as Husserl said) that lets us "identify" a melody. We are able to remember and anticipate a melody, "know" it, as it were, only by means of this hyletic, impressional field. After the last tone has faded away, we continue listening to the melody as we happily go home with an earworm.

A mathematical proof, too, takes time. Both, a piece of music and a proof (as well as a narrative) are objects of experience only as temporal objects of duration. A proof cannot be constituted as an object. Rather, one needs to follow it step by step. One line follows another like its own echo, and yet adds another slightly modified line, like an accord of reason. A mathematical proof, if it manifests something of intellectual life, shows this fundamental

Bourbaki, having lunch, pleased by the sun, free from the burden of meaning

The photograph shows Bourbaki at one of their intensive summer meetings in the countryside on a lunch break. These meetings have not been marked by the silence of proofs that call for "total adhesion". Their meetings have been described as extremely animated, without any formal rules of procedure, harsh criticism, and passionate participation. As Dieudonné (1970) wrote that anyone who attend for the first time would

> ... always come out with the impression that it is a gathering of madmen. They could not imagine how these people, shouting – sometimes three or four at the same time – could ever come up with something intelligent. It is perhaps a mystery but everything calms down in the end.

Youngsters, who did not speak up were not invited again to their meetings. They should contribute as anyone else, since nobody should have been dismissed on personal grounds. Academic politics, at least at the beginning, was never topic of discussion. Mathematics is incorruptible.

Yet this anarchic clamour was held together by the aesthetic appeal of mathematical rigor as described above. We now can understand better the necessary tension between the silence of a proof and the affective set up of the group. The cogency of a proof liberates intellectual experience from the demand of meaning to be explicated and articulated. Speaking figuratively, Bourbaki was the neutralization of the world. Rather than horizon, the world becomes "primitive", like mathematical concepts. One can leap over from here to there, without the effort of actually going through. Michel Serres, who worked in his young years on the difference of Bourbaki and classical mathematics, characterized the axiomatic method in terms of "speed" that he inherited when leaping over from poetry to science and back again:

> Speed is the elegance of thought, which mocks stupidity, heavy and slow. (…) mathematics teaches rapid thought. (…) When you reproach me with 'structure isn't enough; you've got to add all the intermediate steps,' this is not mathematical thought (Serres 1995: 67f.).

With this freedom – as present in Andre Weil's smile – I arrive at the genetically lowest point of my transcendental narrative, at the limit of the phenomenology of intellectual life: the liberation from the *burden of meaning*. The liberation from meaning made possible a new kind of *joy*, a joy beyond any costs of epistemic trade-offs between the general and the particular, beyond compromises between induction and deduction, beyond the ups and down of appearance and concealment. In the moment that structure and meaning are separate (as it describes Bourbaki's programme), and structures are the only object of the intellectual practice, *below* this object, meaning is set free from its need of being articulated.

In other words, now intellectual life merges into that life, by which it otherwise is evoked. In just this moment, the life world (here: the sun shining on Andre Weil's smile) is not the world as I introduced it in the Preliminaries: the world that interrogates, that puts us into question, that we need to stand, and that requires that we ask further – that is, the correlate of intellectual life. Instead, now the life world is the world we *live from*, to use Lévinas notion for our sensual life. What otherwise demands intellectual life is now its nourishment, as Lévinas wrote.

> We live from "good soup", air, light, spectacles, work, ideas, sleep, etc…. These are not objects of representations. We live from them. Nor is what we live from a "means of life", as a pen is a means with respect to the letter it permits us to write – not a goal of life, as communication is the goal of the letter. The things we live from are not tools. (...) They are always in a certain measure – and even the hammer, the needles, and the machines [add: a mathematical proof] – are objects of enjoyment, presenting themselves to taste, already adorned, embellished. Moreover, whereas the recourse to the instrument implies finality and indicates a dependence (…), living from… delineates independence itself, the independence of enjoyment, and of its happiness, which is the original pattern of all independence (1979: 110).

Although speaking of economic life, Lévinas describes as accurately as only he can the joy of Being Bourbaki, the joy of being free from the provenance of intellectual life. The shining sun on Andre Weil's smile is the same sun that let Bourbaki's work shine. Intellectual life returns to the sensual intensity from which it stems.

However, this joy of the mathematical liberation of the burden of meaning needs to remain *secret joy*. When thinking about the pragmatic context of Bourbaki's work, they had to refuse in order to keep this constitutive experience going, as Dieudonné was opposed to any sort of applied mathematics: "This is living mathematics and Bourbaki does not touch living mathematics" (145). Indeed, they would loose their feeling for mathematics. Thus, in the name of non-living mathematics, they could secretly enjoy the most vivid intellectual experience at a moment when it merged with sensible life!

In this ambiguity, the axiomatic separation of meaning and structure is diametrically opposed to phenomenology: While phenomenology is concerned with meaning *before* there is structure – the *hyle* of sense, here: the sun shining on Andre Weil's smile – in mathematics there is content only as reference *after* structure, and experiential hyle only in form of the *secrecy* of a joy. And so, Michel Henry phrased the separation of the reality "of" and *of* science that I interpreted in the photograph above with the following words:

> (The life world) is his (the scientists) *There*, where he lives and where he engages in his occupations, where he takes his meals and his holidays, where he has family and friends. As a living being he experiences his pleasure and his suffer, his worries and ambitions, also if they concern science (…). The scientist is a twofold human being, since on the one hand he asserts that life as the subjective individual life, in short his own life, is nothing, or in any case but an appearance without truth and value. On the other hand does he nevertheless continue living thanks to this life by way of drinking, eating, laughing and singing" (Henry 1994: 206*).

Although intellectual life shows in the mathematical experience its inner most sensibility, this intellectual affection of life is constituted by nothing but the exclusion of this sensibility: mathematics itself speaks. And so science is a "way to sense oneself and to experience oneself that turns against the fact to sense oneself and to experience oneself" (Ibid: 210*). At this point of argument I would like to recall again the lines of Chantal Debreu with which I begun this project.

> He (my father) seemed either to not have the needs other humans had or he managed never to show that he had those needs. Other people, other fathers got tired, got hungry, got thirsty, had a sweet tooth, sweat when it was hot, shivered when it was cold, got distracted when they worked on a difficult task. Other father's clothes got wrinkled or got food stains on them. Other fathers got sad, discouraged or angry. For the longest time I believed that my father never did any of these things. I believed he slept fully clothed in his dress shirt and the bow tie that was his trademark for so long, with his dress slacks, his leather belt, his watch and his dress socks.

temporal constitution of intellectual cogency. Understanding is *following*. Reason, which is elsewhere only an abstract principle of the intellectual efforts to grasp the intricacies of one's problem, is here able to be experienced in its own intensity of conclusiveness. And thus the cogency of a proof can have an appeal similar to a Bach sonata. McCloskey, when speaking of mathematical aesthetics, prefers Mozart (2002: 44), and Arrow prefers Wagner (1992: 50). Debreu, instead, exclusively listened to Bourbaki's music of reason – "the pink radio in the kitchen (…) was turned off as soon as my father got home" (de Soto 2005).

What makes this mathematical experience of a proof so appealing, just as in music, is its finiteness. After a proof and after a piece of music, there is *nothing to add* – silence. When the last tone of a piece passes away, when writing *QED* at the right lower corner of the blackboard, the entire piece of music and the entire proof is condensed into a single moment. The moment of closure. Having followed the proof, there is nothing to add. Mathematicians are thus the only intellectuals who can really go home happily. Others carry their problems with them.

Note, however, that there is a crucial difference between the closure of melodies and of proofs. A proof is conclusive and appealing in that it is made *once and for all*. Melodies can be listened to again and again (at least those of Bach). As Henri Cartan wrote in the obituary of Andre Weil about the initiating moment of Bourbaki:

> One fine day he said to me: 'Now that's enough: Let's meet with some other people to discuss these questions. Let's finalize the answers, and then we will not have to speak of them again.' Thus was born the Bourbaki group" (Cartan 1999a: 634).

The appeal of following mathematical proofs is that they do not have to be proven again. Nothing to add. Once achieved one can *forget* about them by simply assuming them. Although the manifestation of intellectual necessity, a proof thus also reflects the "teleology" of intellectual life, the wish to cut short the efforts to grasp the intricacies of one's problem. The notion of certain judgements comes from this impression of necessity. I do not know whether Bourbaki, or any other mathematicians, write down their proofs several times. Some certainly do, but the pleasure of doing so adds nothing to its validity. A piece of music can become better or worse after several listens. And a proof?

Such closure of mathematics let Bourbaki believe in the *timelessness* of their work. Bourbaki was beyond the rest of mathematics, and thus also *beyond science*. Yet the same affect nourishes the belief in blackboard science: to arrive at conclusions by reasoning only. The secrecy of the experiential constitution of mathematics allows for the conflation of mathematics and science. The closure of the mathematical experience endows it with a typical affective density that in modern science often came to be the material through which scientific *optimism* was expressed. Also in science, so the Galilean dream goes, there should be nothing to add.

Here lies the nub of the mathematical experience, in that it suggests a conflation *with science*, a conflation of the value of mathematical rigour and scientific truth. Both aim at a passivity of being guided: the former, to cut it short, by "validity", the other by "evidence". The decisive question is this: The urge of reality and the urge of cogency – how different do they feel? Affectively, as my description illustrates, there is only a light shadow between being forced to endorse by a valid proof or by scientific evidence.

The great danger of this conflation is the loss of intellectual responsivity. In a responsive science, there is always the possibility of reacting to a result, and adjusting or confirming prior beliefs and expectations. Scientific evidence, just in its responsive character, affects something else. Scientific evidence is never closed as proofs are. When a piece of music closes, and after a proof is made, nothing remains to be said. After Galileo showed the clergy the telescope, however, clamour followed rather than silence. What grants science its gravity is not the same as the gravity of the last tone of a piece of music. A conclusion in science is not a moment of breathing out, but a moment of exhalation, of stammering: What Does This Mean? For most of the history of economic thought, the reactions to invisible hand arguments were of the latter kind. But since Debreu, they are indeed of the former kind.

Bourbaki beyond the Hermeneutic Play of Meaning – Virtual and Symbolic

This description of the mathematical experience serves as the *prima materia* of my narrative from Bourbaki to Smith. The modifications of this affect describe the intellectual biography of Gerard Debreu. It was this fascination with the mathematical experience that led him to speak in high tones of Bourbaki, and later at his Banquet speech of mathematics "satisfy[ing] deep personal intellectual needs" and giving "unsurpassed, addictive intellectual pleasure" (Debreu 1983b). Acknowledging this phenomenological intensity of mathematics as the manifestation of intellectual necessity is not to grant it Platonic integrity. I am also far from presenting a phenomenological underpinning of, say, the analytic-empirical divide. Instead, this aesthetic aspect makes intelligible the problematic encounter of mathematics with the possible responsivity of the scientific experience in economics. This problem, as we now see more clearly, concerns the rupture of the mathematical experience in an intellectual life. It cuts off the experiential pathos from its intellectual ethos.

This loss of ethos was never fully acknowledged by economists when speaking of the aesthetic aspect of mathematics. Robert Aumann, for example, played down the rupture mathematics represents for the economists' ethos by distinguishing expressive and abstract art:

> If one thinks of mathematics as art, then one can think of pure mathematics as abstract art, like a Bach fugue or a Pollock canvas (…); whereas game theory and mathematical economics would be expressive art, like a cubist painting or Tolstoy's War and Peace. We strive to make statements that, while perhaps not falsifiable, do have some universality, do express some insight of a general nature; (…) and at their best, our disciplines do have beauty, simplicity, force and relevance (1985: 42).

Beauty and relevance, so we can learn from Bourbaki, are phenomenologically two experiences between which only a misunderstanding can mediate – namely, precisely that misunderstanding which let mathematical economics and game theory appear to be a matter of War and Peace.

Let me put the fascination of Being Bourbaki into a formula: What drew them into their project was the possibility of being *whole-heartedly engaged and immersed in their project, without being committed to anything in particular*. Bourbaki is the intellectual *liberation from the weight and burden of meaning*, meaning as it loses its bond with a past, to which one had to be responsive, and through which one's efforts could become intelligible to others. There was nobody Bourbaki had to be responsive to or responsible for, nobody who could question or engage in dialogue about their work. Bourbaki was self-speaking, self-contained, self-affected, and untouchable. No discussion. Bourbaki is the liberation from the necessity that epistemic life is manifest in claims, but nevertheless, or just because of that, the possibility of absolute cogency. In Bourbaki's words, mathematics is free from being "freighted with special intuitive references" (Bourbaki 1950: 227) or, as we will read later in Debreu's, without being "marred by a substantial margin of ambiguity" (Debreu 1986: 1266). With Bourbaki, meaning loses its weight and liberates an absolute cogency on its own. Bourbaki is thus beyond any hermeneutical play of meaning, and yet their experience is phenomenologically as dense as the most dignified regions of intellectual life.

This phenomenological conception of Bourbaki can be delineated from a hermeneutical one. To describe what Bourbaki did in hermeneutical terms of the play of meaning between prior preconceptions and a present project is insufficient. The absence of a mathematician in mathematics does not liberate a process of understanding, as some may be reminded to the catch word "death of the author", which says that the meaning of a piece of work is entirely subjected to readings of others. When the Nobel committee later would say, roughly, that 'Debreu proved Smith', such a "death" took place – namely, to erase Bourbakism from Debreu's intellectual work. However, no hermeneutical path whatsoever could lead us from Bourbaki to Smith via an author. The displacement of the author in our case goes further. It implies also a death of the reader, in that Bourbaki's work is independent of its interpretations. Bourbaki's text was not there to be read by someone, and thus beyond the discursive dimensions of the hermeneutical play of meaning. "It seemed very clear that no one was obliged to read Bourbaki (…) [A] bible in mathematics is not like a bible in other subjects. It's a very well-arranged cemetery with a beautiful array of tombstones (Guidj 1985: 20). If there is a history of sense of Bourbaki, it is not a discursive history, but a history of the secret joy that constituted their feeling of community. Bourbaki was held together not by what they wrote, but by how it felt Being Bourbaki.

Since Bourbaki's mathematics is discursively not situated, neither does it result in something discursive. In the absence of an author and a reader, their efforts only can result in a *thing*: a proof that is there once and forever. Such things one also finds in economic textbooks that are full of Marshallian and Hicksean demand functions, Coase theorems, the Edgeworth-boxes, Slutsky-equations, or Tobin's qs, etc. Intellectual achievements turn into facts that are deprived of the context from which they arouse. They are beyond the logic of meaning in a discursive situation. As soon as structure and meaning are separate, the hermeneutical play of meaning is suspended. The problem of economic science, as it is one of the arguments of The Phenomenology of Economics, is not the tension of pre-understanding and epistemic claims in economics, and it is not a matter of playing dirty in the hermeneutic play of meaning. Instead, the problem is the phenomenological tension between the claimed reality and the reality of this claim.

This dissociation of a phenomenological and hermeneutical reading helps me to clarify the label of "structuralism", which is applied to Bourbaki's mathematics as well as in the human sciences to the hermeneutics of structural aprioris (historical, discursive, linguistic etc.). What Bourbaki deals with are not *symbolic* structures but *imaginary* structures, to use a distinction of

Gilles Deleuze. Symbolic structures, just as in the hermeneutic play of meaning, Deleuze argues, determine each other reciprocally.

> Sometimes the origins of structuralism are sought in the area of axiomatics, and it is true that Bourbaki, for example, uses the word "structure". But this use, it seems to me, is in a very different sense, that of the relations between non-specified elements, not even qualitatively specified, whereas in structuralism elements specify each other reciprocally in relations. In this sense, axiomatics would still be imaginary, not symbolic, properly speaking (Deleuze 1998: 265).

Bourbaki is liberated from the symbolic play of meaning. The imaginary object of structure allows for the intellectual experience, after being put to the limits of intellectual life, to show its ancestry in sensible life.

So much for Bourbaki as "mathematics without mathematician". Apart from the pseudonym, a second feature of the intellectual policy of the group is worth mentioning briefly. An age limit of 50 years was imposed on membership. Upon reaching the age of 50, one had to retire from Bourbaki. This was the only formal rule of the group, and until this day there have been no exceptions. Although the members did not present their work as authors, there seems to be a biographical condition of the project in terms of an *intellectual aging*. The Bourbakians were apparently troubled that turning older disqualifies the mathematician. Their project requires a distinction between an early and a late intellectual life. Why did they set such a rule, since it does not, as compared to the pseudonym, seem to be inherent to mathematics proper? A proof cannot turn out to be wrong only because the author ages. Did the Bourbakians have doubts about the sustainability of their reflection, fears of waking up one day in one's age and seeing things differently? The longer one engages in their venture, the higher the risk to become critical of it?

As a pseudonym can be used both to stand beyond the search for personal fame and to hide one's personal interest, this feature could also be interpreted in two ways. The first was supported by Dieudonné, arguing that in order to contribute to Bourbaki one had to be *on the top of the art* of mathematics. The older one is, the less informed one is likely to be about new developments in mathematics.

> (A) man of over 50 can still be a very good and extremely productive mathematician but it is rare for him to adapt to the new ideas, to the ideas of people 25 and 30 years younger than he. Now, an enterprise like Bourbaki seeks to be permanent (Dieudonné 1970: 142).

How come one needed to be up to date for a project that is actually designed to be timeless? Could Bourbaki become out of date? Was there after all a discursive judge of their work?

Yes, Dieudonné supported in his speeches a pragmatic image of Bourbaki. *The Elements* were supposed to be designed for the "working mathematician", which I discuss below. Dieudonné presented their work as being continuously updated to the newest developments in mathematics, mirroring them like an encyclopedia of mathematics. Bourbaki reflects the present state of mathematics in a language encompassing all others. In this image, Bourbaki follows the history of mathematics but does not contribute to it, similar to what textbooks are supposed to do. Bourbaki indeed started out as the attempt to write a new textbook in analysis. It was never finished, however, and Bourbaki never reflected new developments in mathematics. After the rise of category theory, which went beyond the limits of set theory, Bourbaki was simply outdated.

According to the more Platonic image, however, Bourbaki should *not* reflect the developments in mathematics (which one may realize in one's age), but *anticipate* all changes *once and for all*. With an age limit set at 50, Bourbaki ensured that the initial frame of their project was sustainable throughout various generations, without being endangered by the skepticism of older men. This image is supported by the commentator Leo Corry: "Unlike anyone else, Bourbaki actively put forward the view that their conception of mathematics was (...) in fact the ultimate stage in the evolution of mathematics." (Corry 1997: 253). Bourbaki is forever. Not to change one's intellectual orientation is part of the "total adhesion" that Bourbaki requires. Even the slightest change of their project within the process of "working out" amounts to the same as the failure of the entire project. This is the meaning of the intellectual value of *rigour* that represents the heart of the program of Bourbaki, to which I turn now: Never Change Mind!

As I will show in more detail, the pragmatic image of Bourbaki functions as an excuse for the apparent hyperbolic character of their Platonic image of mathematics. It allowed them to maintain the secrecy of the mathematical experience without bothering about the philosophical quarrels surrounding mathematics.

Bourbaki's Program and its Aversions to Philosophy and Science – Their Odd Pragmatism, and, of course, the Ideology of Rigor Including its Victims

The exclusive intellectual value that represented all the density of the mathematical experience in Bourbaki was *rigour*, or better: 'rigour, rigour and nothing but rigour'. This line will become later the "Cowles-song" of the students of Debreu: "We must be rigorous, we must be rigorous/We must fulfil our role/If we hesitate or equivocate/We won't achieve our goal" (quoted in Christ 1994: 34). Also in Bourbaki, the value of rigor "was never attacked in serious discussions" (Mandelbrot 1989: 11). To be rigorously rigorous meant first of all to hide rigorously that there could be intellectual virtues other than rigor. Bourbaki is an attempt at really getting serious about rigor insofar it is exclusive – that is, opposed to intuition and application. Rigor does not mean, as formerly, to "base argumentation on the physical problem situation" (Weintraub 2002: 103). To the contrary, it amounts to an emancipation of a problem situation. Bourbakism is the emancipation of mathematics from all possible guidance by a field of application, and thus from all its *intuitions*. No prior intuition, no "pre-understanding", as the hermeneutist would say, is needed. In other words, mathematics frees itself from philosophy and science.

And so Bourbaki's credo in the introduction to the *Elements of Mathematics* reads as follows: "thus, written in accordance with the axiomatic method and keeping always in view, as it were on the horizon, the possibility of a complete formalization, our series lays claim to perfect rigour" (Bourbaki 1968: 12). This match of the axiomatic method with complete formalization and with the 'claim to perfect rigor' describes the core of the program of Bourbaki. It aimed at nothing but the "solid foundation for the whole of modern mathematics" (Bourbaki 1968: v).

Bourbaki is the mathematical foundation of mathematics. The axiomatic method for Bourbaki is the only method providing such foundation because it is the manifestation of the "profound intelligibility" of mathematics itself (Bourbaki 1950: 223). In the axiomatic method, mathematics speaks for itself. Bourbaki, in other words, mathematized mathematics. They freed mathematics from being an auxiliary element of the sciences, and thus granted it the height of self-referential closure.

Bourbaki's programme is driven by an old dream of mathematics to emancipate itself from the two sources of confusion in mathematics: science and philosophy. While in a non-axiomatic mathematics – say, from Galileo to Gauss – the engine of innovation and development are the problems faced by modern science, an axiomatic mathematics is free of that tie. With Bourbaki's axioms, the different, formerly disconnected branches of mathematics that developed out of different contexts of science (analysis, differential calculus, geometry, etc.) are re-organized in a self-contained body. While in Euclidean geometry the intuition of "space" (side by side, upon and after the other, as Kant emphasized the pre-perceptions of geometry in his transcendental aesthetics), or in Newtonian mechanics the intuition of "mass and energy" (pulling and pressing) was the intuition that guided the formulation of a mathematical framework, Bourbaki's "axioms" are the forms of mathematics itself.

The key to that conception of the axiomatic method was that mathematics was thought of as a hierarchy of *structures* which can all be described in terms of *set theory* – the traditionally preferred axiomatic "language": "inside-outside", "containing-excluding", "identity-difference", "belonging", or, in words of Debreu's first remark about "sets" in his *Theory of Value*, "collecting-spreading", "element-class" (1959: 2). For this reason the advanced textbooks as well as a handful of high-ranked journals in economics are full of $x \in X$. In Bourbaki's oeuvre, the set-theoretical framework is developed in the first volume, *Théorie des ensembles* (1968 [1939]), and all following volumes virtually entail nothing but "$x \in X$" (Vol. II "algebra", Vol. III "topology", Vol. IV "functions of one real variable", Vol. V "topological vector spaces", Vol. VI "integration" – at least so the initial, but never achieved plan). Also in Debreu's *Theory of Value* all chapters entail nothing but the set theoretical framework developed in the first chapter, titled "mathematics". Debreu's second remark on "sets" is thus not surprisingly, "the sets which constitute the universe of discourse must always be explicitly listed at the outset" (1959: 3).

Such starting point supposedly guarantees that the discourse is "virtually self-contained" (Ibid: x). The promise of such self-containment is that the discourse will be self-speaking and self-comprehensible. There should be nothing to add, and no need to explicate. Thus writes Debreu at the beginning of his first chapter: "This chapter presents *all* the mathematical concepts and results, which will be used later (…) Its reading requires, in principle, *no* knowledge of mathematics" (1959: 1). The same line in Bourbaki: "In principle, it requires no particular knowledge of mathematics on the reader's part" (Bourbaki 1968: v). The economic reader of 1959, still not trained in mathematics, uninformed about anything like a mathematical school, must indeed have no question when reading these lines – not because it is self-speaking, but because of the bewilderment of what such a perplexing chapter has to do with economics.

Technically speaking, we know that set theory is haunted by unresolved problems that have been around since Georg Cantor (1845-1918). These flaws have subsequently been superseded by category theory that went beyond the limits of Bourbakian structures. To argue, however, that the problem of axiomatic method in economics could be sorted out by reflecting on the limits of its set-theoretical foundation, as Arnis Vilks for example does (2007), is to ignore the historical paths and phenomenological confusions through which the axiomatic method entered economics. When economists write $x \in X$ at the blackboard, do they in any reasonable sense hold a position of an outmoded philosophy of mathematics?

Set theory aside, Bourbaki perceived mathematics as a hierarchy of *structures*. Within this hierarchy, the first three "mother-structures", as Bourbaki called them, are topology, order, and algebra. Bourbaki wanted to bring order into a mathematics that was diffused in various fields, out of which mathematical problems arouse. They wanted to avoid mathematics "becoming a tower of babel" (Bourbaki 1950: 221), so that mathematics again could speak with one voice. As Mandelbrot explained Bourbaki's "top-down" approach in opposition to more "bottom-up" approaches to mathematics: the "former tend to be built around one key principle or structure (…) the latter tend to organize themselves around a class of problems" (Mandelbrot 1989: 11). Because of this commitment to a top-down approach, a fact that will be important for Debreu's attitude, computational methods that are less rigid in their image of mathematics were excluded from Bourbaki's program. "(A)nything that was purely the result of calculation was not considered by us to be a good proof", as Chevalley said and Debreu believed for his entire life (in Guedj 1985: 22).

Such an image of mathematics fostered Bourbaki's general aversion to science and its methods. It came from the fact that science does not allow for the practice of rigour, or at least not for rigorous rigour and nothing but rigour. Science has to rely on "meaning", let alone reference. The search for justified reason, as might be the scientific motive, develops a *life of its own* as the search for rigour. One can conceive the intellectual value of rigour as a value that stems from science only to the extent that rigour is rooted in an interest in being consistent – that is, *not to forget what one has said before*, thus to be responsive to one's past. Once the urge of evidence as that which we should keep in mind when making claims is experientially equated with the urge of cogency, the search for rigour turns against the scientific interest. What is forgotten after this equation is nothing but the source of the problems of mathematics. Bourbaki is the liberation from being an author, liberation from the burden of meaning, liberation from science, and liberation from what could possibly be problematic and bothersome.

How, then, about Bourbaki's philosophy of mathematics? Their program comes down to a single claim: that "mathematical structures become, properly speaking, the only 'objects' of mathematics" (Bourbaki 1950: 225-6). So what *is* a structure, the philosopher asks? The only meaning of structure I could find in Bourbaki is independence from the meaning of its elements – the "primitives". According to Bourbaki, mathematicians are indeed most mathematicians as long as they are able to successfully avoid the question: What Does That Mean? If I want to comment further on this main programmatic claim and look for some sort of justification or verbal defence of their program, I will not find much. Every attempt to formulate the philosophy of mathematics of Bourbaki is rendered speculative, since there is no such thing as an *outspoken* philosophy of mathematics of Bourbaki. The mathematical experience as described above made them silent about philosophy. All we know stems from secondary material and mainly from two speeches Dieudonné held, unauthorized, under the name of Bourbaki (1949, 1950). In fact, there have been considerable differences regarding the "image" of Bourbaki among its members (for example between Weil and Possel, between Chevalley and Dieudonné, Guedj 1985). It was Dieudonné who stamped the image that some philosophers discuss in their accounts.

Recall what such an expressive philosophy of Bourbaki should have had entailed. It had to react to the preceding discussions about the axiomatic method, which encompasses at least half of the century before Bourbaki, from Gauss, Cantor, Zermelo and Fraenkel, and also the monumental oeuvre of Russel and Whitehead. The most prominent attempt to invest the axiomatic method in the sciences was certainly Hilbert's. The difference between Bourbaki and Hilbert, as Corry has convincingly shown (1997, 2004) and Weintraub, too, considers central for the history of mathematical economics (2002), is of particular importance for my narrative. It frames the field of tension between mathematics and economics in which Debreu moved. It also explains the crucial difference between Debreu and von Neumann, who was once, during his period in Göttingen, a close affiliate of Hilbert and well-informed and moreover responsive to the state of the art of mathematics in the 1930s. The difference is that Hilbert's interest in mathematics was *not* dissociated from his interest in the sciences, but in Bourbaki's case it was. All modern mathematicians since the mid-19th century from Gauss, Poincare, and Hilbert, to von Neumann would have, or indeed had, all difficulties relating to Bourbaki's Platonic verve – although they all contributed to the unhappy separation of the pure and the applied.

Hilbert had an outspoken philosophical interest in the foundational role of mathematics regarding the sciences. For Hilbert, as for the most mathematicians before WWII, mathematics was bound by mathematical science. As opposed to the equation of the axiomatic method with the intellectual virtue of rigour, Hilbert associated various intellectual virtues with axiomatics that do not imply an adversity to science. The requirements for an axiomatic system for Hilbert have been the independence, consistency, completeness, and simplicity of axioms (see Corry 2004: 154 ff). In particular the last, simplicity – how could it make sense in a solely mathematical context, beyond the notion of mathematics as a language? Is this not a virtue that could only apply to the sciences? While in Bourbaki the practice of the axiomatic method is reduced to that of proofs of consistency, Hilbert considered mathematical proofs only a minor part of the axiomatic method.

Hilbert aimed with the axiomatic method at the "deepening of the foundations of the individual scientific disciplines" (quoted in Corry 1997: 262). He held the basic belief that there is a *structural interdependence of a plurality of problems*, which are all expressible in a set of basic axioms. He himself dealt, for example, with a wide range of problems in thermodynamics, probability calculus, kinetic theories of gases, insurance mathematics, electrodynamics, radiation, and even psychophysics. Axioms represent basic structural features of an entire field of research, be they thought of as epistemological categories (implicit definitions or 'atoms of knowledge'), or even basic ontological properties, as economists usually perceive them (agents can actually hold rational preferences). Only regarding such a basic belief in the structural interdependence of scientific problems can I speak of an *interest of expression*. More specifically, only regarding such a basic belief can there be feedbacks from science to mathematics, as Kjeldsen has shown for the case of economics concerning nonlinear programming and the Kuhn-Tucker theorem (2007).

Therefore, only in Hilbert can we speak of an axiomatic *method* as it includes a device for the scientist to do something in particular. Hilbert's axioms should have let scientists see more clearly when lost in a problem. In the moment when things appear too complex, when the scientist is confused by the fuzziness of his object, one may step back *for a second*, and do so as though one deals merely with "chairs", "tables" and "beer-mugs" – a comparison assigned to Hilbert when speaking of the foundations of geometry. For a moment, one takes the connotations and thus emotions out of meaning, and sees the problem regarding its supposed structure. Then one may see which relations can be reduced to each other, and which are crucial for the entire discourse. In the words of the physicist Max Born, who reparaphrased Hilbert's methodological demand: "[S]pecify the assumptions at the beginning of your deliberation, stop for a moment and investigate whether or not these assumptions are partly superfluous or contradict each other" (quoted in Corry 2007). Whether or not there is such a structural interdependency is still open to science to consider *after* the structure has been stated. The difference between Bourbaki and Hilbert is ultimately this role of the *before* and *after* of the axiomatization. Not that Bourbaki contradicts Hilbert explicitly on this image, but the *before* and *after* does not inform their practice. Absorbed by the aesthetic appeal of the mathematical experience, they forgot the before and after of their intellectual efforts.

Seen as a position in the philosophy of mathematics, Hilbert's axiomatic method was opposed to intuitionism as propagated by Hilbert's ex-affiliate Brouwer. The actual challenge, however, for which Bourbaki's philosophy should have given an account, was Gödel's incompleteness results of 1931, saying roughly, that 'the consistency of mathematics cannot be proved mathematically', though it only applied to the axiomatic of arithmetic (see extensively Livingston 1986). Gödel's results indeed triggered a foundational crisis of mathematics in the 1930's that made von Neumann – if I believe Mirowski's account (2001: 118 ff.) – switch from axiomatic to computational mathematics. Von Neumann was one of those who

> revelled in turning logical paradoxes into effective algorithms and computational architectures; and subsequently, computation itself became a metaphor to be extended to fields outside of mathematics proper (Mirowski 2001: 23).

And Bourbaki? Beginning their program in the mid 1930's, did Bourbaki thus have an answer to the foundational crisis in mathematics? No, and it is perhaps *the* distinguishing feature of Bourbaki that they did not. They simply ignored issues of foundations with a great youthful enthusiasm combined with a great deal of philosophical naivety. When addressing the "philosophical systems" of "Plato, of Descartes or of Leibnitz, of arithmetization, or of logistics of the 19th century (…) concerning the relations of mathematics with the twofold universe of the external world and the world of thought", Dieudonné, alias Bourbaki, pretends to be *modest*:

> Our task is a more modest and less extensive one; we shall not undertake to examine the relations of mathematics to reality or to the great categories of thought; we intend to remain within the field of

> mathematics and we shall look for an answer to the question which we have raised [the unity of mathematics], by analysing the procedures of mathematics themselves (Bourbaki 1950: 222).

Eschewing a philosophical notion of mathematics and being ignorant about the present crisis in the foundation of mathematics was even necessary for the founding fathers of Bourbaki. In Dieudonné's words: "(T)he collaborators of Bourbaki were young at the time and doubtless would never have started this job had they been older and better informed" (1970: 136). They would have taken the foundational crisis more seriously. The lack of an entire generation of mathematicians due to WWI may have allowed Bourbaki not to feel committed to respond to the foundational crisis, let alone Hilbert's 23 problems. Bourbaki was in this respect the result more of institutional contingencies than historical necessity of mathematics, as Mandelbrot confirmed: "[T]he main reason why Bourbaki arose was not internal to mathematics but externally motivated by a few brilliant persons and by their responses to various aspects of France after World War I and then after World War II" (1989: 11).

Thus, with a good deal of youthful naivety, Bourbaki could take up the axiomatization of mathematics in the late 1930's without referring to the debates that surrounded axiomatizations in the decade before. Listen to a response of Dieudonnè as the self-appointed spokesman of Bourbaki when addressed about the philosophical foundation of Bourbaki, in particular whether their axioms could correlate with something "real".

> On foundations we believe in the reality of mathematics, but of course when philosophers attack us with their paradoxes we rush to hide behind formalism: 'Mathematics is just a combination of meaningless symbols', and then we bring out Chapters 1) and 2) on set theory (Dieudonné 1970: 145).

We know the famous lines when it came to the point that Bourbaki, alias Dieudonné, had to "bring out" those chapters:

> From the axiomatic point of view, mathematics appears thus as a storehouse of abstract forms – the mathematical structures; and so it happens – without our knowing why – that certain aspects of empirical reality fit themselves into these forms, as if through a kind of preadaptation (1950: 231).

Such philosophical naivety or ignorance is *the* distinguishing characteristic of Bourbakism as compared with other schools in mathematics. Bourbaki could not have exerted a hold as strong as Debreu felt it if the group had been philosophically more aware. Let me anticipate one of the lines in which Debreu shows a similar attitude: "In proving existence one is not trying to make a statement about the real world, one is trying to evaluate the model" (in Feiwel 1987: 243). Mathematics functions thus as a surrogate of philosophical reflection, rather than being informed by it. As Corry summarized this attitude: "Bourbaki did not adopt formalism with full philosophical commitment, but rather as a façade to avoid philosophical difficulties" (Corry, quoted in Weintraub 2002: 112). The conditions to discuss Bourbaki's position within the history of the philosophy of mathematics are not met.

Note that Bourbaki's evasion of foundational issues does not only express a reluctance to deal with the history of mathematics, but has the obscure implication that the basic belief in the worthiness of their project, the "reality of mathematics" (the ontic or epistemic structural interdependence of problems) was indisputable and thus *not at stake* in their work. Bourbaki's program *inherently excludes to reflect upon the belief and motives that give rise to it.* Otherwise they would have had to tackle "the philosopher's paradoxes", and take an explicit position within the philosophy of mathematics, which would have to take place outside the frame of "Chapters 1) and 2) on set theory", and in any case outside the 'universe of discourse explicitly listed at the outset'. Bourbaki was thus held together by a taboo. No discussion.

When Debreu spoke about the "total adhesion" his teacher Henri Cartan demanded, the full impact of this taboo of philosophy is apparent. In this effect Bourbaki can justly be called 'the ideology of rigour', as the historian of science Gorgio Israel observed as early as 1977. Mandelbrot confirmed that behind the virtue of rigour there stood (rather than the altruistic dispense of personal fame) a clear political agenda to exert power in the institutions of mathematics.

> Bourbaki showed extraordinarily wide-reaching concern with political influence across the age groups and across the disciplines. Power to school the children [of which Debreu was one, T.D.], to educate the young to have the 'correct' taste (…) and 'export' of their standards of rigour and taste they do not belong to has done untold harm (1989: 12).

Then, being free from philosophy and the sciences, was there, apart from the strong appeal of the mathematical experience, any other *discursive* warrant for their program? Did Bourbaki at all engage in explicit self-reflective appraisal? How did they make sense of themselves? Until late in the 1950's I hardly find more utterances than what they wrote in the introduction to their *Théorie de Ensemble*. Only after their success had settled in, did Dieudonné begin to present an image of their mathematics. Here I can come back to their odd pragmatic image mentioned above. I read it as an excuse for their philosophical naivety. Their mathematical bible should have been sold as an instructive "handbook" for the *working mathematician* (Dieudonné 1970). How so?

Listen again to Dieudonné's apology that the idea of the "mother structures" was historically not well informed. "I do not say it was an original idea of Bourbaki – there is no question of Bourbaki containing anything original. Bourbaki does not attempt to innovate mathematics" (1970: 138). Dieudonné sells their historical indifference as intellectual moderation. Instead of innovating mathematics, Bourbaki alias Dieudonné believed that their work could be a *tool* for the working mathematician. Responding to the objection that Bourbaki is sterile – which is in fact one of the meanings of the Latin word *rigere* as well as the predicate that defined money in all pre-modern economic writings – Dieudonné distinguishes two notions of tools.

> Bourbaki is accused of sterilized mathematical research. I must say that I completely fail to comprehend this, since Bourbaki has no pretension of being a work stimulating to research. (…) The aim is, I repeat, to provide work tools, not to give stimulating speeches on the open problems of the new mathematics, (…). This is living mathematics and Bourbaki does not touch living mathematics (1970: 144-5).

Mind the difference of tools and stimulation. A tool for Bourbaki is not a tool in the sense of being good for some particular task, designed in light of and somewhat evoking its end, but in the sense of being independent of any end. Do we hear the echo of the difference of the phronetic and the instrumental relation of means and ends that constituted the change from the *oikonomia* to the "the economy"?

From this point of view we can understand better the affect that underlies the fascination of Being Bourbaki: An absolute commitment, on the one hand, combined with an extreme modesty on the other, is the affective underground of what constitutes "instrumental rationality": on one hand extreme modesty, because one deals "merely" with tools, but on the other hand, utmost importance, because one deals with a "universal" tool. Bourbaki's work is of utmost importance for it provides the tool for whatever purpose. But at the same time it can only be belittled since it provides "merely" a tool. In this fashion, Bourbaki's implicit Platonic image of mathematics was supplemented by a truncated pragmatism.

If Bourbaki does not aim at a stimulation of mathematics, was their work then not discouraging for the mathematics profession? According to Mandelbrot, yes: "For Bourbaki, the fields to encourage were few in numbers, and the fields to discourage or suppress were many" (Mandelbrot 1989: 11). How then could Bourbaki sell the advantage of the axiomatic method *as* a method for the applied mathematician? In the moment that Bourbaki refers to the *before* and *after* of the axiomatic method, we see how the mathematician falls out of the picture:

> The 'structures' are tools for the mathematician; as soon as he recognized among the elements, which he is studying, relations which satisfy the axioms of a known type, he has at his disposal *immediately* the entire arsenal of general theorems which belongs to the structures of that type. Previously, on the other hand, he *was obliged to forge for himself* the means of attack on his problems; their power depended on his *personal talents* (Bourbaki 1950: 227, emphasis added).

What is the advantage of Bourbaki according to these lines? If there is a problem in mathematics, one is able to immediately solve it – without forging oneself – thanks to a tool independent of a particular problem. Therefore, *problems lose their character of being bothersome*. As long as a mathematician has a problem, he has not read Bourbaki's bible – which answers all questions by undermining their meaning. Only by means of being bothered, however, could there be a weight of meaning through which a subject of a "working mathematician". Hence – and this is the crucial consequence of this quote – having a universal tool, the mathematician as the subject of mathematics becomes secondary, if not redundant. In this quote, Bourbaki expressed the problem of all formalisms: to make oneself redundant.

Bourbaki alias Dieudonné acknowledges this risk and tries to respond to it. He replies with a strong metaphor of the axiomatic method being the Taylor system of mathematics. The mathematician appears like a Taylor worker who puts elements in sets without knowing any more what the elements actually are. This is scientific management of mathematics.

> One could say that the axiomatic method is nothing but the 'Taylor system' for mathematics. This is however, a very poor analogy; the mathematician does not work like a machine, nor as the workingman on a moving belt; we cannot over-emphasize the fundamental role played in his research by a special intuition* (…)
> *Like all intuitions this one also is frequently wrong" (Bourbaki 1950: 227)

The naivety, arrogance, and ideology of Bourbaki that I have traced in this chapter are all too apparent in this little footnote. Bourbaki recognised the risk they represent for intellectual practices. But they could not actually face the ambiguity of their Platonic-pragmatic image of mathematics – since then, their feeling for mathematics would fade away. The working mathematician of Bourbaki, even if he refused to admit, is a Taylor worker.

Bourbaki was thus an inherently ambiguous project. Their program could not be defended openly. As a result, the gaps between their sublime image of mathematics that was evoked by the mathematical experience, and the actual body of their work grew evermore. Although intuitions of science should not play a role in the choice of which proofs to include in their "encyclopedia", as Cartan admitted, theories "built upon different axiomatic systems have varying degrees of interest" (quoted in Corry 1997: 279). Dieudonné spoke of theories that are treated axiomatically, but that he did not consider important as "axiomatic trash". And he spoke of structures that are artificially constructed as "monster-structures" instead of "mother-structures" (Ibid.). Perhaps Cartan should have told Debreu something of that kind when entering economics with *The Elements* in his bag. But for such warnings there was no place in their "universe of discourse".

Bourbaki did not develop guidance for the working mathematician through which they could be made responsible about the possible side effects of their program on the profession of mathematics. They did not consider what their work does to the working mathematician. "(W)e had absolutely no idea that one day power could become Bourbakised", Chevalley admitted (in Guidij 1985: 20). And so neither did Debreu think about the side effects his work could have on economics. After the Bourbakian teaching, he has not learned to carry out such a reflection. Debreu, like Bourbaki, felt beyond questions of responsibility.

One of the Bourbaki members, Chevalley, felt this ambiguity down to his bones. He later regretted what he had done in his youth, as the following confession shows. Chevalley was asked the question, which was impossible to even consider within the Bourbakian experience:

> *Guedj*: Do you think that one can give birth to such an undertaking without being transformed unavoidably into a tool of power, a tool of the dominant ideology? Isn't there a logic inherent in projects of this type that transforms people who participate in them into 'masters'? You, for example, didn't you try to oppose this deviation?
> *Chevalley*: If I had been sure it would happen, if I had the perspicacity, if I had not been so weak as not to ask myself that question, I think that could have been… (…) I have a sense of remorse at not having tried to point it in a direction that wouldn't lead to power. But I didn't try. (Guedj 1985: 21).

This sense of remorse came from the fact that Chevalley had another life, a political life detached from the life he had with Bourbaki. He thus felt the rupture the mathematical experience represents for intellectual life *in person*. Chevalley was politically active in the anarchist group *Le Ordre Nouveau Libertaire*. Due to the ideology of rigour, he was not able to express his political concerns about the war. Not to be able to express one's political interest in economics is indeed *the* Bourbakian symptom in economics.

> *Guedj*: Politics seem to have been excluded from Bourbaki. How did you live this dichotomy between your political involvement outside, and your almost complete investment in Bourbaki, above all at a time when in Germany the Nazis were beginning to enjoy themselves to their hearts content?
> *Chevalley*: I don't know what to say. It's a mistake. What I wrote in the political arena never satisfied me completely. It was only in Bourbaki that I was truly satisfied in what I wrote (Guedj 1985).

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The ambiguities that will surround Debreu's further intellectual life can be summarized in two words: "Applying Bourbaki" – an oxymoron, as Weintraub said (2002: 103), and as I have explicated in various ways in this chapter. While a Hilbertian axiomatization of economics is at least conceivable, a Bourbakian axiomatization is absurd. Bourbakism was never designed for and even hostile against science. When Debreu "applied Bourbaki" the ambiguity of Being Bourbaki – toggling between an elevated Platonism and a truncated pragmatism, hiding the one behind the other – must in some way become apparent. One thing is already clear at this point: The problem of Debreu's Bourbakism was *not* a particular philosophical belief about the role of mathematics in economics, nor any specific economic belief. It rather represents a riddle: What in economics made the profession sensitive for the Bourbakian experience, in which economists can do nothing but forget their motives of doing economics. How thus, in other words, could Debreu make a career in economics with such mathematical background – a career that even ended in front of the Swedish King?

(2) Debreu's Existential Dilemma, 1943

In 1943, Gerard Debreu entered his last year at Ecole Normale, and Paris entered the last year of the German occupation. Since then he began to feel that the 'grandiose edifice' of Bourbaki was somewhat misplaced. As all students, he could not avoid the question that defines critique according to Husserl – What Am I Up To? Seeing the approaching end of his studies as well as the approaching end of the war, Debreu could no longer see himself as a mathematician. In the turbulences of Paris of 1943-44, just like Chevalley, Debreu would have liked to claim something particular. Bourbaki's liberation from meaning is as fascinating as the commitment to it is frightening – particularly at a time when the forces of meaning were globe-spanning military forces.

Between War and Peace, Debreu in the Nowhere between Mathematics and Economics: How it Happened to Debreu that he Went into Economics.

How could we, and how could Debreu *not* see these years in light of the occupied Paris and the war? The end of the war and the prospect of a new society could have been (and indeed was for other economists) a motive for developing an interest in the social sciences, in particular in the question of what holds society together in the absence of a leader. Was this not the question that more or less openly exerted a strong gravity on all economic claims in the 1950s and 1960s, in the West *and* the East? And to Debreu?

Obviously Debreu was affected by the German occupation. He speaks of the "unique experience (…) of living in a totalitarian state that does not concede any right to its subjects (1991b: 4). Nonetheless, Ecole Normale provided a certain protection against this Paris. It represented another world for the young Debreu. Until D-Day, courses were never interrupted. In the academic year of 1943, though, Debreu risked being caught participating in classes instead of doing forced labor for the German army as a *terrassier* (see Bini and Bruni 1998). "The dark outside world of Paris under German occupation", as he puts the relation of the two worlds, "exerted a strong containing pressure on the microcosm in the rue d'Ulm" (1984a). And here in this pressed microcosm Debreu had to think about what would be next in his life.

Crucial at this point was whether he was able to realize that only a slight move away from Bourbaki would require an intellectual reorientation no less radical than Bourbaki's program itself. Was he able to see that Bourbaki's 'music of reason' did not make him an authority in science? Debreu did face the gap when he read his first French economic textbook full of institutional facts and little "theory". In those days, economic teaching in France was rather historical and institutional in nature. Academic economics, as it has been for centuries in France, was an education for public administrators and thus institutionally specific and not at all oriented at the ethos of the "scientist": "pour pouvoir étudier l'économie il aurait fallu que je fasse du Droit Constitutionnel, du Droit Criminel et que sais-je encore. Je n'avais aucune envie d'étudier ces matières. C'était donc très décourageant pour moi" (in Bini and Bruni 1998).

But then, what? Trying to leave mathematics, Debreu faced a real existential dilemma between his intellectual past and his impulse to move on. He was attracted by claiming something particular, but at the same time repelled by it. That 'economics is not mere mathematics', as the standard critique of economics laments, correlates at this point of Debreu's life with an actual experienced dilemma. Here the telling lines in which Debreu described how it happened that he entered economics:

> (B)y the end of 1942, I began to question whether I was ready for a total commitment to an activity so detached from the real world, and during the following year I explored several alternatives. Economics was one of them. [He considered astrophysics, too, but his teacher, Jewish, had to flee from France, T.D.]. In 1943-44 the teaching of the subject in French universities paid little attention to theory, and the first textbook that I undertook to read reflected this neglect. The distance between the pedestrian approach I was invited to follow, and the ever-higher flight I had been riding for several years looked immense, perhaps irreducible. Reason counseled retreat to a safe source. What kept me on an unreasonable heading? The formless feeling that the intellectual gap could be bridged? The wishful thought that the end of the war was near, and the perception that economists had a contribution to make to the task of reconstruction that would follow? An improbable event brought my search to a close. Maurice Allais, whose *A la Recherché d'une Discipline Economique* had appeared in 1943, sent copies of his book to several class presidents at the Ecole Normale (Debreu 1991b: 3-4).

It is telling that Debreu made his decision to enter economics *in light of*, but also *against* the political concerns he had for the social world around him – as though he could not trust his own perception. On the one hand, Debreu did associate his choice with the political situation of the end of the war: "Things were too chaotic in France and it was then I think that I became serious about economics" (in Weintraub 2002: 137). He could not, however, mobilize this association, since he did not know how to respond to, how to express or translate this political impression in an academic choice. He felt 'detached from the real world' because of the pressing political situation, in which Bourbaki's project seemed inappropriate, to say the least. Given his intellectual values, however, he could experience this uneasiness merely as a "formless feeling", a "wishful thought", a "perception" which could not be taken seriously. Vague impressions or intuitions cannot justify a theoretical interest that could possibly have gratified his intellectual needs. Debreu did not learn from Bourbaki how to reflect on intellectual concerns. Their very existence made him suspicious of being driven by "wishful thought".

What Bourbaki once experienced as an infinite source to insist on rigor – namely the separation of mathematical structure and scientific meaning – now constituted for Debreu an existential dilemma between two kinds of reason: say, mundane and pure reason. The first, mundane reason, lets Debreu dither over a "detachment from the world" and "counseled retreat to safe course." For this reason he went into economics. The other, pure reason, requires "total adhesion", and promises a "grandiose edifice." For this reason he did not enter "pedestrian" economics, about which he did not feel at ease talking about. In light of pure reason, Debreu experienced the pursuit of mundane reason as an "unreasonable heading" or even "wishful thought". In light of mundane reason, the pursuit of pure reason seems an "ever-higher flight" in "irreducibly" upper fields of intellectual life.

The dilemma was that both reasons excluded each other. Debreu was repelled by mathematics, but was not able to enter economics. He wished to overcome the Bourbakian detachment in light of the economic task of reconstructing society, but he could not in light of the economic profession being so "pedestrian". He sensed the need for doing intellectual work of less purity and more substance. Yet, he could not realize nor mobilize this need. He gave up his career as a mathematician with a sense for economic problems, and started his career as an economist with a commitment to mathematics. Between war and peace, Debreu moved between mathematics and economics – neither really here nor there: half-hearted.

In light of the ending war and his Bourbakian training, Debreu could neither go back nor forth. What once for Bourbaki meant the liberation from the burden of meaning now hindered Debreu from mobilizing an impression to an actual decision. Debreu's past fascination with Bourbaki, and his intellectual pathos worked against the possibility of taking the impressions of the ending war and his political concerns seriously – like a self-repulsive affect. His Bourbakian past made him distrust in his own experience, and suspicious of his concerns.

What then did Debreu do? Did he solve the dilemma? As the lines suggest, he did not. Instead, he entered economics by a chance of the encounter with an authority, Maurice Allais. Entering economics was one step in the "random walk" of his life, as he describes his choice as though it merely happened to him. Debreu did not decide to enter economics. He simply did not have the intellectual *ethos* of really facing the dilemma. If ethos refers to the kind of problems one is responsive to, Debreu (and Bourbaki) did not acquire any ethos, as I argued above. The pathos of axioms does not help at a point where the problem consists of a mere impression. Hence the urge to leave mathematics remained *unarticulated* in Debreu's young years. He had simply not learned to do that.

And later in his life? Did he ever reflect on his reasons for entering economics? In his last interview, we hear a slight tone of doubt, perhaps even regret, that he did not face the dilemma straight on. At age of 75 he said:

> Alors j'ai cherché à sortir de là, j'ai passé une agrégation de mathématiques et je me suis placé premier. Donc, j'ai cessé de faire des mathématiques non pas parce que j'en étais incapable mais parce que cela *ne me plaisait pas*. C'est comme ça que j'ai vu la question *à ce moment-là* (in Bini and Bruni 1998).

Not "liking" something is a reason so cheap that it works too easily as an excuse for not having thought about a choice at all, as Debreu seems to admit softly at the end of his life. For at *this* moment he saw it like that, as though he later saw it differently. Debreu shows that he may have felt the failure to ask himself why he wanted to leave mathematics. But his swollish answer shows also that he never really learned how to reflect on his intellectual motives.

Whatever reading one may pursue further on these moments of Debreu's life, one thing becomes clear: As long as Debreu does not face the dilemma of being a mathematical economist, he will always have a reason to hide his Bourbakism, and he will always have a reason not to get too deeply into economic talk – half-hearted. What had to happen, that such half-heartedness could later be celebrated so heartily with the Bank of Sweden Prize?

The Meeting with Maurice Allais and the Time when He was at His Most Economist

The path was set. After D-Day, Debreu served the French army for a year. He was first in officer school in Algeria, and, after May 1945, briefly in Germany. This year at the army, he said, was the only "opportunity" of his life "to experience a life outside the academic cocoon" (1991b: 4). Although he was far away from his Bourbakian monk cell in rue d'ulm, he nevertheless had to prepare in the evenings for exams at Ecole Normale, the "agrégation de mathématiques". In summer 1945 he married, and in the fall he graduated.

Between 1946 and 1948 Debreu then worked in close association with Maurice Allais at the *Centre Nationale de la Recherché Scientifique*, where he read what he called "the classics" – some Marx, and some Keynes, but mainly Hicks' *Value and Capital*, and also Frisch, Pareto, Walras, and also Charles Gide. These two years are noteworthy because they probably were the years when Debreu was at his most economist. In cooperation with Allais, who was not Bourbakian but a scientist of many-layered interests, he partially refrained from the commitment to rigour.

Here he also met his life-long friend Edmond Malinvaud. With him and other colleagues, Debreu came together in a "lunch time group" that was "moved by the spirit of research", as Malinvaud remembered (Kruger 2003: 184). Lunchtime was often extended long in the afternoon. Malinvaud was already by then intellectually differently oriented. He came to economics because he was fascinated by the idea of "intelligent management" (Ibid.: 182). With this group, Debreu read Samuelson's *Foundations* (1961 [1947]), but also Abba Lerner's *Economics of Control* (1944), which carries the spirit of the socialist calculation debate.

At this time Debreu also wrote his first economic article, in French (Debreu 1949). This article, which reads like a summary of Allais' adaptation of Hicks GET, is the least formal article he ever wrote. He shows considerable respect to the interpretive sensibility of economic claims. At the end of the article we find one of the rare cases in which Debreu makes some effort to interpret economic terms. Here, perhaps for the only time of his life, he did what all mathematical economists before him did, but which after him became somewhat redundant. He tried to make the reader aware of a "certain danger" to believe that mathematical endeavour is All There Is about economic theory. Regarding the welfare interpretation of a Pareto-optimum, which was part the crucial question of the socialist calculation debate, he concludes:

> La théorie que nous avons exposée présente, par l'apparente évidence de ses points fondamentaux, un danger certain: elle risqué de faire passer pur un absolu ce qui est éminemment relatif. Et tout d'abord rien n'oblige a admettre qu'une situation ou toutes les satisfactions sont plus grandes que dans une autre lui supérieure. On peut penser, par exemple, que les individus, en général, ne sont par les meilleurs juges de leurs besoins, que la satisfaction économique ne représente qu'un aspect de l'individu, que leur agrégat n'est qu'un aspect de la vie de leur société; on peut même soutenir que le « bonheur » d'un individu est dans une très large mesure indépendant de ce qu'il consomme (Debreu 1949: 614).

While Debreu at that date still warned the economist not to take mathematics too seriously, and thus showed respect for the efforts of economic interpretations, later as we will see, the same interpretive indifference will make him suspicious whether economic interpretations deserve any serious treatment. In his time with Allais, Debreu learned the fundamental economic concepts of GET, but not how to embody them intellectually, not how to carry their weight. Perhaps for this reason the collaboration with Allais was not meant to hold longer (see Bini and Bruni 1998).

(3) Debreu's Discreet Life at Cowles, 1949-1974

In 1949, the 28-year-old Debreu crossed the ocean with a two-year-old daughter and his pregnant wife, Francoise Bled. There he encountered a vastly growing and powerful infrastructure of the U.S. institutions of economic science. At a seminar in Salzburg he acquired a first taste of it when meeting Leontief and Solow, started reading the *Theory of Games* (von Neumann, Morgenstern 1944 – encountering thus the use of Bourbaki-proof use of mathematics, particularly the fix point theorem that made him overlook the anti-Walrasian impulse of this lustrous book), won a Rockefeller fellowship, and then ran into the open arms of Tjalling Koopmans, who had just become the Director of Research at the *Cowles Commission* in Chicago. The Cowles Commission had already won the respect of a leading center for scientific social engineering in and of the U.S. post-war world.

Debreu was welcome because he helped the ex-physician Koopmans to push "Cowles Mark II" – that is, to push from Cowles I with the empiricist motto *Science is Measurement* (recall Hotelling, and also Lange) – to Cowles II with the motto *Theory and Measurement*. Since 1950, Cowles advanced mathematical economics of a rather 'theoretical' type, the label under which Bourbaki now was discussed. Debreu arrived just at the moment when Koopmans made this drastic turn from econometrics to economic theory (Mirowski 2001: 249ff). Debreu recalls:

> When I joined the group in 1950, it seemed to me to be a very theoretical group. In particular, the Cowles Commission monograph on estimation [that is, empirical Cowles I] had by then written and published. But Koopmans himself made a fairly drastic change because in the days this book was developed he was deeply involved in econometrics. But from the time when I knew him, he was never, I believe, working actively on estimation methods, and he had become an economic theorist (…). In those days the Cowles Commission monograph on activity analysis [that is, theoretical Cowles II] was not yet published though it was published I believe shortly after I arrived (in Weintraub 2002: 143).

The conference on *Activity Analysis of Production and Allocation* in June 1949 marks the breakthrough of mathematical economics at Cowles (Koopmans 1951). It marks the beginning of the formalist revolution as well as the proliferation of the Cowles-RAND collaboration. The list of participants is impressive: Mathematicians such as David Gale, Albert Tucker, and Harold Kuhn, economists as Kenneth Arrow, Herbert Simon, Oskar Morgenstern, Paul Samuelson, Nicholas Georgescu-Roegen, and mathematical economists from RAND such as George Brown and from the Air Force such as George Dantzig and Murray Geisler. Only one name was missing, but all contributors referred to him: John von Neumann. In the aftermath of the excitements of this conference, Debreu was welcome.

The details of the events of the years before and after this conference – say, from 1944 to 1954 – are highly intricate. In the last part, I gave some hints at these intricacies. The discontinuities and mutual ways in which mathematics and economics interacted, Weintraub emphasized, are due to the very nature of the early formalist revolution: a split between mathematical structures and economic expressions. When writing Debreu's role in this episode, however, I can easily avoid all institutional ramifications. For Debreu remained discreet in all respects. Since he, moreover, never changed this attitude in the two decades at Cowles, I can pass smoothly through his intellectual life as a mathematical economist. Bourbaki prepared Debreu a convenient life.

Let me lay out two lines of research that met when Debreu arrived at Cowles. The merging of the two historical lines allowed Debreu to be a most rigorous and at the same time a most passive "maker" of the formalist revolution. The first concerns the mitigation of the connotations of mathematical techniques, which I emphasized in the last part. "Activity analysis" was basically another name for linear programming, which in turn was associated with both the efforts put in 'market socialism' as well as the war planning that some participants of the conference had done in the Pentagon. How clean this technique now appeared is apparent when Debreu emphasized George Dantzig's first use of a simplex algorithm without knowing from what context Dantzig's innovations stemmed – namely, from the Air Force quest for efficiency of weaponry (in Feiwel 1987).

The second threat concerns the crowding out of game theory, and its disassociation from topology (see Leonard 1995, Giocoli 2003). The formalist revolution took off in the 1940s with game theory, but only could gain ground in the 1950s in GET. The conference in June 1949 meant the breakthrough of von Neumann's (and Morgenstern's) topological proofs of the minimax theorem. However, this happened in a context beyond the intuitions that initially drove game theory: in the context of allocation.

> What was important about game theory was not necessarily all the verbiage, which Koopmans would have found indigestible in any event, about indeterminacy and strategic problems of information processing, but rather that it provided the paradigm for a rather more sophisticated type of maximization procedure employing a different set of mathematical tools (Mirowski 2001: 255).

The crowding out of game theory and the changed connotations of mathematical techniques were the two conditions under which the junction of rigor and GET could take off at Cowles. And this junction came to be personified by Gerard Debreu. At his arrival, he could not feel much of the spirit of Cowles I, as though the preference for rigor had always been there. Now he felt liberated from the suspicion that lay on the mathematician in the circles of Allais. He recalls:

> Whereas before I was in a group which felt mathematics went too far and points of rigor were not terribly important, at Cowles I came to think, very quickly, that full understanding of a problem required no compromise whatsoever with rigor (in Weintraub 2002: 153).

Chicago was welcoming to Debreu's Bourbakism. He did not have to first bourbakize his director Tjalling Koopmans. Koopmans had acquired his Bourbakian taste from Marshall Stone (1903-1998), chairman of the mathematics department at Chicago in those years. Stone called on Andre Weil for a chair at Chicago in 1947, who then became one of the most famous mathematicians of the immediate post-war years. Andre Weil, again, invited Samuel Eilenberg, then in Michigan, to the Bourbaki shrine in 1949. Andre Weil, moreover, could cultivate Bourbaki in the U.S. on the seedbed of an already established mathematical school that was driven by a Platonic verve similar to the Bourbaki group – the school of *Postulational Analysis* around Robert Moore (1882-1974) and Edward Huntington (1874-1952) (Corry 2004: 172 ff.). Since these years in the 1950s mathematicians grow up with axiomatic rigor rather than the belief in a world of mathematical dignity.

The 1950s were the years when Bourbaki published most frequently and won evermore authority in U.S. mathematics departments. Debreu must have followed these publications. He had been in direct contact with Andre Weil, whom he consulted and thanked in his first paper at Cowles for his help with Eilenberg's account of fixed-point theorems (Debreu 1952). Debreu also wrote jointly with his colleagues next door at the mathematics department, such as Israel Herstein (Debreu and Herstein 1952). He recalls:

> A Chicago il y a un excellent département de mathématiques, très rigoureux, qui a été profondément influencé par l'école de Bourbaki dont l'un des représentants principaux était le grand mathématicien André Weil. (…) En ce qui me concerne, dès que je suis arrivé à Chicago, j'ai senti cette influence. (in Bieri and Bini 1998).

Since Cowles was located independently, the other door to the economics department remained closed for Debreu. There, and also at Cowles itself, he could have met those "economists [who] had a contribution to make to the task of reconstruction" (1991b: 4); there he could have argued with those economists who not only claimed but actually made "the economy". But Debreu entered none of these debates.

> I never attended a meeting of the Department of Economics of which I was not a member. I was left alone to do my work during the five years from 1950 to 1955, a marvellous opportunity that I tried to use fully (in Feiwel 1987: 256).

Left alone, Cowles meant for Debreu another microcosm that provided shelter from the dark outside world of Chicago-school ideologies. For Debreu, arriving at Cowles had the taste of both entering an overwhelmingly powerful institution of economics as well as returning to Bourbaki.

Discreet in all Respects – in the Internal and External Affairs of Cowles, in his Methodology…

How then did Debreu understand himself in his role at Cowles? How did he understand himself as a mathematical (pause) economist? How did he deal with the unresolved tension of "applying Bourbaki"? Did he face it? Could he avoid it? Or did he even adapt his intellectual values? Debreu was evidently not trained in dealing with topics commonly perceived as economic issues as identified in institutional, legal or political terms. He knew from Allais not much more than the fundamental concepts of the Walrasian world, but hardly their historical and institutional meaning as they were discussed by his colleagues at Cowles, and moreover as they entered the political arena in a surprisingly direct but still mysterious way. To say the least, Debreu did not have the intellectual ethos of an economist who could stand the political virulence of the economic claims made.

Debreu thus had something to avoid. He had to avoid being asked about his particular position *in* economics. Debreu was in need of an ethos beyond, prior, preliminary to any economic expression. Let me discuss the *distance* he had to adopt as the *discreteness* of Debreu in economics. It represents the next step in my affective history of the axiomatic method. After the axiomatic separation of structure and meaning first correlated with the fascination of an aesthetic void, then put a spell on the young student Debreu, and then meant a dilemma between mathematical and mundane reason, it now resulted in the *discreetness* of Debreu as an economist. To be a mathematical economist for Debreu meant to be discreet.

Debreu, to begin with, was discreet in his political role at Cowles. He could hardly be called an activist of either Cowles' internal affairs concerning theory versus measurement or its external affairs regarding the explosive political relations Cowles undertook in the Cold War. Before 1983, Debreu never actively pushed or even took a stance regarding the axiomatic method in economics. He simply embodied it. For Debreu there was no need to be explicit, let alone to defend his mathematical taste – just as Bourbaki never propagated a philosophy of mathematics. There were other people who build up the bulwark of mathematical rigor as *the* value of economic science.

Koopmans was ahead of all the others. The Measurement Without Theory (or vice versa) debate Koopmans had with the remaining institutionalists (here, Mitchell) must have still been in the air upon Debreu's arrival (Koopmans 1947). It was perhaps the last echo of the old *Methodenstreit*, in that Koopmans argued for microfounded estimation techniques of demand and supply curves. A decade later, Koopmans wrote the methodological pamphlet that came closest to a formalist position, namely his *Three Essays on the State of Economic Science* (1957). There he defended the "separation (…) of reasoning and recognition of facts" (1957: viii) under the title of "postulational analysis" (which he, rather than from Robbins, took from the mathematical school with the same name). In spite of this separation, he supported a realist image of his method in opposition to Friedman, with supporting reference to both Hutchison and Robbins. Postulates, he emphasized repeatedly, *represent* well-established beliefs with distinct (refutable) reference. As a reason for engaging in methodology, Koopmans quoted Roy Harrod: "My substantial excuse for choosing methodology today is that I feel a strong inner urge to say something" (130).

Debreu, under the wing of Koopmans, did not share this urge by any means. He did not perceive himself to play an active role in cultivating the taste for rigor, although he knew that his mathematical skills were ahead of the others, including Koopmans:

> I do remember that he [Koopmans] was not familiar with the definition of a Banach space, because somebody had used the concept of a Banach space, and he asked for a definition, so I imagine that he was not familiar with infinite dimensional spaces (in Weintraub 2002: 148).

Debreu merely imagined. He apparently did not even talk about his or Koopmans' mathematical backgrounds. How then could he have written Koopmans' essays, even if he may have agreed with them word for word?

Debreu had quite some occasions to take a position as a mathematical economist. An area of tension that Debreu must have been aware of was clearly that between the Cowles Commission and Friedman's economics department. Friedman was the gatekeeper between Cowles and what is known as the Chicago school. Making a methodological statement in this context, however, one could hardly suppress the old political connotations of mathematics that Friedman clearly remembered (Hammond 1993).

But also within Cowles, where one could silence these connotations, the debate was carried on, at least via the platform of the Econometric Society in 1953 (see Mirowski 2001: 394 ff.). Notably, Morgenstern demanded as a condition of membership in the society that one "must have been in one way or another in actual contact with data" (Ibid.: 395). Neumann had certainly applauded but, curiously, was used by Jacob Marschak as a defense shield against Morgenstern. Von Neumann, Marschak argued, had to be excluded from the society if the proposal passed. The confusion of whom stood for what, politically and scientifically, was perfect.

Another open debate was caused by David Novick in the Review of Economics and Statistics in 1954. Samuelson, Tinbergen, Klein, Koopmans, and Solow, among others, participated. It was held on merely philosophical grounds, though the discussion had as a clear backdrop, if I follow Mirowski's account, the question of whether mathematical economics could hold its political promises for RAND (Ibid.). Regarding the philosophical justification, at least, the discussion seemed to have been decided beforehand. Let me merely quote the tone with which Samuelson opened the symposium:

> Editor Harris has given me the fun of acting as Master of Ceremonies for the slugfest set off by David Novick's blast against mathematical economics. Seven economists have replied to Novick and according to my reckoning the score stands: For Novick, 7 epsilons; against him, 8 minus 7 epsilons. (Only Solow refuses to concede even an epsilon). Of course, the scores refer to those who struck at Seymour Harris's lure: one of the two nonrespondents to his invitation might in vehemence have overpowered the seven defenders of mathematics (Samuelson 1954: 359).

Discussion Futile. Mathematical economics, just as Bourbakism, could not be successful after a debate. If it could find inroads in economics at all, then it was because it was *incontestable*. As Bourbaki did not engage actively in disciplinary politics, and was influential just because of this neutral appearance, Debreu could be influential just because he never pushed too hard. He, 'left alone to do his work', kept distance to all these debates. Some things in life need to remain unspoken – namely, those things that we better treat discreetly, be they somewhat elevated or simply embarrassing.

The same holds for Debreu and the external affairs of Cowles, above all in its relationship to RAND, operations research, and all the fictions of scientific cold-war solutions. Debreu, as opposed to his affiliate Kenneth Arrow, never engaged in the actual design of the Cowles research program as a supplier of political ideas. Between Cowles and RAND there stood Bourbaki in the person of the unknowing Debreu, who was ignorant as to what his colleagues did "during the summer at RAND". At least so he asserts later, somewhat ambiguously.

> Some of the mathematical economists I knew spent a significant part of the summer at RAND. I did not do that and that may be due to some extent, but not entirely, because I was not a U.S. citizen, and RAND was doing a number of things for the army. (…) I do not know who from Cowles went to RAND in the summer (in Weintraub 2002: 143/145).

Some went, Debreu knew. But he preferred not ask who went. Who knows what they do there? So better not.

With such an attitude, it is a sheer impossibility to imagine Debreu sitting next to von Neumann in a row with Norbert Wiener and Margaret Mead in the Macy conferences, designing the new man. He would have been deeply embarrassed by the insipidity with which the political clamor defiled the music of reason, if one could hear it there at all. Debreu restrained himself from Cowles' politics, and must have been glad that there was always a mediator between him and the economic profession, like Allais, Koopmans, and so later Arrow. Debreu felt safer in the shadow of economists.

Not only in his role at Cowles, but also as an economist Debreu showed utmost discreetness. He was aloof, careful, and restrained in making economic claims. He may even have felt an obstacle to do so, slightly scared of saying something particular, being associated with a particular position for which he could be held responsible. He would not feel at ease when being addressed about an economic contest, anxious of saying something wrong in light of the omnipresent threat of being blamed for the consequences. Within the high ideological density of the 1950s – Smith Act still held, McCarthyism rose, Truman doctrine was signed – Debreu was not in control of the channels that lead from theoretical to political claims. Even if he still would have liked to say something particular, he perceived an invisible force that too easily leads a theoretical claim against the political respectability of the author. So better not claim too much. As a colleague at Berkeley said later about him: "Although he was friendly and had interesting things to say on just about any subject, he was also rather shy, and extremely protective of his personal thoughts and feelings" (Anderson 2005).

I thus propose to see Debreu's Bourbakism as a convenient way to handle the high ideological pressure that lasted on each theoretical claim in economics in the 1950s. As the last years of the war made Debreu feel like saying more than $x \in X$, the environment of Cowles made him feel rather like staying silent. And what is a more natural way to say nothing in academia than to hide behind the silence of $x \in X$? At Cowles Debreu's Bourbakism was reinforced, since it was an effective means to avoid making economic claims for which he would later be taken responsible. To be rigorous, rigorous, and nothing but rigorous, now meant *not* to be suspicious. Avoiding the question 'What does that mean?' (which defines the good mathematician according to Bourbaki) now meant at least *not* being a bad economist – *whatever* that means.

This protective attitude, however, had another aspect I already touched on above: to show *respect* for economics. In his time at Cowles, Debreu held an implicit belief he never found worth expressing: that the actual work in economics had to be done by others. Debreu had never perceived a mathematical economics without economics around. To the contrary, the strict separation of "mathematical form" and "economic content" for which Debreu came to be known, expresses his methodological discreetness; it says nothing but that it is impossible to substitute economics with mathematical economics. By keeping economics out, Debreu wanted to avoid the misunderstanding that economic reasoning is no longer necessary. This image of his self-understanding is at least suggested by some of his later statements on methodology (see below, chapter 5).

Debreu may have held this belief implicitly. But his first French publication that I cited above was the last time he explicitly warned the economist of the inherent misunderstanding in mathematics. Regarding the crucial question of the economic interpretation of "assumptions", he no longer warned the economist, but "he refused", one of his students said, "to comment on the reasonableness of assumptions, believing that his job was to make the assumptions clear, and it was the reader's job to assess them" (Anderson 2005). Assumptions are something to be made clear, not something to be discussed. This had to be done *by others*. No Discussion.

Even though Debreu did not hold an expressive methodology, he may have had the trust, just as Bourbaki, that he could *help* the economist to be more explicit, precise, concise, simple, and to see contradictions more easily, untangle redundant assumptions, etc. He may never have believed that this sense of consistency could meet the full need of scientificity in economics, yet it certainly should have added something valuable. In other words, the non-scientific attitude of Bourbaki never occurred to Debreu. Stamped by Bourbaki's philosophical naivety, he never considered it worth pointing out the role of economists. Is this attitude not more than plausible since the need for economists in economics should be obvious, at least for someone who was 'left alone to do his work'? As it was not obvious, does this show the success of a philosophy of mathematical economics that Debreu during his active intellectual never openly promoted? Or does it show the susceptibility of economics to the liberation from meaning?

…and, of course, Concerning *Uniqueness* and *Stability* of an Equilibrium. Why Arrow and Debreu Understood Each Other so Easily

Only such an image of Debreu's discreet ethos makes understandable his well-known theoretical stance on economics. Debreu was discreet in that he rigorously excluded those issues others discussed on the basis of his work, mainly the *uniqueness* and *stability* of an equilibrium. Let me quickly recall the theoretical infrastructure of his work between the glorious proof of the *existence* of a general equilibrium (1954) and the devastating proof of the *indeterminacy* of a general equilibrium (1974). What did Debreu do as an economist?

Debreu's 1954 article, "The Existence of an Equilibrium in an Competitive Economy", written jointly with Kenneth Arrow, was worth a Nobel prize for each author, two and three decades later, respectively. The key insight that Arrow and Debreu first had independently from each other (both acknowledging Lionel McKenzie, too) was that a general competitive equilibrium could be described as a fixed-point problem. This "insight" brought together two historical lines of literature that historians have researched substantially (Weintraub 2002, Giocoli 2003, Punzo 1991). The first line goes back to Karl Menger's Vienna colloquium, in particular Karl Schlesinger and Abraham Wald, who were reading Cassel's reformulation of Walras and tackled the logical possibility of equilibrium solutions. Although Wald stayed shortly at Cowles, he showed no further interest in the problem. All three names (Wald, Walras, Cassel) are mentioned on the first page of the article.

The second line goes back to von Neumann's 1928 article on "Gesellschaftspiele" with its minimax theorem (1959 [1928]), as well as his 1932 talk at Princeton, published in 1937 in Menger's *Ergebnisse*, where he used a fixed-point proof in the context of a growth equilibrium model (1968 [1937]) (see Mirowski 2001: 94 ff). The line continues with the mathematician Kakutani, who in 1941 generalized the fixed-point technique in topological terms. Perhaps the most crucial contribution has been Nash's 1950 one-page article, in which he used this topological proof technique for the solution of an n-players game. Debreu was reading precisely these articles during his first time at Cowles, which gave him the feeling that no 'compromise whatsoever' needed to be made regarding rigor. When saying so, he apparently glossed over the underlying economic intuitions regarding strategic behavior that were still so obvious in Neumann's minimax-theorem (see next page).

Note that the only cross point of these two historical lines is von Neumann's talk of 1932, in that it was discussed in Vienna. Von Neumann, ironically, neither pursued further the first "Walrasian" line, nor the line of non-constructive topological fixed points proofs. This has caused an entire chain of historical confusions. For Debreu, instead, it brought back the warm feeling of Bourbakian rigor. Note, furthermore, that in both lines everything that could be sensibly called Walrasian economics – including those economists between Pareto and Hicks who officially advanced GET – did not play any positive role at all. Their absence underlines the fact that the 'modeling of the individual' was by no means a motivation for writing the article. Debreu refers to this Walrasian tradition mostly in relation to his charge that differential analysis is non-rigorous mathematics.

Considering the actual writing of the article, one could roughly associate Arrow's theoretical interest with the first line from Vienna, and Debreu's interest with the second line via topology. In his Nobel lecture, Debreu will later comment with the following words.

> Kenneth Arrow has told in his Nobel lecture about the path that he followed to the point where it joined mine. The route that led me to our collaboration was somewhat different. After having been influenced at the Ecole Normale Superieure in the early forties by the axiomatic approach of N. Bourbaki (…) (Debreu 1984: 88).

The process of writing the article was indeed rather a sharing of labor than a collaboration. Perhaps Arrow and Debreu, who hardly knew one another before 1951, never really discussed their theoretical interest. They did not have many occasions for doing so, as Arrow was traveling around Europe during most of the writing process (see Feiwel 1987: 194 f). There was not much debate, Arrow suggested about the collaboration: "It was a wonderful experience, he was just so brilliant to work with. One of us would say a single word, and the other would just understand immediately" (in Gallagher 2005). No discussion, yes, but immediate understanding? Arrow and Debreu understood immediately because it was difficult to object to Debreu, who did not have a strong position or interest in the Walrasian problem of representing "the economy". Debreu was not taken by the encounter with Wald's article, but with von Neumann's:

> The paper by Wald that gave the first proof of existence in the early 1930s did not happen to be important for me. The work of von Neumann on growth turned out to be much more significant since, in particular, it led to Kakutani's theorem (in Feiwel 1987: 249).

EQUILIBRIUM POINTS IN N-PERSON GAMES

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(…)

* The author is indebted to Dr. David Gale for suggesting the use of Kakutani's theorem to simplify the proof and to the A. E. C. for financial support.

[1] Kakutani, S., *Duke Math. J.*, **8**, 457–459 (1941).

[2] Von Neumann, J., and Morgenstern, O., *The Theory of Games and Economic Behaviour*, Chap. 3, Princeton University Press, Princeton, 1947.

Nash, *Proceedings of the National Academy of Sciences* 36: 48-9

Nash's one-page article has been crucial for both Kenneth Arrow and Gerard Debreu, for their insight of proving the existence of a general equilibrium with the fixed-point theorem. Nash's page marks one of the miracles of the formalist revolution, in that it took off from game theory and settled in general equilibrium theory. More precisely, topological fixed point proofs were first used in order to indirectly prove the minimax theorem in a game, while it was later used beyond strategic behaviour in the context of GET. GET is not descriptive of strategies, learning processes of agents, or similar. GET fostered indirect proofs and the separation of rigour and reference, which game theory only could in part. The historical line I have drawn as that preceding Debreu was a gradual movement into the background of the economic implications of mathematical proofs. Let me dwell a little more on that passage, for it maps the split between "Bourbaki" and "Hilbert" in economics.

Angle point of this passage is the (in)sufficiency of indirect as opposed to constructive proofs for economic theory. In 1928, von Neumann had explicitly pointed to the need for a constructive proof that begins and ends with the intuition of 'minimizing one's losses' (see also section 17.8 of the *Theory of Games*). What von Neumann found appealing in the minimax theorem and his proof should have reflected was the same as what was appealing for Thomas Mun and Nicolas Barbon when beginning with economic theory – going *beyond* economic suspicion: "(…) it makes no difference which of the two players is the better psychologists, the game is so insensitive that the result is always the same" (von Neumann 1959 [1928]: 23).

Why then did later economists lose interest in a constructive (read: economically expressive) proof? The footnotes of John Nash are telling in this respect. Although he does acknowledge *The Theory of Games* as the background of the problem of an N-person game, he does not acknowledge von Neumann as a source of the use of fixed-point proofs. Instead it was the mathematician David Gale, who had just earned his PhD at Princeton, who pointed Nash to the article of Kakutani, who, in turn, had never perceived any use of it for economic purposes. Thus, the two references to von Neumann and Kakutani show how far the separation of mathematical structure and economic problems had already advanced by 1949.

But there was another step to take. Nash did not believe that his article would be influential in *economics*. He presented his proof to mathematicians, and skipped reference to a constructive proof and the economic problem. A constructive proof would involve algorithms relating the theory to data. Such was simply not interesting for the mathematician. Nash in fact had a positive interpretation of the equilibrium, about which Binmore argued characteristically that if the profession had known it, it would not have been successful (Giocoli 2003: 27). Indeed, precisely for this reason was Debreu so inspired by the article. At the time when he was engaged in the existence proof in GET, the genesis of the technique and its economic problems no longer played a role. This is most apparent in his 1959 monograph, in which he skipped all reference to game theory.

The formalist revolution got off the ground from game theory onward, but settled down in GET because the normative and descriptive connotations were too dominant in early game theory. Mathematical rigour could not play out its full affective force as long as it was biased towards the purposes von Neumann has associated with it. Thinking of strategic behaviour did not meet the needs for an aloof ethos in the postwar years (although it did meet the needs of the military in its use of economics).

The 1932 talk of von Neumann was, I just mentioned, the only time von Neumann used a general equilibrium framework. But it was not even that which made Debreu interested in it.

If there was anything that made the 1954 article Walrasian, then it was the problem it left unresolved: the actual *tatonnement*, that is to say, learning-process by which an equilibrium is achieved. In the 1954 article, this appeared as the problem of the virtual subject of the Walrasian auctioneer who needs to set a price system. This problem arose in particular because Arrow and Debreu presented the equilibrium as the generalization of von Neumann's notion of a game that was clearly grounded in an intuition of economic behavior. Arrow was moved by that problem:

> the competitive equilibrium could be described as the equilibrium point of a suitably defined game by adding some artificial players who choose prices and others who choose marginal utilities of the income for the individuals (quoted in Weintraub 2002: 191).

The 'artificial player' infused the entire analytical effort of the 1954 paper, at least for Arrow. He must have felt uneasy about it, for he was still sensitive to the subtle connotation of such an "administrative subject" that could perhaps even replace government. These connotations that refer back to the political battle about the meaning of Walras' model should have been obvious to any economist in the years preceding 1954. Only in relation to the Walrasian claim of representing the "the economy" within a mathematical system, and in relation to the analytical challenges of relating competitive and strategic equilibria with one other, the Arrow-Debreu model was economically contestable (note, *not* because of any assumptions about the individual). And it was this contest that let Arrow continue research in other directions such as social choice theory and issues of uncertainty (Mirowski 2001: 295 ff). In his age, his research brought him to the point of co-organizing the first conference on complexity in Santa Fe. Arrow was exceptional in the history of economics, in that he did not have inhibitions in crossing theoretical boundaries.

For Debreu, instead, the analytic problems of the artificial player sprung from a point beyond his theoretical interest. Theoretically crucial for Debreu when reading von Neumann was not the notion of a game in its relation to an equilibrium, but the encounter of topological proof techniques. Since topology was one of the "mother-structures" of Bourbaki, this disclosed the way to a Bourbaki-proof analysis. Later, in his *Theory of Value* (1959), Debreu no longer referred to the generalization of the notion of a game, nor did he refer to von Neumann as the pioneer of fixed-point proofs in economics. His neo-Walrasian bible – the end of almost a century of economic theories of values – was of no interest for those who actually questioned Walras's model on the grounds of its economic assumptions. It was only of interest for those who were already indoctrinated in the neo-Walrasian community, who rarely cared about other theoretical developments towards game theory and other analytic standards than rigor. In this "neo-Walrasian", or better: Debreuvian community enrolled people like Roy Radner, Abraham Robinson, Hugo Sonnenschein and more clearly the second generation such as Mas-Colell, David Schmeidler, David Kreps, and my teacher Egbert Dierker (see for a sober survey Debreu 1983, 1983c).

Given this context, neither the 1954 paper nor the 1959 book could attract the common interest of economists. No economist may have perceived it as a real advance in GET. The

1954 paper was rather an internal success at Cowles. How difficult it was to get the paper accepted by then, the objection of the first referee shows who rejected the paper because it was not rigorous! (Weintraub 2002: 195 ff.) The existence proof was mentioned in an economic textbook for the first time in 1958 by Henderson and Quandt, and partially presented not before 1971 (Ibid.: 188). Clearly, the paper and the monograph could not strike immediately, since its economic meaning was a matter of the projections of its reception. The recognition that the conditions under which a general equilibrium holds is the discursive benchmark of economic theory, needed at least until the 1970s to settle down in the profession.

The 1954 paper could be said to have reached the profession's interest by means of the two questions it left open. Beyond merely conceiving of a consistent equilibrium, the Walrasian question of the process of how to get there was rendered blank. Two questions remained open: first, the uniqueness, that is, the logical determinability (Is there one equilibrium, or could all states be an equilibrium?), and second stability (Does an equilibrium hold more than one moment at a time, or are we every moment in another equilibrium?). For an economist, the latter two questions are essential regarding the *intuition* of GET. Stability is such a pressing issue because only then does the market "bring about" something, make a difference, and play out in a context in which there are other (political) alternatives to the market. Only then does the market *matter*. The stability question could even be conceived of as *the* East-West question: is the market stabilizing "the economy" and all politics disturbing it, or is the market destabilizing, and only politics brings about proper social order?

But these were yesterday's questions. In the 1960s, people like Herbert Scarf, Kenneth Arrow, Leonid Hurwicz, Lionel McKenzie, Franco Modigliani, and also Herbert Simon were tackling these questions with the same analytical verve as Debreu without disturbing political undertones. Fixed-point techniques were utilized for designing algorithms with which one could calculate a general equilibrium (see e.g. Scarf 1982). This research came to be discussed as "applied" or also "computational" GET (Scarf in Arrow and Intriligator 1982: 1007-63). Rather than success stories of mathematical proofs, however, one can observe decreasing trust in the rigorous treatment of stability and a crumbling of Bourbakian-proof mathematics along the rise of more bottom-up approaches to mathematics. Simulation techniques that picked up estimation techniques of the 1940s again came to fill the theoretical structures. Many hoped that one could operationalize economic theory without losing its rigor simply by "adding on" more structure (see for example the letter of Alain Lewis to Debreu in Mirowski 2001: 432).

Concerning this research that Debreu had prompted, he himself showed utmost discreetness. As Ingrao and Israel emphasized in their classical study, Debreu did not invest any efforts in the question of stability (1990: 329 ff.). He consistently rejected the use of dynamic equations for computational purposes. He only went as far as "regular economies" with which he has shown, similarly to the indirect proof of the existence, that the amount of equilibria is at least finite (Debreu 1970). Others, like Dierker, were inspired and have shown that the amount of equilibria must be an odd number (Dierker 1974, Dierker in Arrow and Intriligator 1982: 79 ff., see also Ingrao and Israel 319 ff).

Most of the contributors to the neo-Walrasian community followed Debreu in ignoring computation. Topics of discussion have been the conditions for uniqueness, for which Dierker laid down the conditions of absolute uniqueness by distinguishing between local and global solutions – topologically, of course (Dierker and Dierker 1972). Radner further scrutinized uncertainty of future markets – a matter of indexing the commodity market (Radner in Arrow and Intriligator 1982: 923 ff.). Another hot topic for Aumann, Hildenbrand, Scarf, and Debreu was the relation of Edgeworth's core and a GE that brought forward measure theory and non-standard analysis. It could hardly be called a contribution to Pareto's original program. More orthodox highlights were, for example, when Mas-Colell presented an existence proof without transitivity and completeness conditions (Mas-Colell 1974). All in all, however, this research proceeded by way of a dialectic between proof and counter proof with the latter finally dominant (Rizvi 2003). If economic meaning entered in this research, then it was only in domination by analytic structures. "The result was a theory that became increasingly abstruse and rarified, such that the average practitioner increasingly became disenchanted with and unable to understand or use general equilibrium theory," as Rizvi commented (Ibid.: 382).

The research of the neo-Walrasian community represents the coma of modern economics. No economist needs to remember it in detail. No economist does not want to go beyond it. The following statement by Debreu pointedly summarizes the ambivalent role of this research in the history of economic theory. Debreu was asked why he remained absent from "dynamic analysis". He replied he had *reservations*.

> I had my own *reservations* about dynamics in spite of the fact that I had studied classical mechanics (…) I thought that the whole question was very facile, and that in economics one did not specify, then test, the dynamic equations that we so easily taken up because of the analogy to classical mechanics. So I was very, always very, suspicious of dynamics and that is a view I have held very consistently (…) I thought about those questions of course *as every economist must*, but it seemed to me that the contributions made were *not important* (in Weintraub 2002: 146, *e.a.*).

The ambiguity of these lines is all too apparent. Debreu discreetly did not participate in discussions about stability, although he thought about them "as an economist must". As an economist, he apparently considered them important, but as a mathematician he could not. As a mathematician he could not because dynamic equations implied computational methods, which as a Bourbakian he could not consider rigorous. We clearly hear in these lines the echo of the dilemma he faced in his early years. Again he is torn between his mathematical values and his interest in making an economic claim. Debreu consistently and rigorously avoided facing the dilemma of "applying Bourbaki".

To explicate the economic theory addressed in the quoted reply is an intricate matter. The reply is dense in the history of economics, in which Debreu intervened. It shows how subtle his influence was on the discipline and how unique his position in the history of economic thought. Nobody else could have replied in that way. The association of 'dynamics' with 'classical mechanics' and the possibility of 'empirical testing' must be surprising for both economists before and after Debreu. Empirical testing, Scarf would say, sure. But why does this require the analogy with *mechanics*?

Neither before Debreu, even at Cowles, nor later even in the research he himself triggered, were issues of dynamics and time in general so consistently rejected. Although nobody before or after Debreu would have given such an answer, the theoretical status of economic time changed rapidly in Debreu's days. In fact, it is perhaps the most common objection against

Debreu's theorizing that markets are only pictured in their end-state, but not regarding the temporal passage that brings about such an end-state.

> What is little understood about the Formalist Revolution of the 1950s is precisely that the process-conception of equilibrium was so effectively buried in that period that what is now called neoclassical orthodoxy, mainstream economics, consist entirely of static end-state equilibrium theorizing with little attention to the stability of equilibrium. (…) That everything depends on everything else is no reason to think that it depends on everything else simultaneously and instantly without the passage of real time (Blaug 2003: 146/154).

The key to this burying of the process-conception of the market is to understand why Debreu associated dynamics with classical mechanics. For those who engaged in dynamic analysis, this association must have been odd. Nevertheless, Debreu justified his reservation with the rejection of that analogy. Debreu speaks here as a Bourbakian against the dependency of mathematics on specific intuitions, particularly physical intuitions. Bourbaki did not permit analogies as a source of meaning in mathematics – above all, *not* from physics.

For most economists before and after the marginal revolution, to say that the market brings about a stable equilibrium was to say that markets necessitate actions as if these actions were subjected to physical laws (note, the "as if" indicates the philosophical disinterest!). Natural necessity was the main reference in order to spell out the metaphor of the "invisible hand". Although the metaphor was, of course, never spelled out entirely – for, as I argued, physical metaphors were only a sufficient, but not a necessary condition of the appearance of epistemic authority – the metaphor was nevertheless important for the belief in the *expressiveness* of economic theory. Physical analogies granted the invisible hand imaginative appeal. Such imagination served both the classical liberals as well as later more socialist-inclined economists. The analogy with "mechanics" suggested that the market really brings about something, really makes things happen. Even if market forces are beyond their historical contingencies, the time of the market mechanism should still be the same time that people are living through: historical time. This belief we still find in later general equilibrium theorists such as in John Hicks' notion of the *week*-equilibrium (1965).

Weintraub states correctly that Debreu's "Bourbaki program marked a definitive break with physical metaphors" (2002: 122). The formation of the neo-Walrasian core of economics during the formalist revolution was entirely free from an intervention of the physical sciences, or anything other than mathematics. Debreu's influence as Bourbakian lays therefore in the fact that one could discuss stability *without* the analogy of mechanics, and, moreover *without* associations with the East-West question. Stability became a theoretical issue that did not touch the expressiveness of the theory. More concretely, if one speaks about "mechanism" today – which is still the dominant concept when referring to the ontological status of the market – economists do no longer *sense* that there is a metaphor that needs to be interpreted.

The influence of Debreu on the profession of economics was thus that the intuitive grounds of market theorizing lost importance. Debreu had a neutralizing effect on economics. And just that lightening of meaning granted Debreu's work its authority. But just that happened contrary to Debreu's own self-understanding as a mathematical economist – discreet and aloof, rather the economist's midwife of cogency than himself an economist. In other words, Debreu had "unintended consequences", beyond his own rather unarticulated, perhaps even respectful, but certainly weak and discreet intentions; he reconfigured the core of the profession as if led by an invisible hand.

Debreu's discreetness, to which this chapter is dedicated, finds its last expression in his excuse for why he did not consider questions of dynamics important, although he thought about them 'as an economist must'? His reply echoes the implicit Platonic vision of mathematics that he should have made explicit in his 20 years in economics. "(W)hen you are out of equilibrium, in economics you cannot assume that every commodity has a unique price because that is already an equilibrium determination" (in Weintraub 2002: 146).

This statement is as unique as it is striking. The concept of disequilibrium is for Debreu a contradiction in itself! If we talk about markets, we talk necessarily always already about an equilibrium, since *in disequilibria prices have no conceivable identity whatsoever*! Equilibrium is thus tantamount to consistency, while consistency is the *condition* of a scientific market theory. Debreu thus does not avoid speaking about disequilibria in opposition to the possible fact that we are most of the time in such a state (as the canon goes today), but because it is beyond what could be consistently said in economic science. The existence of an equilibrium is the condition of the possibility of economic science. Debreu was concerned with the possibility of speaking about the market scientifically, not with a particular economic theory. "In proving existence one is not trying to make a statement about the real world, one is trying to evaluate the model," Debreu said much too late in his life (in Feiwel 1987: 243).

If economists today still have to prove the equilibrium solution of their model and thus show its internal consistency, they are Debreuvian even in a straightforward theoretical sense. One does not need to assume perfect and symmetric information, perfect knowledge, perfect cognitive capacities, or the like in order to be Debreuvian. Debreu never did so! To equate equilibrium with consistency as a condition of scientific theorizing is the point where mathematics and scientificity in economics fall together even on a theoretical level. As long as economics cannot conceive of economic theory without any reference to equilibrium, it is Debreuvian-Bourbakian.

Debreu's Apology of the Indeterminacy of an Equilibrium and Retreat to Astronomy

In accordance with what I said about Debreu's professional life, it is tempting to read the so-called Sonnenschein-Mantel-Debreu result of the early 1970s as a further confirmation of Debreu's discreetness (Sonnenschein 1972, Debreu 1974, Arrow and Hahn 1971: ch. XII). It reads like an *apology* by Debreu, having promised too much since it proved that one really *has nothing to say* with the axiomatic method. Market excess demand functions (with which an equilibrium can be characterized) are structurally undetermined, so that for every price vector and *arbitrary* agent characteristics, there is an economy for which the price vector constitutes an equilibrium price – all terms defined set-theoretically, of course. "[O]bservations on market prices alone do not restrict in any meaningful way the sort of economy that could have generated them" (Rizvi 2006: 231, see also Rizvi 2003: 383 f., for an early discussion see Kirman 1989). There are thus no sufficient conditions on the input level (the individual) that would guarantee a unique equilibrium. Or, as Arrow commented: "In the aggregate, the hypotheses of rational behavior has in general no implications" (in Rizvi 2006: 233). The project of micro-foundation fell. We neither know whether the market is an equilibrating system, as Smith led us to think, or a dis-equilibrating system, as Marxists claim. Debreu's mathematization could not unravel or visualize the invisible hand. What, then, remains to be said for an economist? "(W)e can hardly hope to understand special features [of the economy]", as Dierker comments brightly on these results, "due to the economic nature of the world." (1974: 15) We cannot understand "the economy" because of its economic nature?

Was the result surprising for Debreu? Is the existence proof and the structural indeterminacy proof not the same result reversed? The existence proof showed that one could only prove existence, and this, the result of 1974 showed, is indeed woefully little – at least not enough for an actual economic claim. The structural indeterminacy result can be understood as an *apology* for a misunderstanding. It merely underlined what was already shown implicitly with the existence proof: that the Bourbakian value of rigor does not lead to an economic claim.

There was one neo-Walrasian economist who seemed to know that all along: Frank Hahn. Once a great pusher of neo-Walrasian research, he later said about a theory that answers the big questions of the invisible hand and also suffices for the intellectual needs of this literature, that "in some intrinsic sense such a theory is impossible" (quoted in Ingrao and Israel 1990: 361). I will come back to his famous "ju-jitsu" defense of GET in that it helps in clarifying the misunderstanding this theory itself has caused: "In attempting to answer the question 'Could it be true'?, we learn a good deal about why it might not be true" (Arrow and Hahn 1991: vii).

The Sonnenschein-Mantel-Debreu result potentially could have triggered a foundational crisis in economics comparable with Gödel's theorem. For economists like Werner Hildenbrand it did (Rizvi 2003: 384). But was the rest of the profession of economists in the position to respond to this result adequately? How could this result trigger its deserved historical and methodological reflection back to the time before the axiomatic method? As the profession could not fully understand the article of 1954, it hardly even noticed the results of 1974. It needed historians to show its importance, the first being Ingrao and Israel (1990). Weintraub explains:

> (T)he set of practices had by that late date gathered its own momentum, to such an extent that both Bourbakist and Debreuvian formalism had come to represent a style of mathematical expression long after they had dropped the role of providing philosophical grounding for their respective disciplinary programs (Weintraub 2002: 123).

Thus, again, No Discussion! All the theoretical developments in economics since the early 1970s, I argued above, could indeed be understood as responses to the inner problems of the axiomatized GET. None of them, however, were informed by it. Advances like the so-called applied GET, new econometric methods, and above all game theory – all developments that actually required an absolute split with Bourbaki – were represented as a continuation or even a "rescue" of the same standards (Rizvi 1994). Even if we can observe paradigmatic changes from pure to applied mathematics in economics, from Bourbaki to von Neumann, from axiomatic to computation, from competition to strategy, from theory to models, from deduction to simulation, etc., how can those differences become effective *for the economist* as long as there was never any reflection on the Bourbakian character of the theories they attempt to replace? As long as the official narrative is that game theory 'drops the assumption of complete competition', that behavioral economics 'drops the assumption of complete information' etc., but there still remains a bitter aftertaste of Bourbaki (think of Ken Binmore), to what do these changes amount? I have mentioned already the aesthetic value Aumann emphasized when speaking about 'mathematical economics *and* game theory' as though they were one in the same (1985).

Consider once more the peculiar history of game theory, in particular regarding the neutralization of the non-Walrasian intuitions that drove game theory I (von Neumann and Morgenstern) in game theory II (Aumann, Shubik). According to my narrative, crucial for the historical gap was the philosophical naivety of Bourbaki (as against the internal challenges of axiomatizations), and their preference for topology as rigorous practice. Debreu comments on the miracle that led from game theory I to II, but without referring to himself as one of its agents.

> I have said that the publication of the *Theory of Game and Economic Behaviour* was a symbol of the beginning of a golden age. I must be more precise. I did not mean that the framework and all of the central concepts of game theory had to be taken literally. I meant that there was in the book of von Neumann and Morgenstern a reformulation of economic theory, that new mathematical tools, in particular convex analysis, were introduced (…). The influence of their work has been great, but in many cases it has been indirect and was felt in ways that were unanticipated by the two authors (in Feiwel 1987: 252).

…unanticipated because the non-Walrasian impetus of game theory got lost during Debreu's Bourbakian intermezzo. The foundational problems of game theory, not surprisingly, are not of a very different kind than those of Debreu, as Rizvi (1994) has shown thoroughly. Think, for example, of the problem of multiple equilibria. The point of Debreu's neutralizing effect is this: If someone who has no training in economics shares with a game theorist the intuition that markets are about interdependent, strategic behavior, perhaps combined with an intuition of power, the conversation with the game theorist will likely fail because the game theorist does not seem to be very expressive of that intuition. Because of Debreu, theoretical intuitions (from strategic behavior to asymmetric information) have less of an effect on the contests of economics, and thus the *ethos* of economists. Economics is not more scientific because it considers things like incomplete information, but it considers incomplete information *because* it is *already* scientific. If economists speak about the individual today, then, because there is no longer any risk to do so.

While game theory seemingly brings economics closer to the people, the second post-Walrasian development worth mentioning is the revival of econometrics that allegedly brought economics closer to the world. About econometrics that merges presently with complexity theory a very similar argument could be made. With complexity theory, if I follow the suggestion of Mirowski (2001, forthcoming), we face an even longer delay in von Neumann's vision of the practice of science. Thus, did Debreu's Bourbakism 'ward off cyborgs' until today?

> While neoclassical economists seemed to enjoy a warm glow from their existence proofs, cyborg scientists needed to get out and *calculate*. Subsequent generations of economists seemed unable to appreciate the theory of computation as a liberating doctrine (…) (T)he Bourbakist feint of behaving for all the world as though the nastier implications of the theorems of Gödel and Turing had effortlessly been circumvented through redoubled axiomatizations would shape Cowles's attitudes toward computation for decades to come. In practice, Bourbaki would come a charm to ward off cyborgs" (Mirowski 2001: 23/394).

Even if these decades are now past, and today's economists 'get out and calculate', the liberation they draw from it is the same liberation they experienced with Bourbaki: the liberation from the burden of meaning. The chimerical character of the economist today is not that of a "cyborg"; it is not the mixture of realities previously perceived as irreducible, of the social and the natural, or, for that matter, of representation and the represented. The chimerical character of the economist is that between the reality that gives rise to their practices, and the reality that stems from it, the reality "of" science and the reality *of* science.

Let me quickly sum up Debreu's intervention in economic theory. Although the theoretical history of the axiomatic method finds a preliminary end in the result of 1974, the influence of the axiomatic method goes beyond the history of theories it informed, and beyond the methodological device it was associated with. Rejection of GET combined with the affirmation of its standards of scientificity are still today common practice among economists. Economics today still can be mapped along the different *interpretations* of why GET is insufficient (as I attempted above). This is why since the beginning of the 1980s economics seems to head straight beyond the paradigm of GET as well as rational choice, yet moves even further beyond their contestable ethos as scientists. Even though economists do not share anything with Debreu's theoretical intuitions – he did not have *any*! –, they share his intellectual ethos more than that of any other economist before him – namely *no* ethos!

So much for economic theory. What happened to Debreu after 1974? For Sonnenschein, the indeterminacy result was reason enough to turn cynical about the discipline. "This work was great fun. I often find myself beginning with the hypothesis that the king has no clothes", he said later (in Feiwel 1987: 331). Mantel, instead, plead for a turn to game theory (see Rizvi 1994). All in all, neo-Walrasian enthusiasm softened. Debreu, when asked about the 1974 results, tried to squeeze out a positive message so that it may lead economists to other topics without yet embracing Hahn's conclusion (see Feiwel 1987). In practice, nonetheless, now already in his mid-50s, he backed away from the stage of economics. After 1974, Debreu published less than before. The work he did on regular differentiable economies and the like was, knowingly, more something for devotees than "the working economist".

In 1975 he received an appointment as a mathematics professor at Berkeley. It must have been like coming home. In 1976 he became "officer" of the French Legion of Honour and the "coach" of the football team of Berkley (Anderson 2005). His team played against Arrow's at Stanford – all things one does *after* the rest has been done – things one does in one's age.

Remember that Debreu, back in 1944, considered going into astrophysics instead of economics. He did not forget his passion:

> By 1982 it seemed that time was softening the edges a bit. (…). My father seemed warmer and less formal; he hiked at Roy Reyes, played bridges with his grandchildren, and loved to get out his telescope on starry summer nights and for special events like solar and lunar eclipse (de Soto 2005).

Watching the stars must indeed be a closer experience to the aesthetic appeal of Bourbaki than doing economic theory: a safe distance from the world – elements and sets, stars and clear nights. In the early 1980s, Debreu must have believed his days in economics were numbered. He implemented a sense of rigor in economics, and may have appreciated how the profession continued in other directions. There was no longer any question of being an economist or not. Gerard Debreu made it. No Discussion.

> Then in October 1983 came the thunderbolt out of the blue: the Nobel in Economics. I don't think any of us in the family at the time recognized it for the disaster that it was to be. The world, of course, was doing him a great honor and the trip to Stockholm which we all took together was magical in many ways. We attended the ceremonies, shook hands with the king and queen, were interviewed on television, had banquets at the royal palace and the embassies, and danced at the ball. My sister's 8 year old son, Jeremy, lost a baby tooth in the middle of the banquet with the king and queen and brought his bloody tooth wrapped in a white linen napkin for the his grandfather and the queen to see. I danced Swedish folk dances in a dress with a 5-inch train in a ballroom whose walls were covered in gold (de Sotot 2005).

(4) The Disaster of the Bank of Sweden Prize, 1983

10th of December 1983, Stockholm, Royal Academy of Science. Gerard Debreu receives the *Bank of Sweden Prize*. Into this festive mood the affective history of the biography of Gerard Debreu culminates. After Bourbaki's enthusiasm, Debreu's youthful fascination and unresolved existential dilemma, after years of professional discreetness, the mathematical experience of separating structure and meaning is now celebrated with the greatest honour of economic science. Now we are prepared to answer the question I have posed several times: How was that possible? How was it possible to announce in front of the entire world that Debreu's work is of "vital importance for the understanding of the market" (Press Release). How could one grant weight to Debreu's work? And at which expense? And what does this tell us about economists' intellectual needs?

The Bank of Sweden Prize in Economic Science in Memory of Alfred Nobel (translation varies from year to year) is not to be mixed up with the Nobel Prize given for other disciplines like physics, chemistry or peace. The Prize was "bought" by the Bank of Sweden in 1968. It does not have the same name, but the formal rules are equivalent with the actual Nobel Prize, including their common ceremony on the 10th of December each year. The Prize was one of the manifestations of the great success of economics in shaping post-war institutions of science. More than that, the prize could be seen as the last manifestation of the scientification of economics in the sense that it stands for the possibility of assigning *symbolic* value to economics. Only uncontested values can be symbolic, and only symbolic values make up a festive celebration. Economists are celebrated shoulder to shoulder with physics and peace without evoking suspicion of not being worth Nobel ideals. Since I argued that Debreu was central to the symbolic value of economics as a science, it is not surprising that he, too, had to receive the prize sooner or later. He contributed to the rising belief that economics needs a Nobel Prize.

Ever since the Nobel for economics was launched, however, some resistance overshadowed the celebrations. Most notably the nephew of Alfred Nobel, Peter Nobel, argued outspokenly that the prize was against the spirit of its founder. The Prize, he wrote in 2001, is "a PR coup by economists to improve their reputation" (in Brittan 2003). Cassidy in 1996 argues for the abolition of the prize for the sake of more pragmatism: "Deprived of the publicity surrounding the annual Stockholm ceremony, economists would actually have to do something useful to get noticed." (1996: 50) Even the head of the Bank of Sweden, Kjell Olof Feldt, has advocated abolishing the prize (in Brittan 2003). For the Marxists Resnick and Wolff the prize "sits in a clear cause-and-effect relationship to the predominance of capitalist institutions in society" (1984: 31).

In 1974, there was a characteristic confusion when the prize was given to the socialist Myrdal and the liberal Hayek. Did the committee worry about appearing politically biased? Myrdal said that he would abolish the Prize because it is given to people like Hayek (but he, too, accepted the honor). Hayek has always been skeptical about the scientification of economics for it plays into the hands of the left. In his Nobel lecture, *The Pretence of Knowledge* (1989 [1974]), he expressed his doubts whether there ever has been any progress in discovering economic laws, as one should expect from a science. When he received the prize he "toasted the King and Queen of Sweden during the Nobel banquet by saying that, had he been consulted, he would have 'decidedly advised' against creating the prize in the first place" (in Nasar 2001). Perhaps, after his toast, he also whispered into the ears of King and Queen a more friendly invitation to Mont Pelerin. For the chair of the Nobel committee, Erik Lundberg, has already been a member of this society.

If there is an unambiguous bias of the prize, it is that it supports, reinforces, or perhaps even represents the social hierarchies of economic institutions of Stanford-Yale-Chicago. Of the 58 Laureates between 1969 and 2008, more than 80% are based in U.S. departments. Practically all of them come from one of the five or six Chicago-MIT-Stanford universities. No economist has ever rejected the prize, and the profession fully absorbed it as a monument to its importance. Would it not be the first moment of critique of post-war economics when a Laureate would ask: 'Is this what I am truly seeking?', stand up, and reject the prize?

At the evening of the 10th December 1983 Gerard Debreu was 62 years old. The prize was handed over "for having incorporated new analytical methods into economic theory and for his rigorous reformulation of the theory of general equilibrium" (Press Release). Sounds harmless. But if I unfold these words in light of the preceding narrative, as I will do in this chapter, its virulence will become clear. To begin with, the anachronism of the prize is obvious. The 1970s were a time of strong antipathy against Debreu's work; yet, it was also a time when the influence of his research could actually be recognized. In the early 1980s, all economists have understood that in the preceding decades a "formalist revolution" has taken place. The first to name it was Ward (1972), and I have already gone through the intensive mourning in 1970s AEA speeches about the insignificance of work that applied the standards set by Debreu. In the early 1980s, the discontent about the old and the desire for a new theoretical paradigm grew among the entire profession, be it in the heterodoxy or orthodoxy, as well as in the commentary of economics. The inner voices of economists began to revoke from exactly the achievement celebrated at 10th of December 1983.

In order to dip into the paradox of celebrating Debreu, and to see what it tells us about economic science, a close reading of the speeches held that evening suggests itself. The Presentation Speech of one of the Swedish professors of the committee – in this case it was Karl Göran Mäler - had to solemnly justify Debreu's outstanding achievement supposedly "of vital importance for understanding of the market" (Press Release). Debreu himself had to explicate his work in his Nobel Lecture and Banquet Speech in front of a well-disposed and interested, but not necessarily well-trained audience. The reasoning of the Nobel Committee reveals at least two things: first, how one had to receive, or better: what one had to make of

Debreu's work in order to celebrate it in economics. And second, at which expense such could be done for both Debreu and the profession. What had to be solemnly silenced when announcing the "vital importance" of Debreu? Thus, why did Debreu receive the Nobel Prize, and, no less obvious, why did Debreu accept it?

The Rhetoric of Significance – (a) The Inconspicuousness of the Wide-Ranging Consequences: The Existential Meaning of the Existence of an Equilibrium?

Let me lay out three rhetorical strategies of evoking an impression of 'important research' and thus evoking the solemnity of the evening. The distinction roughly resembles the first distinction in Aristotle's rhetoric between three kinds of speech: an epideictic, juridical, and political speech (1984).

The importance of a scientific work can, first, be established by means of an epideictic tone that evokes impressions of importance by means of relating the work to other instances that are already perceived as important. Standing in front of a general audience, one may relate the work to issues that currently move social life. Here the committee has to rely on the *sensus comunis*, on *topoi* of importance. Considering 1983, one could think among other issues of the latest oil crisis, of the East-West question, of course, or also of the tumults of terror of the 1970s that Europeans, at least, still felt in their bones. Could the committee rely on this rhetoric of the "wide-ranging" consequences of Debreu's work for the world-for-everyone?

Second, importance can be established with a juridical tone in which one judges the past. A piece of work is related to a specific tradition that tackled a common research question, or applied the same method, or had the same theoretical interest. The judgement of the committee in this respect can be taken as an active writing of the history of economic thought. They give shape, manifest, and authorize the incontestable importance of the work. Since the judgement is positive, such a history is necessarily linear to some degree, constituted by some failures following the final success of the honoured person. How did the committee celebrate Debreu's "long-awaited solution"?

Third, importance certainly has a political character in that one looks ahead and praises the future possibilities disclosed by a piece of research. Here the speeches function as an active disciplinary policy. They include some kinds and exclude other kinds of research that are worth pursuing in future. Here we would expect the rhetoric of "path-breaking", promising research that envisions new fictions of science.

Path-breaking, long-awaited, and wide-ranging – by means of such rhetoric of significance the moment of the Noble festivities can emerge as a milestone in the development of a discipline. A piece of science resolves a past, plays out in a present, and evokes a future, so that the ceremony is granted the importance of an "historical event". This is how history proceeds in science. Talking about the importance of science is talking about the affective marks of the passage of the time of scientific institutions. Such marks allow for an intellectual orientation of the scientists' community, as well as the solemnity of the moment of handing over the prize – a value-laden moment of real weight in the life of a scientist, which amounts in this case to precisely 1.5 Million Swedish Kronor.

Actually, it had been three Million Kronor, since the Bank of Sweden paid twice for the same article. Kenneth J. Arrow already received the Nobel Prize jointly with John Hicks eleven years earlier. Karl Göran Mäler seemed to have read Arrow's Nobel lecture when he tried to address the relevance of Debreu's work regarding present social issues. The existence of an equilibrium is a "phenomenon", Mäler said, that is "so much part of everyday life, that one generally does not stop to consider it" (1983). Since that first strategy of celebrating Debreu remained rather underexposed through the rest of the evening, it is worth wandering off for a moment in order to get a feeling for the wild range of discussions one had to enter at that point. How did Arrow justify the relevance of the existence proof?

General equilibrium theory, as Arrow opened his Nobel lecture, enhances the trust in markets which is at stake at moments of uncertainty.

> In everyday, normal experience, there is something of a balance between the amounts of goods and services that some individuals want to supply and the amounts that other, different individuals want to sell. Would-be buyers ordinarily count correctly on being able to carry out their intentions, and would-be sellers do not ordinarily find themselves producing great amounts of goods that they cannot sell. This experience of balance is indeed so widespread that it raises no intellectual disquiet among laymen. They take it so much for granted that they are not disposed to understand the mechanism by which it occurs. The paradoxical result is that they have no idea of the system's strength and are unwilling to trust it in any considerable departure from normal conditions (Arrow 1972: 253).

Arrow showed in these lines a conception of market equilibrium as the consistency of expectations and plans, which Hayek made popular. With some interpretive imagination, we can still hear the echo of Smith's baker man, who we expect to bake our bread not because he feels committed to, but because he does so for himself. Going to the bakery, we do not trust the baker, but the market. When we enter a store and expect to find the products we seek, Arrow suggests, we implicitly assume or show belief in the existence of an equilibrium. The belief in an equilibrium is, in Husserl's terms, the *Urdoxa* of economic life in capitalism. First of all, and for most of our life in markets, the "phenomenon" of the (actual) existence of market equilibrium does not cause intellectual disquiet, since all our market practices already presuppose a basic belief in the existence of an equilibrium. For this reason we do not expect the shopkeeper to cheat us, do not negotiate, and thus take prices as "given". Is the existence of an equilibrium the transcendental condition of life in capitalism?

Arrow poses the existence question in a similar fashion as Heidegger poses the question of 'worldliness'. The logic of the market is so familiar to us that it is too close for being visible and graspable. We see through the market but not the market itself. The invisibility of the market's "hand" that guides our practices is the same invisibility, in terms of Heidegger's example, as that of our glasses 'sitting on our nose'.

> When, for instance, a man wears a pair of spectacles, which are so close to him distantially that they are 'sitting on his nose', they are environmentally more remote from him than the picture on the opposite wall. Such equipment has so little closeness that often it is proximally quite impossible to find. Equipment for seeing – and likewise for hearing, such as the telephone receiver – has what we have designated as the inconspicuousness of the proximally ready-to-hand. So too, for instance, does the street, as equipment for walking (1962 [1927]: 141).

Does the market 'sit on our nose', inconspicuous always there? Is the existence of an equilibrium an Heideggerian '*Existenzial*' of life in capitalism? Is the economist's abstraction the abstraction from the familiar, the distancing from what is too near to be close? Is this what Arrow meant? He did not elaborate. But he did at least claim that the feeling of safety in markets should have been somehow *exposed* and even *reinforced* with the existence proof. The belief in markets, according to Arrow, needs to be strengthened because in some exceptional cases we lose trust in markets. Just as the existence of our glasses pops up in the moment they break, as Arrow thought about the importance of GET, the basic belief in capitalism can be shaken in moments of "war" and "catastrophes" (1972: 253).

What could Arrow have meant? If we think of the decades around 1972, did this mean that people would less likely by out stores, bunker food and water in case they have to hide in their cellars while Air Force One circles around the earth? Hardly. Would that mean that stockbrokers gamble away less money when hearing someone whispering about the plans of some sheiks? Perhaps. Would that mean that people are less likely to suspect those who pretend to go to war for freedom but actually do so for oil? I doubt it. Important at this point is that Arrow believed that *there are* "considerable departures" of market order. Arrow believed that there are indeed cases that general equilibrium theory could put at stake since his intuition of this theory does correlate with an actual practical belief. And this belief is in some sense or another concrete, for it must be shared more among the people in the west than in the east. Arrow believes that his article expressed an *episteme* that correlates with a particular *doxa*. On the basis of this belief, Arrow could have enough motivation to mobilize his theoretical interest in a vivacious political life between cold war and the war on terror. He even was a founding member (together with Malinvaud) of the *Pontifical Academy of Social Sciences*. How could I possibly downplay the efforts he put into his expressive life?

With such a belief, Arrow is certainly a child of 1945. Having experienced an exceptional situation could be called a typical motive for an invisible hand theorist. Being in a world of threatened social order, of anxiety, and turmoil could make one ask: what keeps society together if there is nobody who takes care of it? In an interview, Hayek mentioned the days in Vienna following WWI, after the army left the social stage, as the fledgling moment of his interest in the social sciences. Other moments would have included the Great Depression of 1929 that was crucial for an entire generation of economists, including Hicks, Samuelson, Friedman, or also Lawrence Klein, who called it "psychologically difficult to grow up during the depression. It was easy to become discouraged about economic life" (in Breit and Spencer 1995: 21). As long as one reminds such an event and all the past intellectual efforts to which it gave rise, and rearticulates one's understanding of it in light of present social life, intellectual life is responsive to the critical question: 'What does this mean?', 'What am I up to?'.

A quick look at the 1954 article justifies the doubt that Arrow's motivation, even if he could hold onto it his entire life, informed his engagement in GET. His interpretation might have served as a rationalization of GET, particularly at evenings such as the Nobel lecture. But nowhere in this elusive text do we find the basic market belief even mentioned, let alone an account of how it orders the experiences of dealing with prices. We would expect an answer to questions like: what kind of consistency is required for the belief in the market, given that *first of all and foremost* it is unproblematic, but *sometimes* ineffective? Arrow also would have to enter a debate with market theories that start with the opposite intuition: namely, that people only act in markets insofar as they perceive a disequilibrium, as in more institutional theories. He had to encounter those speaking of the "violence the idea of a general equilibrium does to our sense of reality", as Weintraub's father Sidney did (in Weintruab 2002: 186).

Arrow could have also entered in an argument with McCloskey, to add another association with the inconspicuousness of market equilibria in economic life. She compares the price system with a *language*, through which in some sense or another our economic life becomes manifest in a social reality. Like Saussure's structure of languages, we cannot change prizes, so that their genesis slips into the unconscious of economic life – inconspicuous, invisible.

> The situation in markets is identical to that of language. No prudent person will initiate conversations with strangers on the bus about the definition of "givenness" in economic theory. (…) We use the French word amour or the English word love without stopping to quarrel about their meanings, or insisting that love actually means "hate," or "light bulb," or "the train will arrive in six minutes." That is, the on-going conversation of language – I note that Walras' colleague Saussure made this point a century ago - gives to us mere ordinary speakers of it a set of distinctions serving to define what's on offer in French or English by way of sheep/mutton as against mouton (McCloskey 2006: 49).

Would Walras agree? I doubt it, for he was much too bourgeois for such wild analogies. Consider, I may add, how effective is the language of prices. It does not only save us from negotiating with endless rows of people involved in baking our bread, but it saves us from speaking to anyone at all – a mouse click is enough. Bourbaki could leap over from "love" to "hate", on to "light bulbs" and "beer-mugs", as Hilbert would add, *without resistance* – just like those who trade "love" for "kidneys" for "blinking pocket lighters" in silence. The social reality of the market is absolutely discreet – precisely as Debreu. If one learns one thing from The Phenomenology of Economics, then it is that *structures don't tell*!

These short remarks show that Arrow actually opened a can of worms in commenting on the meaning of the inconspicuousness of market equilibrium. He had to seriously ask himself: Can there possibly be a *meaning of the structure* of the market? How could the structure of the market be endowed with meaning? A satisfying account of how a theoretical interest arises from the basic belief in markets would have to answer this question. Even if Arrow did not do so, his understanding shows that for him this question could at least be posed meaningfully. And in exactly this sense he differs from Debreu. The very fact that the text was written by Arrow and Debreu, yet associated with a different basic understanding of GET shows that no theoretical interest, let alone a 'basic belief in the market', was at stake while working on the article.

(b) The Long-Awaited Historical Dumbness: 'Debreu Proved Smith'. And how Close Discreetness and Cynicism can be.

At the evening of Debreu's Nobel Prize, apart from the short reminder of Mäler, there was no further reflection on this first type of relevance for everyday life. The adopted tone was rather that of "penetrating basic research" (Press Release) that appeals to the layman's understanding of his own inability to understand. Yet, we find much about the second strategy of establishing the relevance of research, namely regarding the tradition of market theories. Here my narrative arrives and also returns to "Smith", but in a very different sense than 'moving beyond the intellectual milieu of the mercantile discourse'. Now we move beyond any intellectual milieu – Smith's in particular.

The main reasoning for the Prize resembles a historical link that is symptomatic for the neo-Walrasian community as well as for the historical mind of all post-Debreuvian economics. This reasoning shows that one needs to embrace the scope of the whole of modern economic science in order to celebrate Debreu. Mäler made a direct link to the initiating ignition of economic science – the invisible hand. Debreu and Smith had the same in mind, Mäler suggests, but Smith had only a vague intuition and a metaphor rooted in political science. Debreu, instead, had the scientific proof. *Debreu proved Smith*:

> In a decentralized market system, individual consumers and firms make decisions on the purchase and sale of goods and services solely on the basis of self-interest. Adam Smith had already raised the question of how these decisions apparently independent of one another, are coordinated (…) Smith's answer was that (…) prize systems *automatically* bring about the desired coordination of individual plans. Toward the end of the 19th century, Leon Walras formulated this idea in mathematical terms as a system of equations (…). But it was not until long afterwards that this system of equations was scrutinized to ascertain whether it had an *economically meaningful solution*, i.e. whether this theoretical structure of vital importance for understanding the market system was *logically consistent* (…). Arrow and Debreu managed to prove the existence of equilibrium prizes, i.e., they confirmed the internal logical consistency of Smith's and Walras' model of the market economy (Press Release, *e.a.*).

In these lines, the identification of mathematics and economic scientificity that underlies my affective history of Debreu has been made *explicitly*. The committee's argument says roughly that 'Debreu proved Smith, that is, that markets work automatically, by showing that equilibrium has an economic meaning, that is, that it is logically consistent.' The easiest reaction to such lines would be to quash them as bad journalism. Note, however, that they do resemble a rough version of most openings of myriad neo-Walrasian articles, which liturgically repeat (freely reparaphrased): '(i) One of the fundamental features of an economic system is that independent actions are mutually consistent with each other (…). (ii) Arrow and Debreu have shown that (…). (iii) However, the assumption of x can be relaxed under condition y, specified in topological terms z. (…).

The historical the committee's judgment establishes is exclusive: it says nothing but that only since Debreu has there been a genuine economic science, because only since then one can speak of a generic market of inner-consistency, and thus a proper object to study. Only since Debreu, markets have gained an "economic meaning". All history of economic thought before Debreu is wiped out to the extent that it appears *preliminary* to economic science. Popular science press repeats Mäler, such as here Eugene Garfield:

> Smith's assertion was suggestive on an intuitive level. In fact (…) it provided the justification for antitrust regulation and underlies policies that rest on faith in the operation of a free market. But Smith's exposition on the invisible hand occupies barely a page in his landmark work (…) Smith's propositions are vague and ambiguous, and did not even constitute a proof that there exists a competitive market equilibrium. (…) [I]n 1954, Debreu and Arrow co-authored a brief paper (…) [It] established realistic conditions under which equilibrium could be achieved (1985: 69f.).

One does not need much knowledge in the history of economic thought to debunk the hair-raising link of Debreu and Smith – hair-raising for the equation of "economically meaningful" and "logically consistent". The historian Mark Blaug, for example, calls the article of Arrow and Debreu one that "exhibits the worst features of formalism", namely, that "the equilibrium solution of the mathematically formulated economic model" was taken to be "the final answer to the question that prompted the investigation in the first place" (2003: 146). Sure, but what if it is inherent to their work that one forgets the question that informed the "investigation"? What if this little bit of historical knowledge that is necessary to debunk the reasoning of the committee is no longer obvious, precisely because of the success of this article?

Commenting on this reasoning, then, means less to debunk it as obviously wrong, but rather to take it seriously in what it shows of economics. The historical link of Smith and Debreu shows what had to happen to economics in order to grant weight to Debreu's work. It reflects less the motivation of Debreu and what was important for him, but it shows how Debreu, unwillingly, has altered the conditions under which a contribution in economics can appear significant. I need to read the reasoning of the committee as a symptom rather than a reflection of Debreu's work. Debreu altered the values of economics to such extent that his work could appear as important, although, ironically, he himself did not perceive his work as particularly relevant for economics at all.

In order to give a full sense of the historical dumbness the Debreu-Smith link reveals, let me recall some of its intermediate historical elements. Those in the audience who have ever heard anything of the history of economics could pose the following or countless other questions, each of which proves the committee's reasoning to be sheer farce.

> Debreu proved the existence of a general equilibrium in a competitive economy. Did he therefore prove that Hicks attempts in Value and Capital to take general equilibrium as a framework of growth, money, stability, and capital makes sense? Did he therefore prove that the efforts of the 1930s of estimating statistically Walrasian demand and supply is possible? Did he therefore prove that the Walrasian framework is indeed a justified generalization of Marshal's humble partial equilibrium? Did he therefore prove that the production and consumption of wealth could be subsumed under the same logic although the one concerns nature, the other ethics as Mill once thought? Did he therefore prove that Cournot's notion of perfect competition is the holistic approach proper to capture how everything is connected with everything in the economy? Did he therefore prove that rivalry between people is really beneficiary for society as liberals before 1850 believed? Did he therefore prove that Smith's invisible hand is scientifically grounded? Did he therefore prove that the market works automatically?

Did Debreu bring an end to this history of contests of rather diverse theoretical interests? Did he bring an end to a long tradition of "making the invisible hand visible", explicating its political and social consequences and conditions? Did he bring an end to the riddle of how the invisible hand brings about social harmony *ex nihilo*, or at least out of something that by no means predetermines such a harmony?

Since Debreu, economists have the "proven" permission not to think about the market on the basis of slippery intuitions of what the market is all about. Debreu proved that the market is a generic object of science, and beyond messy economic talk. Now the market can be investigated "economically meaningful". Speaking about the market economy is scientifically approved talk. Or, more vital for the self-understanding of the economist, economists do not have to rely on already socialized beings borrowed from other sciences. Economics is a separate science. Economics genuinely achieves something. Since 1954 (or 1983?), economics makes sense.

The crucial question is this: Under which conditions could any economist possibly laud this hair-raising historical reasoning that subsumes contradictory theoretical interests under one generic object? Apparently, only if *the problematic* of the tradition of market theorizing is downplayed. What is neglected in those lines, in particular in the equation of "logically consistent" and "economically meaningful", are the *interpretive gulfs* that separated and motivated all market theorizing. These interpretive gulfs, to mention only the most salient examples, are those between competition and rivalry, efficiency and growth, utilitarian and naturalistic foundations, historic versus mechanistic conceptions of time, empiricism versus apriorism, normative versus positive interpretations, and, of course, socialism versus capitalism. To say that Debreu and Smith had the same thing in mind but Debreu the scientific proof is to show that one has lost a sense of these gulfs. The condition of granting weight to Debreu was to discredit the possible weight economic theory can have. The first condition of celebrating Debreu is thus that economists are *insensitive to the problematic* of market theories. Since I characterized the life-world as the locus from which problems come, this reasoning manifests nothing but the oblivion of the life-world. Only by means of a problematic, of something bothersome, of an issue to be resolved, are economists responsive, and might possibly claim something of weight or relevance. Economists are liberated from the burden of meaning. Applause to Debreu.

Recall once more one of these interpretive markers of GET: the socialist calculation debate. After Hayek's last contributions following the war, the debate calmed down. In light of my narrative, it was not Hayek who closed the debate, but Debreu because after Debreu this debate could not be meaningfully continued *in* science. Economists, in harsher terms, became discreet about their own concerns. Or if I reverse this statement, economists became cynical about the very possibility of being concerned with an economic claim. Between the *discreetness* of Debreu and the *suspicion* that economic meaning is beyond science, there is but one small step, as the following reply of Debreu shows. In one of his last interviews, Debreu was again asked about the interpretive indifference of the Pareto optimum, which in his first article he still referred to as a "certain danger".

> J'espère que la dimension normative est très réduite. Si je considère par exemple l'étude de l'optime de Pareto, elle a donné lieu à des discussions conceptuelles par des économistes libéraux qui ont dit: 'Ah! Voilà! C'est démontré!', et d'autres économistes de tendance marxiste ont dit: 'Ah! Voilà! Les hypothèses précises qu'il faut faire sont inacceptables pour avoir l'optime de Pareto'. Moi, j'adopte simplement l'attitude suivante: que les hypothèses qui portent à des conclusions on peut en faire ce qu'on veut: si cela satisfait les économistes libéraux et les marxisants, parfait! Je ne peux rien demander de mieux. Intellectuellement vous êtes emporté par le courant des idées et vous allez dans la direction où il vous porte (in Bini and Bruni 1998).

Marxian? Liberal? Both? *Parfait*!

When it comes to the economic meaning of Debreu's work, Debreu withdraws. He could no longer believe that economic claims mean more than what is presently the "current of ideas", such as the liberal currents of the early modern period, the socialist current in high modern economics, and the neoliberal currents since the 1970s. While in his young years with Allais, he still warned of the risk of underestimating the need of interpretive labour, he himself became suspicious about the very possibility of a scientific economic claim. While his discreetness could still count as a way of showing respect to economic meaning, now it leads to the cynicism of letting economic meaning flow into the current of ideas – into the stream of ideology. Everything one can claim economically beyond mathematical form is beyond the control of science. The intellectual life of an economist is like a drop in the current of ideas, a "particle in a high-dimensional space". What else is this, phenomenologically speaking, than the thorough degeneration of economic theorizing?

We slowly get a full image of the affective becoming of Debreu, the mathematical (pause) economist. His career begins with the secret attraction of making an economic claim in 1944, continues with being deterred from an economic claim in 1949, avoiding it until 1974, forgetting it until 1983, and then, after being celebrated as an economist, becoming cynical about the very possibility of any economic claim.

The Path-Breaking Degradation of Economists to Taylor Workers: 'Debreu Proved Smith with Bourbaki'

The oblivion of what could be problematic in economics is even more apparent when it comes to the remaining rhetorical strategy of importance: the political strategy regarding the future research enabled by Debreu. How did the commission appraise Debreu's influence on economics? The effect of Debreu, as the committee had to notice in the early 1980s, did not concern GET. Although Debreu was praised for having solved the oldest problem of economic science, this, ironically, did not explain his importance because hardly any economist still cared about this old problem in 1983. Debreu's influence was more of an "indirect" kind, the committee had to admit. Debreu's actual importance has nothing to do with economic theory, but with the "choice of methods". The "unsurpassed effect" of Debreu's Bourbakism had to be acknowledged indirectly. An explicit reflection on Bourbaki had shown the non-scientific character of this "method" – which is not a method at all but a lived experience on its own. If there was an influence of Debreu, it was the bright shadow he cast on the discipline – sheltering, threatening and difficult to identify.

Recall Bourbaki's ambivalent relationship with the future of mathematics. Bourbaki wanted to anticipate all possible mathematics by means of indifference to the contexts from which mathematics stems. Bourbaki is the "end" of mathematics since mathematicians merely refill structures – like "Taylor workers". The same the committee celebrated with Debreu.

> Debreu's foremost contribution is perhaps of a more indirect nature, however. His clarity, analytical stringency, and insistence on always making *a clear cut distinction between a theory and its interpretation* have

> had a profound and unsurpassed effect on the choice of methods and analytical techniques in economics (Press Release, *e.a.*).

What is meant by the clear-cut distinction between interpretation and theory, without naming, is nothing but Bourbakism: structure and meaning, theory and interpretation, form and content are separate. The unsurpassed and profound effect Debreu had on the methodology of economics is, in other words, that interpretations of the market cannot possibly affect the scientificity of economics. The condition of celebrating Debreu despite the fact that neo-Walrasian economics has already lost appeal was to arrive at a level of scientificity *beyond* the intellectual interest of economists – just as scientification of economics meant to arrive at a level of reflection beyond economic suspicion!

It would be interesting to consider the committee's philosophical naivety and historical ignorance in the context of the rise of the field of history and methodology of economics in the 1970s and 1980s. Just as it was necessary for celebrating the Debreu-Smith link *not* to reflect on it (resembling the historical indifference), it was necessary not to reflect on the Debreu-Bourbaki link (resembling the philosophical naivety). Indeed, in the same sense as it became the predominant attitude that there is not much to gain from reading Smith and going back in history, it became the predominant attitude that there is not much to gain from discussing Bourbaki and the sort of mathematics one "applies". In the same sense as the historical indifference of economists allows for a *separate* interest in the history of economic thought, the philosophical naivety allows for a *separate* interest in the philosophy of economics. Only since Debreu are "method" and "history" separate concerns in economics. Their separation from the discipline shows that they neither contribute any longer to the ethos of economists as scientists (Düppe 2009).

Let me thus bring the threats of this chapter together. The irony was that although Debreu was the only one who strictly separated mathematical structure and economic meaning, and who thus separated the Smith link from the Bourbaki link, he could be celebrated only in combination of both. Reasoning only with "Smith" was not sufficient since GET was outmoded; reasoning only with "Bourbaki" was beyond economic theory. Moreover, only as long as neither is reflected upon, it was possible that Debreu could be celebrated with the prize for economic (pause) science, hiding the inner tension between a theoretical interest (Smith) and the claim to epistemic authority (Bourbaki).

At the evening of the Nobel festivities, there remained two blind spots of the rationale of handing over the Prize: Smith and Bourbaki. Only because one did not ask what Smith and Bourbaki were actually up to, could one celebrate Debreu's long-awaited and path-breaking contribution. Otherwise one would have seen that "applying Bourbaki to Smith" is a self-defeating intellectual program, and that the reasoning of the committee was rather a symptom than the rationale of that program. Reflecting on the clear-cut distinction between theory and interpretation would have undermined all of the three mentioned strategies of establishing the economic significance of Debreu's work. Debreu proved Smith with Bourbaki: to neutralize the past and undermine the future of economics are the two expensive conditions necessary to celebrate Debreu's importance for economics.

The most virulent moment of the evening was thus when both Smith and Bourbaki together but invisibly were used to celebrate Debreu's achievement. According to my narrative it says that the *condition of the significance of market theory* – the Smith line – is its *insignificance for the economist* – the Bourbaki line. Such virulence constituted the most value-laden moment of the evening at the 10th of December 1983: Debreu proved Smith with Bourbaki.

> Professor Debreu, (…) More than anyone else, you are symbol of a new approach to economic analysis, an approach that, while highly abstract, yields a better intuitive understanding of the basic economics. Your influence on methods, standards, and analytical techniques used by economists has been outstanding. On behalf of the Royal Academy of sciences I wish to convey to you our warmest congratulations and now I ask you to receive your prize from the hands of His Majesty the King (Mäler 1983)

Applause, tingling under the skin, shaking hands, 1.5 Million Kronor.

(5) Debreu's Methodological Apologies after 1983: Defense or Excuse?

At the evening of the Nobel festivities, after Mäler's speech, Debreu also had to explain in his Nobel Lecture his contribution to economics – in reality Nobel lectures are held two days before at the 8th December at Stockholm University. He titled his speech *Economic Theory in the Mathematical Mode*. This was one of the first occasions when Debreu had to *speak out* about his work – in front of the entire world. What must have been striking for the audience is that Debreu's account of his contribution to economics hardly overlapped with the account of the committee. The link between Debreu and Smith should have reflected Debreu's own personal engagement if it was a credible account of his 'vital importance'. After Debreu's speech, however, one could have reasonable doubts whether he ever had read Smith. He mentioned the invisible hand only in passing, let alone the other economists in that line. Although at the end of his speech he calls a "profound insight" of Smith that "the many agents of an economy, making independent decisions, do not create utter chaos", yet he does not elaborate the "central importance" of that insight at all (1983: 98).

The body of his speech consists of mainly technical issues in mathematical economics, which are introduced with some historical remarks, and close with some notes on the virtues of mathematical economics. Roughly said, Debreu spelled out what remained implicit in the committee's judgement concerning his influence on "method", namely his link with Bourbaki. Yet he mentioned Bourbaki like Smith only in passing when pointing to his different intellectual socialization as compared to Arrow. The body of research he mentions include, freely summarized, the following.

> I have proven the existence of a general equilibrium in a competitive economy. Therefore I have proven that Wald was right to question Cassel's sloppy reformulation of the even more sloppy formulation of Walras' market as n equations with n-1 unknowns as the mathematical proof of existence. Therefore I have proven that the scientific accidents of Cournot in 1838, and Thünen in 1826 were path breaking in freeing economics from calculation altogether. Therefore I have proven that differential analysis as applied from Pareto to Hicks can be dismissed for the sake of convexity analysis as utilized with Minkowski's hyperplane theorem, Lyapunov's and Hahn-Banach's theorem. Therefore I have proven that Kakatuni's fixed point technique describes an equilibrium rigorously in topological terms just as Nash did for games with n-players. Therefore I have proven that

> von Neumann's notion of a game can be generalized as an equilibrium. Therefore I have proven that Koopmanns did well in changing the outlook of the Cowles Commission from empiricism to theory. Therefore I have proven that Bourbaki meets economists' intellectual needs.

His speech must have been surprising for the audience. Apart from the mentioned names and some of Debreu's students, there have only been a few economists, not to speak of the layman, who are versed in the history of that research. What is most striking is that his position in this "tradition" is intelligible only on the basis of the last link to Bourbaki.

The very fact that Debreu held a speech on his work, reflecting on his own tradition of mathematical economics, was already a novelty for him. After the many years when others next to Debreu mediated between him and the profession, he now had to give his own account of his "methodology". Only after 1983, Debreu felt the need to set things right and spoke openly about the virtues of the axiomatic method. Only after 1983, he felt the need to defend the axiomatic method as a methodology of economics. Only then he became aware of the possible misuses, side effects, unintended consequences, and lacking "social benefits" of rigor that has "satisfie[d] [his] deep personal intellectual needs" for so many years (Debreu 1984b). The speeches he held after 1983 were an ex-post justification after he already ceased publishing, and after the profession already moved away from the theoretical paradigm of neo-Walrasian economics. Thus, after 1983, how was it possible to defend mathematical reasoning in economics?

At three quite prominent places Debreu had the chance to defend mathematical economics *as a method*. The first was his mentioned Nobel Lecture, "Economic Theory in the Mathematical Mode", second in his Frisch lecture at the *Econometric Society* in 1986, "Theoretic Models: Mathematical Form and Economic Content", and third in his Presidential Address of the *American Economic Association* in 1990, "The Mathematization of Economic Theory".

In this chapter, before closing the affective history of Gerard Debreu, I discuss these methodological speeches. They do not add to the affective history of Debreu's intellectual biography. My discussion puts the ambiguity that runs through his life in methodological terms: Applying Bourbaki to Smith. I show how Debreu methodologically toggled between Platonism and pragmatism of the axiomatic method, between Bourbakian and Hilbertian intellectual values, between mathematical form and economic content, and thus between addressing and undermining the economist in economics. I show, similar to Weintraub, "how the pure and impure were constantly intermingled in mathematical practice" which represents both "the attractions and dangers that fertilized the transplant [of Bourbaki to economics]" (2002: 104). In a wider perspective, this discussion will bring us back to the second threat of The Phenomenology of Economics next to intellectual responsivity: social responsibility. The social responsibility of economists, rather than any ontological or epistemological concerns, is where the actual problem of the axiomatic method in economics occurs.

The two questions to which Debreu's speeches reply, are first what *is* the axiomatic method, and second what is it *worth* for the economist? According to how I have pictured Gerard Debreu, the exposition of his methodology must show the following: When explicating the axiomatic method along the radical Bourbakian separation of meaning and structure, it must be impossible to speak of a *theoretical practice* and thus of a positive role of the economist. The self-defeating character of mathematical economics, which has often been stated (regarding the axiomatic method e.g. Clower 1995, and regarding GET e.g. Kirman 1989), is a matter of the redundancy of theoretical practices: practices of abstracting, simplifying, explaining, or any other kind of reasoning given a particular *theoretical interest*. As soon as mathematical form and economic content are separate no theoretical act is possible.

Second, as a consequence, when defending the axiomatic method in its advantages for the economist, Debreu must continuously back away from the radical separation of mathematical form and economic content. Advantages for the economist can only be established by smuggling other intellectual values than axiomatic rigor into economic theory. Conversely, only as long as there is no separation of theory and interpretation can economic theory be relevant for the economist. Debreu's speeches have thus been "apologies" in two senses: they did not only defend it, but actually also excused the false promises mathematical economics may have evoked.

The Four Steps of the Axiomatic Method, and its Fifth Wheel: Interpretation

The creed of Debreu's methodological conception is the separation of meaning and structure, or as he translates, the separation of mathematical form and economic content, or as he also says, the separation of interpretation and theory, which results in a separation of matters of science and matters of meaning. This credo in Debreu's words: "Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations" (1959: x). What then is Debreu's "philosophy of economic analysis" (1991b: 7)?

Debreu's three speeches strongly resemble each other and closely resemble Bourbaki's methodological speeches (1949, 1950). There is only one formulation Debreu repeatedly describes as a "scheme" of the axiomatic method (1984: 275, 1986: 1256-8, 1991: 4-5). This scheme follows step-by-step what Bourbaki said about the concept of a mathematical structure.

> It now can be made clear what is to be understood, in general, by a mathematical structure. The common character of the different concepts designated by this generic name, is that they can be applied to sets of elements whose nature has not been specified; to define a structure, one takes as given one or several relations, which these elements enter (…); then one postulates that the given relation, or relations, satisfy certain conditions (which are explicitly stated and *which are the axioms of the structure* under consideration). To set up the axiomatic theory of a given structure, amounts to the deduction of logical consequences of the axioms of the structure (Bourbaki 1950: 225-6, *e.a.*)

The only objects of mathematics according to Bourbaki are structures, since only structures, not the meaning of the elements can be the playground of mathematical rigour. In Debreu these lines read as follows:

> An axiomatized theory first selects its primitive concepts and represents each one of them by a mathematical object. […] Next assumptions on the objects representing the primitive concepts are

> specified, and consequences are mathematically derived from them. The economic interpretation of the theorems obtained is the last step of the analysis. According to this schema an axiomatized theory has a mathematical form that is completely separated from its economic content (1986: 1265).

In order to axiomatize a theory, one has to follow a scheme consisting of five steps: *selecting*, *representing*, *specifying*, *deriving*, and *interpreting*. Let me discuss each of them.

(1) *Selecting*. The subject of the sentence, "an axiomatized theory selects its primitive concepts", is apparently ill-expressed, yet as I suggest, is symptomatic for Debreu's methodology. An axiomatized theory does not "do" anything. Debreu does. He axiomatizes GET. He first selects. And this selection, like the beginning of all intellectual activity, is crucial for the assessment of the entire work. Or at least so one says in the philosophy of science. An axiomatic analysis is an analysis *of* a theory that is *already* in place. There must be something prior to the axiomatization through which its "universe of discourse" gains shape. This would include among other things the purpose under which a theory is developed, the intellectual space in which one operates, and the limits of the entire enterprise – thus, some reference to a *theoretical interest*. This prior theoretical interest, I emphasised above, was crucial for Hilbert to speak of a *proper moment*, a particular stage of theorizing when the axiomatic method is suitable.

When talking about consistency of theoretical practices one usually considers a theory in light of such theoretical interest. Intellectual acts are consistent in that they refer back to their interest. One either applies an internal criticism by taking the theoretical interest as given and asks if the theory carries out what it purports; or one examines further that interest and questions the legitimacy, origin, or the presuppositions implicit in the basic concepts and framework (which defines the intellectual wit of the philosopher of science). Could I criticize Debreu's axiomatization of GET on such grounds? Was Debreu committed to a particular theoretical interest when "selecting primitive concepts"?

At this point, I want to come back once more to Debreu's alleged "Walrasianism". The received understanding of Debreu, not only of the Nobel committee, is that Debreu shares a theoretical interest with Walras and transformed it into what came to be called neo-Walrasian economics. In this case, Walras formulated the theory ("the economy" as a system of n-equations), missed the rigorous proof (n-equations with n-1 unknowns, thus solvable), which Debreu delivered 80 years later (using a rigorous topological theorem). Such reading seems to be suggested in the first sentence of the 1954 paper:

> L. Walras first formulated the state of the economic system at any point of time as the solution of a system of simultaneous equations representing the demand for goods by consumers, the supply of goods by producers, and the equilibrium condition that supply equal demand on every market (265).

Walras was in fact not the first who did so. For the sake of historical accuracy, one should mention the 18th century mathematical economist Isnard who did a very similar thing (see Berg 2006). But important here is whether Debreu, when referring to Walras, was committed to a real choice. Did he share a theoretical interest with Walras?

There are apparent reasons to doubt that he did not. How could he believe that he finds in Walras the "mother-structure for the elaboration of mathematical economic theory" (Weintraub 2002: 125)? Historically speaking, such a belief would be at least surprising because "to believe that the structure of all analytical economics lay half-obscured in the relatively dormant Walrasian/Paretian variant in 1950 was a bold leap of faith" (Weintraub 2002: 122). Clower agrees, for "no later than 1939, any but inspirational connection with Walras is absent" (1995: 307). Walras's *Elements* (1874) were almost forgotten in the early 1950s and the economist of those days rather would have associated Marshall as the market theory. "It is doubtful that there were more than a half-dozen economists in the world that have ever read Walras, much less understood him" in the 1950s (Blaug 2003: 150). Not only historically, but also mathematically Debreu, at least we have learned by now, was not committed to Walras. Debreu refers to the Walras-Cassel representation and particularly the differential analysis from Pareto to Hicks only in that he rejects its mathematics.

What then was the theoretical interest of Walras's *Elements of Pure Economics* (1874)? One common way to delineate Walras from Debreu is to point to Walras's supplementary use of pure economics as a framework for applied economics. In the neo-Walrasian community, however, GET is treated as an actual theory, which amounts to a "category mistake", according to Blaug (1980: 162 ff). This answer leaves open the question how pure economics for Walras could give form to its applications. Regarding this relation let me suggest a *Platonist* image of Walras – if only for the sake of the concept. It departs less from his official Newtonian depiction of his work, but from his enduring motivation to carry out this project.

Walras's mission was a scientific social science. During his lifetime, he could hardly convince his French colleagues of that mission – neither in economics, nor in mathematics. He tried to find support for his project from French economists as well from foreign economists such as Jevons, and from mathematicians like Poincare and Volterra (see Ingrao and Israel 1990: 154 ff). Both economists and mathematicians, however, warned Walras and reminded him that the actual theoretical labour requires the consideration of real human beings – the measurability of utility functioned prominently. But Walras never gave up his mission. After a speech in 1873 at the *Académie des Sciences Morales et Politiques*, where he earned devastating critiques, he wrote in a letter to Joseph Garnier.

> These gentlemen imagine that the sole object of the application of mathematics to political economy is the substitution in certain given cases of calculus for the market's mechanism of competition (…). A mathematical theory of exchange (mine at any rate) is something altogether different. It is *purely and simply the search for a mathematical expression* of this mechanism (…) Do you perhaps believe that I consider the intensity of utility as being measurable? Not in the least. I am perfectly aware that it is not. However, this circumstance (…) is not opposed to its algebraic or geometric *expression*" (in Ingrao and Israel 1990: 147, *e.a.*).

Being accused of descriptive inaccurateness, Walras did neither react, as it is common today, by adopting a watered down notion of "utility", nor by stating that market forces are imposed to human will, nor did he swerve pragmatically. But he replied that his model is in the *immediate* sense an *expression* of the market. Walras believed, as it were, in the metaphysics of "the economy". Pure and applied did not relate like abstract and concrete, general and particular, simple and complex etc. Walras *associated* his system of equations with "the economy" on the base of a metaphysical belief in the dignity of the world in that it is capable and worthy a mathematical truth. Reality can be *expressed*, rather than represented, in the beauty, consistency, and simplicity of mathematics. Walras associated mathematics with "the economy" because in both 'everything depends on everything'. Rather than an impression of complexity, as it usually evokes, Walras had the idea of a μετεξισ – the 'participation' of the world in the closure of mathematics. The reality of "the economy" participates and is embedded in its idea.

At the heart of Walras's mathematical economics was not the belief that the analogy of Newtonian mechanics and market forces really holds in reality, as he officially frames his theory. His model was instead an *immediate expression* of the idea of "the economy". His mathematics expressed "the economy" in that it is *worth its reality*! Walras was moved by a Platonist meaning of structure of mathematics, which supposedly is, metaphysically speaking, the same as the meaning of the structure of "the economy". Not the representational, but the expressive value of Walras's model made him believe in his mission.

Debreu, instead, had no interest in expressive efforts. If structure and meaning are separate, how could the world possibly show a rigorous order? Instead, Debreu thought mathematical economics is the *condition of the possibility* of expression. Also Weintraub, without making the difference of representation and expression, argued that Debreu's take on GET meant to abstract from the theoretical interest of Walras: "The objective was no longer to represent the economy whatever that might mean, but rather to codify the very essence of that elusive entity, the Walrasian system" (Weintraub 2002: 121). Considering Walras's theory itself as the object of economic reflection beyond its theoretical interest, Debreu did something similar with Walras than Mill, Marshal, and Samuelson with their predecessors. Debreu did not connect to but cut the tie with Walras. Debreu and Walras did not share a theoretical interest, but Debreu used Walras as a jumping board for an enterprise that was motivated differently. Walras, in the first sentence of the 1954 paper, was nothing but window dressing, a placeholder for economic theory.

The neo-Walrasian use of Walras is apparent in the use of the name "Walras" in the following quote by Dierker:

> In the last section [*The Economic Framework*] we saw that Walrasian economics leads to a study of singularities of a tangent field ζ on S, an open piece of a sphere. To get a feeling of what statements one might hope for, it is useful to forget the economic origin of the problem for a while (1974: 15).

Not only would no reader of Walras recognize the tangent field ζ on S as Walras's economics, but also would hope in vain that this "while" ends at one point of Dierker's text.

Walras aside, what Debreu actually did select was not a present or past market theory, but he selected "primitive concepts". The first and only primitive concept is "commodity". To choose "commodity" as a primitive concept is to choose it as a *category* of *all possible market theories*. As Kant said that truth is a matter of statements (is it?), Debreu said, markets are a matter of commodities (are they?). Commodities are indeed *the objects* of markets, that is, what the market brings about. Markets constitute commodities. They are the *things* of economic theory because they are the things of *value*. Did Debreu make a real choice when selecting "commodity" as a primitive concept? What did he rule out?

The meaning of the word "commodity" is not only vague, but also essentially contested. It ranges from what assuages our hunger to what bears 'metaphysical subtleties and theological niceties'. It is just this question of how commodities "have" value that an economic theory of value, and furthermore of price-determination has to pose. But Debreu's *Theory of Value* does not even touch that question. Debreu did not select a particular meaning of "commodity". Primitive concepts remain undefined, and undetermined. With the word "commodity" Debreu only points to a *possible* theoretical interest but does not address it. He merely makes his axiomatization appear to be of interest for economists. The possible meanings of "commodity" are not the irreducible rest of meaning that ultimately has to determine Debreu's contribution to economics. It is not a notion *in* economic theory next to others, but the notion *of* economic theory. Debreu selected virtual, not symbolic meaning, to remind Deleuze's distinction mentioned above. When Debreu introduces the concept in chapter two of the *Theory of Value* also heuristically as one that is completely determined in *physical* terms regarding place, time, quantity, and quality (close to Aristotle's categories), then he does not select a particular meaning, but simply makes the concept appear to be capable of concreteness. Primitive concepts do not represent content, but are "*abstract schemata of possible contents*" (Ingrao and Israel 1990: 182). And so Debreu concludes the chapter that introduces the primitive concept of commodity, including "price", "action", and "value of action", as defined by commodities. (1959: 35):

> **To conclude this chapter it remains to sum up the formulation of all the above concepts in the language of the theory:**
>
> ***The number l of commodities is a given positive integer. An action a of an agent is a point of R^l, the commodity space. A price system p is a point of R^l. The value of an action a relative to a price system p is the inner product $p \cdot a$.***
>
> **All that precedes this statement is irrelevant for the logical development of the theory. Its aim is to provide possible interpretations of the latter.**

… possible interpretations, not actual! The word commodity is for the economists like the carrot on the stick in front of the donkey's eyes that it never will reach.

What then could have been the crucial choice Debreu made? Since I suggested that with Debreu something *of* economics as such shows, Debreu must have shared this choice with *all* economists before him, including Thomas Mun. In order to locate his choice, I thus need to refer back to the structuralist turn from the *oikonomia* to "the economy", the former referring to a particular conception of economic life, the latter referring to a social *structure*. If there was any real problematic choice, hidden behind Walras and the seemingly unproblematic concept of commodity, then that economic theory *has* a structure – as opposed to a literary, instructive, or preaching character. If this was the choice then what does Debreu, Smith, and Mun have in common? Could I say that Debreu does not need to be interested in the meaning of commodities in the same sense as the Smithean baker man does not need to be interested in moral codes but merely in his own interest? Is meaning and structure of economic theory separated in the same sense that the constitution of commodities in markets is separated from the constitution of the value of goods? Such analogy is not mere hair splitting if we remember that the theoretical interest of the first structuralist move in invisible hand theorizing was to deal with the economic suspicion of the motives of *economists themselves*. Then the formalism *of* market theories (the use of formal language), and the formalism *in* market theories (no substantive requirements regarding agents) have the same roots.

Debreu certainly did not choose structuralism in this sense. The analogy of the separation of meaning and structure *in* and *of* economic theory – with which I come back to the horizon from which The Phenomenology of Economics is written – is not Debreu's interest. For Debreu "structure" was a feature of market theories, not of the market. He does not discuss in what sense "the economy" could be perceived as a "structure" in the first place. Hence Debreu's formalism substantiated the belief that the problem of economic theory is a matter of dealing with a structure. And this is true today as it was 50 years ago when the question of an economic theory of value disappeared in the structuralist verve of *The Theory of Value*.

(2) *Representing*. Whatever Debreu has selected as a primitive concept, the choice is diluted when this concept is represented as a mathematical object. After the primitive concept is selected it is not discussed in light of the interpretations that have been discarded, but they are represented as something else: a *mathematical object* – x. With the first move of writing an x at the blackboard and saying "consider a set of commodities $x \in X$" – the words with which every Mas-Colell class begins – the discussion of any hermeneutical prior can no longer be discussed. Hence, I should read the first two steps within one breathe: *selecting-and-representing*. Primitive concepts are not objects of selection, but first of all subjected to a representation, a replacement, displacement, and taken "out of context". Their meaning, let alone their reference, does not find inroads into the formal analysis.

The notion of representation nevertheless suggests a common feature or similarity between the represented and what represents. Is there anything by virtue of which a mathematical object can represent a primitive concept? No, the very question of the conditions of representing cannot be posed in Debreu's scheme. Instead of representation, Debreu also speaks more accurately of the *substitution* of primitive concepts by mathematical objects. Then the substituted concepts are not the represented, but function as the identifier of the mathematical objects – their nicknames. Did the same happen when Hilbert said one could substitute in geometry notions like "line" and "point" with "tables", "chairs", or "beer bugs"? Does the same happen when in GET one poses a "numeraire"?

Clearly, the act of representation in Debreu is an act of positing. It is by no means an act of abstraction, idealization, comparison, simplification, inference, deduction or induction, and neither abduction into another context, as Khan interprets the first two steps of as a choice of metaphor (1993). The difference of Bourbakian mathematical objects and metaphors is crucial for the understanding of the role of formalism in economics. Metaphors *need* to be interpreted. Mathematical objects *could* be interpreted.

The primitive concept "commodity" is thus represented as a mathematical object, an "element" in the "commodity space" $x \in X$. The rest of the primitive concepts are not selected, so that their meaning is not even substituted. In order to settle the structure of GET, one needs at least two different relations defined on the commodity space: "preferences" and "prices". Only then one can speak of a question of consistency. Defined by the commodity space, the two relations do not even resemble the question of a theory of economic value how prizes "represent" value. Instead, they are structural requirements in order to formulate the mathematical problem of consistency.

> [T]he action of an agent is represented by a point in the commodity space, a finite-dimensional real vector space. Similarly the prices in the economy are represented by a point in the price space, the real vector space dual of the commodity space (Debreu 1986: 1265).

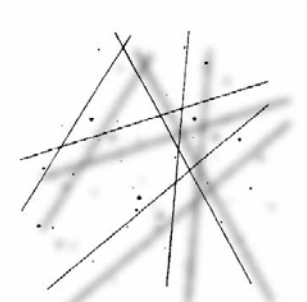
Thus, Debreu's "universe of discourse" of GET (commodity, consumer and prices) is 'explicitly listed at the outset'. There are, of course, other concepts defined by those primitive concepts. "Producers" are recognizable by their minus signs in front of their commodity sets – rather than by, say, "social class". "The economy", to quote also these lines, is defined as follows (1959: 75):

> *An economy E is defined by: for each* $i = 1, \cdots, m$ *a non-empty subset* X_i *of* R^l *completely preordered by* $\underset{i}{\precsim}$; *for each* $j = 1, \cdots, n$ *a non-empty subset* Y_j *of* R^l; *a point* ω *of* R^l.
>
> *A state of E is an* $(m + n)$*-tuple of points of* R^l.

Where then could a discussion on "consumer choice" take place? Whether the binary relations with the nickname "preferences" or "action" are, for example, reflections or determinants of the choices we make (do we?) cannot even be addressed. The young Amartya Sen made this distinction as a condition of rational choice being more than an "elaborate pun" (Sen 1973: 243). Yet Debreu did contribute to this research of modeling the individual, in particular to the so-called "integrability problem" that haunted the theories of values during the decades before the war, but for very different reasons as after the war (see thoroughly Hands 2006). Debreu, as many before and after him, investigated the conditions under which preference relations can be represented as "utility functions" in terms of differential analysis (1972, 1976). Why? Because preferences "in reality" are usually not expressed in terms of Cartesian products of commodity spaces? No, he did not engage in the issue of integrability for reasons of empirical utilization. He rather answers the question: What is missing in economic theory if one treats it as mathematics? When he showed that the assumptions of differential analysis are redundant, he moved away from economics having its methodological *Urmotif* in the numerical character of (observable) economic phenomena.

Again, I can put these remarks in a broader horizon of the theoretical perception of "the economy", specifically in that it is supposed to represent economic life. Was the question of price determination not initially how prices represent or express our acts of valuations? Only then all fuss made about the GDP, economic performance, growth and wealth means something. If the consistency of the theoretical perception of "the economy" can be formulated independently of this question, what then does Debreu show of the very idea of prices representing values, and of "the economy" representing economic life? Did Debreu indirectly show something *of* the market if its theory is independent of its meaning? Do the formalism *of* market theory and the formalism *in* market theory have the same root? Did Bourbaki liberate the mathematician from the burden of meaning, and Debreu the economist from the burden of interpreting in the same sense as the economic agent in markets is liberated from the social articulation of value?

(3) *Specifying*. The next step is to *specify* "assumptions". Those who remember their micro course know that the commodity space representing "commodities" must be (i) finite, (ii) convex, and (iii) has a lower bound. The binary relations representing "preferences" must be (j) continuous,



John Cage: *Variations I*

"Structure" in John Cage

The first two steps of the axiomatic method can be illustrated with one of the composing methods of John Cage. He put a piece of transparent paper onto a piece of notation paper and used the wrinkles of the paper as the pattern for his composition. Cage's attempt was to ease the burden of choice in order to grant full space for the creative listening of his audience, their "interpretations".

Another example in art, closer to Bourbaki, is the poetry group "Oulipo", which wrote Bourbakian poems: "each line of a poem could be replaced by its homologue from any of the nine others, while preserving the rhythms, rhyme, and grammatical structure of the newly obtained poem" (Aubin 1997: 321). The following sonnet can be reordered randomly such that one can generate a set of 1014 perfectly legitimate sonnets!

Le roi de la pampa retourne sa chemise
La mettre à sécher aux cornes des taureaux
Le cornédbîf en boîte empeste et la remise
Et fermentent de méme et les cuirs et les peaux
(The king of the pampas turns his shirt
To let it dry on the horns of the bulls
The canned corned beef makes the shed stink
And so are fermenting leathers and skins)

The same sense of *play* evident in these poems, as well as in John Cage, stamps the axiomatic method in economics to the same extent that it stamps the technocratic empiricism of today's economists.

Without going deeper into these parallels in art and mathematics, let me clarify the relationship between the structuralist reduction and the phenomenological reduction. Bourbaki's method and the phenomenological method overlap in that they both "reduce being". Neither aims at a judgment. As the phenomenologist brackets all ontological references that are given in "natural attitude", so does the mathematician. But Bourbaki reduces being to structure beyond the meaning of being. They liberate being from its meaning. In the phenomenological reduction, instead, one reduces being to meaning beyond the structure of being (remember: sense is prior to being!). The big difference, philosophically speaking, is that the phenomenological reduction allows for the question of *constitution*: What is the meaning "of" structure, what is the content "of" form, and what is meaning "before" it came to be attached to being? For this reason Bourbaki is, as opposed to phenomenology, transcendentally naive. While phenomenology traces meaning insofar as it allows us to be a subject, Bourbaki reduces being in order to come closer to a structure as the "meaning for nobody". Instead of transcendental inquiry, that is always an inquiry into one's own possibilities, Bourbaki is the liberation from a subject or a self for the sake of a secret experience of the beauty of mathematical structures. Both methods are thus intimidating. While this intimidation was fatal for Bourbaki, it was the genuine accomplishment of phenomenology.

(jj) strictly monotonously increasing, and (jjj) strictly quasi concave and (k) the technology space strictly convex. In a textbook of 1958, (j) to (k) are mentioned for the first time as follows: no increasing returns to scale (producing more does not reduce average costs), at least one factor for production (things are not like "air"), and consumer wants cannot be saturated (we are all excessive if we can) (see Weintraub 2002: 188).

After the first two steps, these two specifications of assumptions must be two different things. The confusion is that between "assumption" and "axiom". Assumptions are not specified on primitive concepts, let alone their referential meaning, but on the mathematical objects that substitute these primitive concepts. To speak about "assumptions" is misleading in the sense that Debreu does not assume something of the object under consideration like in the case of a hypothesis, a supposition, a basic belief or a mere hunch, as it is discussed in the philosophy of science. The "assumptions" are, to the contrary, the *result* of Debreu's efforts. The assumptions are actually the axioms: "then one postulates that the given relation, or relations, satisfy certain conditions (which are explicitly stated and *which are the axioms* of the structure under consideration)" (Bourbaki 1950: 226, *e.a.*). An axiomatization results in axioms saying what are the minimal requirements of mathematical objects that allow speaking of a consistent structure. The neo-Walrasian research programme, as it were, was then to show the weakest conditions of an equilibrium – "weak" in terms of the Bourbakian hierarchy of mathematical structures, the weakest of all assumptions being $x \in X$.

The neo-Walrasian discussion about "assumptions" did therefore not take place in terms of the (primitive) concepts as in the textbook, but in mathematical terms. One great result at which Mas-Colell arrived was for example "An Equilibrium Existence Theorem without Complete or Transitive Preferences" (1974). Or, as an early work of Princeton Schmeidler asked whether "Competitive Equilibria in Markets with a Continuum of Traders and Incomplete Preferences" (1969) is possible. Axioms are not like "hypothesis" or prior beliefs that are at stake when theorizing. Recall again Debreu's student: "He [Debreu] refused to comment on the reasonableness of assumptions, believing that his job was to make the assumptions clear, and it was the reader's job to assess them" (Anderson 2005). Debreu did not theorize in the face of the world, but in the face of a structure.

Sheer irony is how in the neo-Walrasian community Debreu's discreetness toward economic content could reproduce, as here in Dierker: "Economic knowledge is not required, but especially a reader without economic background will gain much by reading Debreu's classic 'Theory of Value'" (Dierker 1974: iii).

Nevertheless, some of Debreu's axioms *could* have an economic interpretation. One mathematical economist was brave enough to announce: "the behaviour of market economies depends on how convex the world is" (in Ramrattan, Szenberg 2005). While most other economists may have difficulties to imagine what a 'convex world' would look like, axioms like that of transitivity are indeed commonly understood as demanding something *of* the actual holding of preferences (do we?). Here the interpretive labour of the economics instructors begins. They literally invent narratives for mathematical objects. By virtue of their stories, the impression of concrete reference of mathematical objects is produced. In a similar fashion, textbooks undermine a historical reflection by means of interpreting mathematical structures in digestible terms. If textbooks put emphasis on intelligibility and keep the mathematics low, they, ironically, even reinforce the mathematical bulwark against a critical historical reflection. This, I am afraid, is also the case in the recent textbook of McCloskey, Klamer, and Ziliak (2008). There can be no such thing as axiomatic intuitions of economic theory that students could acquire without mathematics ("opportunity costs" being one of them). Such intuitions hide the formalism they want to avoid. The only alternative for the teaching of economics is teaching on the base of the history of economics. Only then students will spontaneously think beyond the disciplinary borders of economics.

The same irony applies of course to those economists who took their undergraduate narratives literally as descriptive truths and went into behavioural economics. I am afraid that such research is based on the confusion of assumptions and axioms and actually reinforces the underlying structure that is independent of any interpretation. It happened mostly by virtue of these "secondary" interpretations of axioms that the neo-Walrasian community could have the success as a benchmark of post-war economics. Axioms of choice, like that of independence, completeness, did not express the basic beliefs of the neo-Walrasian research program. There were none.

Again, I can speculate further on the more indirect effect of the confusion of axioms and assumptions on the discursive environment of economics. If the mathematical void of axioms is confused as the philosophical weakness of assumptions this confusion can take on in the political arena very different connotations – namely that of (cling on) *freedom*. If assumptions and axioms are confused in GET it does not demand a particular behaviour from economic agents. The mathematicians' search for "weak assumptions" confused with the scientific virtue of explaining much by little translates in the political discourse to the ideology of negative freedom. As Debreu only spoke about structures, others do not have to demand that economic agents need to have a preference for particular goods. The x of Debreu and the *whatever you want* of the liberals – is it the same? Do weaker assumptions make for a freer society? The question comes slowly to the tip of our tongue: is Bourbakism the last hallmark of "bourgeois economics"?

It is possible to go further with this association of Debreu's rigor with neoliberal politics. In order to provide an alternative interpretation of Debreu, the Marxian commentators of economics Ruccio and Amariglio pointed to the post-modern aspect of Debreu's axiomatic method. They suggest that the absence of a "body" of economic agents allows capturing the *fragmented subject* of our days, which is torn between Sushi and other pop drugs, like the "man without qualities". Debreu allowed for new "re-evaluations of the experiences and distributions of pleasure and pain, work and desire, base and refined instincts, emotions and reasons, passions and interests, sex, race, and class" (2003: 101).

> There is a refreshing quality to recent neoclassical thinking in that it mostly displaces the question of the body as origin. It proliferates, instead, a differentiated, fragmented body according to various functions, which do not necessarily impinge on one another. To say this differently, we are interested and regard with some cautious degree of approval the appearance of a body in high-neoclassical theory (as, for example in Debreu's *Theory of Value* or in Arrow and Hahn's *General Competitive Analysis*) in which bodily functions of consumption, production, distribution, choice and so forth only obliquely relate to a central, unifying dimension (2003: 110).

Thus, no "bodily unity and depth" is required, but "bodily disunity and 'flatness'." Debreu, so Ruccio and Amariglio suggest, allows for an "infinitely fragmented, 'no-self' conception of the body" (119). Clearly, Debreu would immediately turn red of shame when reading such lines. He never even changed his tie! How could he have endorsed such a liberated subject? It was discreetness, not a sense for a plural society that made him withdraw from a "unified body" of classical economics. Debreu does not touch any body, neither at its surface, nor at its origin.

Ruccio and Amariglio thus turn the argument of the insignificance of Debreu's work on its head: precisely because there is no reference to bodily agents, Debreu and neoclassical economics in general allow for a cluttered, fragmented world that is *free* for a post-modern play of meaning. Is Bourbaki's refreshing liberation from meaning – $x \in X$ – the virtue of the post-modern society that is only "obliquely" unified by the market? Whatever one may reply, Ruccio and Amariglio resemble, rather than replace, the neoliberal fascination of avoiding talk about people. Then, post-modernism works as the new hermeneutics of capitalism. Politically speaking, moreover, a post-modern pluralism based on discreet ignorance is not very different from a totalitarian world in which bodies are discreetly covered by burkas.

(4) *Deriving.* The fourth step – "deriving consequences" – concerns the kind of consistency the axiomatic method requires. Here we enter the playground of rigor, and nothing but rigor: to proceed from "fully specified" assumptions to conclusions, step by step, in full cogency. While the first three steps did not correlate with an intellectual effort, here all the labour and affective weight of mathematical reasoning comes into play. The philosophical question at this step is how formal logic and mathematical logic relate. Does mathematical logic adds something to formal logic, or the other way around, is the language of logic proper. This question once moved the generation of Frege, Whitehead and Russell. Debreu and Bourbaki are reluctant to enter such debates.

> Whether mathematical thought is logical in its essence is partly psychological and partly metaphysical question which I am quite incompetent to discuss. (…) It serves little purpose to argue that logic exists outside mathematics (…) Logical or (what I believe to be the same) mathematical reasoning (…) (Bourbaki 1949: 2).

Also for Debreu the intellectual value of rigor is simply identified with valid reasoning. "The theory of value is treated here with the standards of rigor of the formalist school of mathematics. The effort towards rigor substitutes correct reasoning and results for incorrect ones" (1959: 1).

Note that "deriving consequences" does not describe the actual practice of axiomatic theorizing, but rather its *result*. Only when the mathematical proof is presented in its final shape one "derives consequences". But actually writing of a proof is rather a process of trial and error that requires an experienced mathematician. Writing a proof is like groping one's way in the dark, playing with ever-weaker "assumptions", and trying to grasp through a forest of tautologies. While the reading of a proof might have a great aesthetic appeal, the writing is rather dirty, lengthy and uncertain. Here I may report an anecdote of one of his students in Chicago, Mark Blaug. During a summer course in 1955 at Michigan University, a doubt arouse whether a line in the proof is correct. Instead of thinking through in class, Debreu left the room. After some time he returned with the words 'of course, it is correct'. The actual mathematical labour, as it were, does not take place in class or in any other show room.

Obviously, the term "consequences" does not mean "effect" or the like. There is no deriving from something out of something different. Deriving consequences is to deal with logical implications and reformulations with the aim to prove the consistency of the axioms. Debreu's existence proof did precisely that: it showed that the mathematical relations ("assumptions") specified as the axioms do not contradict each other. Debreu's proof was an indirect as opposed to constructive proof, showing that a disequilibrium leads to a violation of the axioms.

That reasoning in economic theory and rigorous rigour are two different things is clear when I consider the concept of GET in which reason culminates: equilibrium. Along this concept, invisible hand theorizing has been contested, in particular regarding what describes a state of equilibrium. Backhouse (2004) discusses four different meanings; Giocoli (2003) shows their genesis. For Debreu equilibrium simply means consistent structure of axioms. It is not an economic concept. "In proving existence one is not trying to make a statement about the real world, one is trying to evaluate the model" (in Feiwel 1987: 243). How could it be different after the first three steps?

Recall that Debreu believed that speaking about disequilibria is a contradiction since in such state there could be no conceivable price system whatsoever (in Weintraub 2002: 146). Consistency and equilibrium are equated. This brings me to the crucial point where the equation of mathematics and science happened, which represents the bottom of Debreu's "applied Bourbakism", as well as the confusion of existence and consistency. As long as one of the core devises of economic modelling is to first show the equilibrium conditions, this equation takes place. "The necessity of proving the existence of equilibrium is now recognized", Debreu said in the mid 1980s, "and authors who propose an equilibrium concept either in economic theory or in game theory feel compelled to specify assumptions guaranteeing existence" (in Feiwel 1987: 244). A model is only worth as much as it shows to have a logically consistent solution – that is, as long as it is an equilibrium model. And this applies to models of growth, innovation, or uncertainty to the same extent. And so Debreu said about those who engaged in the

> theory of temporary equilibrium, a so-called theory of disequilibrium (a misnomer since it is a theory of equilibrium under new constraints). They show, if it were needed, that the concept of equilibrium is an organizing intellectual concept of great generality with which it is difficult to dispense in the social sciences (in Feiwel 1987: 253).

(5) *Interpreting.* Debreu mentions interpreting as the fifth and last step of the axiomatization, although the separation of interpretations is the result of the axiomatization. Thus, is interpreting a required part of the axiomatization? Or is it rather like the *fifth wheel* that one does not need, crams into a corner and forgets? Regarding the interpretations themselves, what difference does it make if they are subjected to such separation?

Separation is the great gesture of *analysis.* The Greek word αναλυσισ means to "dissolve", "liberate", "dissect", "get rid of", "abolish", and "finish". Analytic thought is in this sense that thought which silences. Recall the words with which Henry Cartan and Andre Weil launched

Bourbaki: "Now that's enough: Let's meet with some other people to discuss these questions. Let's finalize the answers, and then we will not have to speak of them again" (Cartan 1999a: 634). Analytic thought seeks to come to rest in the differences it draws. Separating science from history, episteme from doxa, justification from discovery, reality "of" and *of* science, etc. makes intellectual life redundant. It rips intellectual life out of the ground from which it gains its force: forgetting and thus calming down. Analytical thought is the (final) judgement that ossifies past: juridical thought, and thus indeed, as the Greek used the term in a transferred sense, the thought of death. The task of analytic thought is to make itself redundant.

Debreu may not have known about this meaning of the word analysis, but he indeed illustrates the separation of mathematical form and economic content with a strong image of a theory having blood, flesh and bones. He talks about an "acid test" that economic theories have to pass in order to be called rigorous – an "acid test of removing all their economic interpretations and letting their mathematical infrastructure stand on its own" (Debreu 1991: 3). Acid is put on the body of theory, corroding all the flesh and leaving the skeleton behind. Our flesh is what makes us sensible beings, responsive to touch, and thus vulnerable to offence. Accordingly, an axiomatized theory might have a strong backbone, yet has nothing to carry, is insensitive to its surrounding, and immune to criticism. And so, Debreu says: "As a formal model of an economy acquires a *mathematical life of its own*, it becomes the object of an inexorable process in which rigor, generality, and simplicity are relentlessly pursued" (1265, *e.a.*). There may be a life of an axiomatized theory, but it is a life without affection, not our life – like a skeleton haunting economic science since 1959.

Debreu's corrosion of interpretation is in fact radical. By using "content" and "interpretation" of theories interchangeably he acknowledges that interpretations are not interpretations of things that are independently given without, or before being interpreted, as most scientists believe since Bacon. Economic content is nothing but interpretation. This sounds like a hermeneutical claim. Debreu, however, equates content and interpretation not in order to highlight how the theorist is enmeshed in the material he attempts to grasp, but rather to let interpretations appear as *things*. In light of an axiomatization, interpretations do not correlate with an *activity*. They are no intentional acts, but they are themselves *things to be discovered*: "(…) whenever a novel interpretation of primitive concepts is *discovered* (…)" (Debreu 1986: 1265, *e.a.*) The *act of interpreting* simply disappears from the stage of rigor. Interpretations do not describe an activity of someone, but lie around in the world waiting to be discovered. The task of economists is then to pick them up and to fill them into the mother structures in order to give them a consistent shape, much like a Taylor-designed worker, as Bourbaki refused to admit.

Within the textual hierarchy of the axiomatic method one never arrives at interpretations, but as Bourbaki said, at "remarks": "definitions, axioms, theorems, propositions, lemmas, corollaries, remarks" (Bourbaki 1968: v). More cannot be said. Explications are excluded from intellectual practices. A "rigorous interpretation" or "rigorous reformulation", as the Nobel committee said, is inconceivable. Interpretations are not more or less suited, more or less accurate, but only *vague*. There cannot be any consistency regarding interpretations. In order to be rigorous, one needs to be indifferent to all expressive, descriptive, normative, and explicative practices. To be rigorous is to rigorously avoid the question 'what does this mean'?

All sources of meaning of practicing economics, therefore, cannot be confronted in mathematical economics. This indifference to everything one usually associates with theoretical activity is the core of the *interpretive indifference* of economic theories that I have encountered various times: Interpretations are not made. They are there.

What, then, remains for philosophers of science in terms of theory appraisal? How did Debreu 'evaluate' the model? Note that since there is no act of abstraction, and no actual act of representation *all* interpretations have the right of being economic theories. Debreu doubtlessly thinks implicitly of the interpretations of economists. However, there is nothing that excludes the interpretations of others. Since the act of interpretation renders redundant, *all* possible interpretations waiting for being discovered fit into the form of GET, like all institutional, Marxian, Austrian, cultural, sociological, psychological, and all non-academic interpretations. Marxian? Liberal? Both? *Parfait*! QED!

Weintraub is moderate when speaking of a "take-no-prisoner attitude when it came to specifying the 'economic' content of the exercise" (2002: 116). But Debreu not only takes no prisoners, as though he acts like a diplomat of economic science, but he indirectly undermines and discourages interpretations. Not only that all interpretations are potential economic theories, but also that all interpretations are *equivalent* regarding their *scientificity*. Interpretations and their underlying intuitions do not affect the scientificity of economic theory. Rather than a democratic, pluralist diplomat, Debreu wipes out the need of valuating economic theories. And only by means of such valuations something of worth could be at risk for the economist. Just as Bourbaki thought to be the final stage of the development of mathematics, the traumatizing moment of Debreu was that everything that possibly could be said about markets *has implicitly already been said*.

The Four Virtues of the Axiomatic Method and their Supplement: the Economist

According to this image of the axiomatic method, it must seem like a miracle that Debreu is able to justify the advantages of the virtue of rigor and his axiomatic method to the economist. There is nothing left of what one naturally would associate with an act of economic theorizing. In fact when it comes to sell these advantages, Debreu continuously backed away from the radical separation of mathematical form and economic content just described. All advantages can only be established by smuggling other intellectual values as axiomatic rigor back into the analysis – thus, only as long as there is *no* separation of *meaning and structure*. There are four advantages Debreu repeatedly refers to: generality, weakness of assumptions, clarity of expression, and, yes, being free of ideology.

(1) *Generality*. "The pursuit of generality in a formalized theory is no less imperative than the pursuit of rigor" (Debreu 1986: 1267). How are rigor and generality of GET related?

GET is usually called general because it is not restricted to a particular market as the market for apples, oil, kidneys or the market for 4WD-Gran-Vitara-AWD-metallic-blue'S. GET is general in that it encompasses all markets since only children, politicians, moralists, and marketing experts believe that one market is independent from another market. In "the economy" everything depends on everything else. Generality as a virtue of the philosophy of science, moreover, refers to the scope of explanation, and follows the old devise of explaining much by little. Methods and theories are as general as their explanatory scope.

Debreu gives us the feeling that generality is a quality of the axiomatized GET in this sense of explanatory scope, that is, that GET extends the limits of other theories and is applicable to a greater range of phenomena. "A newly discovered interpretation can then increase considerably the range of applicability of the theory without requiring any change in its structure" (1991: 5). With the same use of terms, he even presents the axiomatic method as *appropriate* to the ontic properties of the market.

> A global view of an economy that wants to take into account the large number of its commodities, the equally large number of its prices, the multitude of its agents, and their interactions requires a mathematical model (1991: 3).

Debreu thus evokes the methodological *Urmotif* of economic abstraction that I have traced since the first chapter: the complexity of "the economy". Complexity functions as the most common excuse to remain in a formal theoretical context. Perhaps Debreu, like all other modern men really believed that "the economy" *is* complex. But the question is whether this belief informed his work.

Debreu tops this assertion of the theoretical propriety of mathematics when claiming that mathematics is "neutral" because commodities and prices *are* numerical things: "Since economics gives a central role to quantities of commodities and prices, the use of mathematics seems entirely neutral" (in Feiwel 1987: 253). Whence the "reality"? Whence the basic belief in "the economy"? Do we now enter a discussion of the "ontology of commodities"? Why is it now necessary to point out, as Mirowski did, that

> there is no firm evidence that prices, commodity units and money were ever constituted as numbers in some pristine ontological sense: they were (and still are) contingent upon a whole range of other social practices, might be reorganized in a myriad of ways, and exhibit no 'natural' or stable mathematical character (forthcoming).

Did Debreu really believe that commodities could be convex in any sensible way? Thus, does he really speak of generality as a theoretical virtue of explaining the totality of markets? He does not enter this discussion.

The actual meaning of generality is not that of standard philosophy of science. The axiomatized GET does not encompass several market theories of particular markets, but is independent of them. How could a theory with a structure that is independent of its (referential) meaning be called general? Debreu relies here on the confusion of generality as the encompassment of content, and formality as the absence of content. Abstraction and formalization, as I repeatedly argued, are two different practices. Forms are not more or less general. There is no trouble of a "trade-off" between empty forms and particular content. What Debreu praises as generality is to be free of the logic of the general and particular.

By "general" Debreu means that one can generate a market theory out of any interpretation of the primitive concept "commodity" *immediately* and *effortlessly*, or as Bourbaki said, 'without forging ones means personally' (1950: 227). "The divorce of form and content *immediately* yields a new theory whenever a novel interpretation of primitive concepts is discovered" (Debreu 1986: 1265, *e.a.*). In his Nobel Lecture we read, "[t]he axiomatization may also give *ready answers* to new questions when a novel interpretation of primitive concepts is discovered" (Debreu 1983: 98, *e.a.*). The economist is able to immediately leap over from an interpretation to a fully developed "theory" without the effort of actual theorizing. Debreu thus admits that his formalism makes the *effort* of theorizing redundant. With the axiomatic method the theorist can be substituted just as the primitive concepts, and its possible interpretations are substituted with mathematical objects. The practice of economic theory demands less intellectual effort.

The example Debreu repeatedly refers to in order to illustrate, and celebrate the virtue of generality is markets with uncertainty developed in chapter 6 of his *Theory of Value*. The difference of certainty and uncertainty makes the world for a group of economists (Austrians, institutionalists, evolutionary, and also some behavioral) who all start their essays with 'Knight and Shackle emphasized the role of uncertainty for economic analysis'. In these cases, uncertainty amounts to a challenge for economic theorizing since the market cannot be fully determined let alone be listed in advance within a unique "universe of discourse". In Debreu, however, whether commodities are certain in the sense that we know everything about them, or uncertain because there is *time* is a matter of the interpretation of the primitive concept "commodity": 'commodity today' or 'commodity tomorrow': x_t. Uncertainty does not affect the axioms themselves. Debreu does acknowledge the importance of the difference between certainty and uncertainty, but cannot reflect it within the axiomatic scheme: "Several important questions left unanswered are emphasized below. One may stress the certainty assumption made, at the level of interpretations (…)." (1959: x). On that level, however, nothing really happens: "by simply reinterpreting the primitive concept" "one immediately leads to a new theory". The problem of uncertainty is solved by moving it outside of theoretical concern. The value of such re-interpretations Weintraub assessed critically:

> Debreu's evident enthusiasm (...) over his capacity to incorporate 'uncertainty' into the axiomatized model by keeping the identical mathematical formalism but redefining the 'interpretation' of the commodity thus should not be regarded as a new contribution to the economic theory of risk or ignorance; rather, in this reading, Debreu developed it as a ratification of the structural character of his axioms (Weintraub 2002: 121).

Debreu used uncertainty as a ratification of his method. But was there a question, or the need of ratification as long as theory and interpretation are separate? Then it should have been possible that the problem of uncertainty touches Debreu's "mother structures". How could it? The application of uncertainty was rather a way to show that in fact nothing happens if one reinterprets, and that a reinterpretation is actually *not* needed for GET to stand. Debreu did not show the value of a reinterpretation. Rather, he showed that reinterpretations do not make a difference – disillusioning, to say the least, for those who want to attack, and who want to defend GET on the base of its supposed economic meaning. Debreu showed with his application to uncertainty that there is *nothing to be said* about uncertainty.

(2) *Weakness of assumptions.* Close to the virtue of generality is the weakness of assumptions. Recall that assumptions in Debreu are not weak in relation to a basic belief of the theory – its "ontology", as it is discussed in the philosophy of science. In Debreu, the weakness of assumptions is expressed in terms of the Bourbakian hierarchy of mathematical structures, the weakest of all assumptions being $x \in X$. To say that the assumption of transitivity is weaker than that of continuity is to say that transitivity is implied by continuity but not vice versa – as Mas-Colell et al. let their students prove along lexicographic preferences in micro 1.02. In terms of cognitive capacities, for example, it could be the other way around. Such Bourbakian weakness of assumptions Debreu actually meant when speaking of "generality":

> Look at the study of consumer behaviour by means of the differential calculus where you find a complex formulation in terms of decreasing marginal rates of substitution. In contrast, the convexity analysis is not only simpler, it is also more general since the boundary of a convex set does not have to be smooth [differentiable in all orders] (in Feiwel: 247).

Though clearly a matter of mathematical structures, Debreu gives his audience the feeling that the these structures are related with the domain of applicability: "The mathematician's compulsive search for ever weaker assumptions is reinforced by the economist's awareness of the limitations of his postulates", as he describes the interaction of mathematicians and economists (1986: 267). But what is the effect on the 'domain of applicability' if the mathematician "expurgated superfluous differentiability assumptions from economic theory" (Ibid)? The confusion Debreu relies on here is the philosopher's virtue of 'explaining much by little' with that of logical implication.

The issue in the background of this confusion is what came to be known as *economic imperialism* – the infusion of economic ideas in other social sciences and economic talk in general. Economic imperialism is problematic, and different from a fruitful interdisciplinary effort because economists loose their sense of the domain of applicability, and enter other domains without caring about their characteristics – without sensing resistance when passing limits. Ironically, on the ground of the confusion of assumptions and axioms Debreu could argue that the axiomatic method was an effective regulative tool *against* economic imperialism:

> The exact formulation of assumptions and of conclusions turned out, moreover, to be an effective guide against the ever-present temptation to apply an economic theory beyond its domain of validity (Debreu 1986: 1266).

Debreu repeats here the same enthusiasm that Koopmans in 1957 already has shown about the 'sobering effect' of rigor: making assumptions explicit.

> The best safeguard against overestimation of the range of applicability of economic propositions is a careful spelling out of the premises on which they rest. Precision and rigor in the statement of premises and proofs can be expected to have a sobering effect on our beliefs about the reach of the propositions we have developed (Koopmans 1957: 147).

Sobering yes, but a 'safeguard against overestimation'? Do we sense the irony in these lines? Debreu turned perhaps the greatest vice of post-war economics into a virtue of his method having restricted it. How could he possibly believe that his axiomatized GET *ever* functioned as such regulative device to remain close at a particular domain of social life? My entire argument has suggested the opposite. The development of the theoretical perception of "the economy" is the *dissolution* of the intuition of economic life constituting a particular domain.

Becker, despite the conflict between Friedman and Cowles, could be said to have received the Nobel Prize for showing that Arrow and Debreu not only proved that the logic of the market is independent of its interpretation, but that market theory is not even restricted to a phenomenon called market – in the words of the Swedish professors, 'for having extended the domain of microeconomic analysis to a wide range of human behavior and interaction, including nonmarket behavior'. Becker goes indeed further than Arrow and Debreu, since he uses the lack of interpretation of the market as a vehicle to turn market theory into a method. Without Debreu, however, this step would have been impossible. The link between the axiomatic method and imperialism here in the words of Mirowski:

> The practical effect of the Cowles program was to toughen up the mathematical training of economists and thus repel anyone trying to trespass from another social science (…) What Cowles ultimately sought to do was to shore up the boundaries between neoclassical economics and the other social sciences. Pending that, transcendental urge was re-conceptualised as the periodic forays of the economic imperialists, bringing back home raw materials wrest forcibly from the natives as fuel for their stationary engine of analysis (Mirowski 2001: 266).

If Debreu set a benchmark for post-war economics, then contrary to his assertion, because economists tried to regain a sense of their domain by relaxing rather strong and unrealistic assumptions that came to be assigned to his "model", such as perfect knowledge, perfect rationality, symmetric information, etc. Ironically, these attempts of escaping the narrowness of his theorizing Debreu could present as evidence for his method serving as a *benchmark* of theory – perfectly in line with my notion of the negative closure of economics today.

> Its role as a *benchmark* was also perceived clearly, a role which prompted *extensions* to incomplete markets for contingent commodities, externalities, indivisibilities, increasing returns, public goods, temporary equilibrium … (1986: 1268, *e.a.*)

The economists' sensibility for an economic domain, which they try to regain when they engage in such theories, Debreu can still read as an "extension" of his axiomatic GET – quasi an application. Since the weakness of assumptions is not measured in ontic terms but in mathematical terms, relaxing the assumptions of GET does not change the mode of theorizing. Therefore, even if market theories start with an intuition of the (ontic) domain of the market they could turn out analytically equivalent with Debreu's GET. The more economists struggle to be "realistic", the more they "extend" Debreu's model.

(3) *Clarity of expression.* The third advantage Debreu lists is *clarity of expression*. Whence, out of the blue, the expressiveness of the theory? How can Debreu make us believe that the economist can use his axiomatization as an alternative *expression* of their economic theories? How could one still believe at this point in the 1980s that mathematics is a language that claims to say the same thing, just more clearly by taking the "noise" out of the conversations of economists?

> Still another consequence of the axiomatization of economic theory has been a greater *clarity of expression*, one of the most significant gains that it has achieved. To that effect, axiomatization does more than making assumptions and conclusions explicit and exposing the deductions linking them. The very definition of an economic concept is usually *marred by a substantial margin of ambiguity*. An axiomatized theory *substitutes* for that ambiguous concept a mathematical object that is subjected to definite rules of reasoning. Thus an axiomatic theorist succeeds in *communicating the meaning* he intends to give a primitive concept because of the completely specified formal context in which he operates (1986: 1266, *e.a.*)

How could one possibly communicate intended meaning with a language that is separated from any meaning? Primitive concepts are not conceptualized at all, but *substituted* with a mathematical object. The only thing clear in Debreu is the separation of mathematical form and economic content where meaning falls together with ambiguity. Debreu of course knew that axioms are not expressive. He made the separation of mathematical form and economic content even typographically visible in his *Theory of Value*.

> Allegiance to rigour dictates the axiomatic form of the analysis, where the theory, the strict sense, is logically entirely disconnected from its interpretations. In order to bring out fully this disconnectedness, all the hypotheses, and the main results of the theory, in the strict sense, are distinguished by italics; moreover, the transition from the informal discussion of interpretations to the formal construction of the theory is often marked by one of the expressions 'in the language of the theory', 'for the sake of the theory', 'formally' (1959: x).

Debreu separated passages of form from those of content with italics, Bourbaki with an asterisk, the "omission" of which "of course, have no disadvantage, from a purely logical point of view" (Bourbaki 1968: v). Only in the "(s)mall type passages", which are "irrelevant for the logical developments of the text proper", is it "permissible to draw upon an intuitive knowledge of the physical world" (Debreu 1959: 2) – note the subtlety: like a Bourbakian slip, Debreu notoriously speaks of the "physical world" when it comes to referential meaning, as though he never even considered that economic theory refers to the *economic world*.

How could there be an *expression* of something in terms of something else? If there are two "contexts" in Debreu, then a context of structure/form and another of expression. Intellectual effort only takes place within the former. The latter cannot be higher in the textual hierarchy than "remarks," or at most "corollaries". The point is that Debreu not only substitutes the 'substantial margin of ambiguity', but renders meaning unexpressed. Structures may feel good, but they don't tell anything! When using such expressive words such as "marred with a substantial margin of ambiguity" Debreu expressed nothing but the *suspicion of meaning* into which his intellectual life merged.

Nevertheless, Debreu was very successful in making the economist believe that his axiomatization implies "clarity of expression". The margins of economic theories are still today narrative, while the analytical core of models is still formal. Intellectual effort in economic science does not take place in literary passages. But did this enhance the communication among economists? To some extent yes. The more mathematics, the less one needs literary skills. It became easier to communicate beyond the cultural noise of languages. Bourbakian economics is free from the risk of becoming a 'tower of Babel'. On the other hand, what is the Bourbakian Esperanto of $x \in X$ good for if it is free from expressiveness? What is the clarity of language good for if disagreements become impossible? Heilbroner and Milberg, for example, argued that one symptom of the loss of vision is that economists are evermore indifferent to the views of others. The economic "discipline appears to be less and less (…) a matter of general agreement" (1995: 15). If everything one could say scientifically in market theory has already been said implicitly, how reluctant must an economist be listening to someone else? The drawback of the clarity of expression is the reluctance to engage in debates.

For the sake of clarity, let me oppose this argument against the superiority of mathematics as a language against another objection that is common in the philosophical commentary of economics.

> The desire to derive arguments rigorously means that they [economists] are confining themselves to saying what these theoretical tools allow them to say. Given the state of the techniques available to economists, pursuing this form of rigour has severely constrained what economists have been able to say – the models and theories they have been able to work with (Backhouse 2005: 383).

Although Backhouse's argument arrives at the same effect as mine, namely that rigour makes one forget about previously important questions, Backhouse does grant the theoretical tools of rigour expressiveness in some domain. By virtue of the narratives that have been assigned to these tools, rigour appears to be applicable to a *constrained range of problems* (we know the song: allocation instead of distribution, competition instead of industries etc.). Also Debreu has acknowledged this risk in his all-embracing philosophical naivety: "The very choice of questions to which he [the economist] tries to find answers is influenced by his mathematical background" (1991: 5). But the actual problem was not that rigour "constrained" the range of problems, but that it made the economist forget the problems.

Recall that in Bourbaki the axiomatic method meant less to enhance communication, but rather to stop the conversation in critical moments such as when 'philosophers attack them with their paradoxes' and Bourbaki 'brings out Chapters 1) and 2) on set theory'. Debreu does the same. The interview with George Feiwel begins with the question: "Why is the question of existence of general economic equilibrium so profoundly important?" Such was the question I posed to him in this chapter. Debreu had no sense for it. The question is too vague and marred with ambiguity: "Since I have not seen your question discussed in the terms I would like to use, I will not give you a concise answer" (Feiwel 1987: 243). The commitment to rigor, in fact, makes the economist insensible to the question of significance.

(4) *Free from Ideology*. Little by little the guiding threads of The Phenomenology of Economics begin to cross. The suspicion of meaning as it describes the formalism of Debreu is the draw back of the attempts to deal with the economic suspicion. In such way I have designed my narrative of economic science. Thus, is it not plausible that Debreu rounds up his speeches by celebrating his formalism as having freed economics from ideology? Did I not argue the same?

In which sense then, as he asserts, was "economic analysis sometimes brought closer to its ideology-free ideal" (Debreu 1986: 1266)? Debreu illustrates with (what else?) the interpretation of the two welfare-theorems:

> Foes of state intervention read in those two theorems a mathematical demonstration of the unqualified superiority of market economies, while advocates of state intervention welcome the same theorems

> because the explicitness of their assumptions emphasizes discrepancies between the theoretic model and the economies that they observe (Ibid).

Until the formalist revolution the political meaning of the welfare implications of GET have been debated in such terms. Since then, by and large, the discussion has calmed. But has the issue been resolved? Is economics free from ideology issues because it has resolved them "scientifically"? Did it establish an authority that all political parties agree on? Have economists ever been the political judges? Certainly not Debreu, although in his philosophical naivety he, too, displayed his dream of rigorous blackboard politics:

> The theory that we are discussing tries to be ideologically neutral. It deals with problems that are basic and common to all economic systems, for instance the efficient allocation of resources through decentralized procedures (…) Mathematical models of the economy help to analyse the optimal extent of this decentralization. The risk of misinterpretation (…) is lessened by the uncompromising exactness of the modelization" (in Feiwel 1987: 246).

The ideologically neutral 'decentralization of the allocation of resources'? What then is the 'optimal extent of this decentralization'? 65%? And is the optimization function "smooth"?

Matter is different. Recall that the interpretive indifference of the welfare implications of GET, which Debreu presents as the liberation from ideology, once in his youth was reason enough to warn the economist of the insufficiency of mathematical reasoning. Later, however, the same issue let him later speak cynically about the very possibility of such interpretations within economics. Economists are "carried by the current of ideas". Let me quote again the telling lines:

> [S]i cela satisfait les économistes libéraux et les marxisants, parfait! Je ne peux rien demander de mieux. Intellectuellement vous êtes *emporté par le courant des idées* et vous allez dans la direction où il vous porte (in Bini and Bruni 1998, e.a.).

'Marxian? Liberal? Both? *Parfait*! Economists can argue in favour of or against capitalism 'by simply reinterpreting the primitive concepts'. QED, economics is a science!

The confusion Debreu relies on is clear. Debreu did not *solve* a political problem by any epistemic means. He rather de-politicized economic science. What Debreu celebrates with the liberation from ideology is to be free from political relevance. Only in this sense economics is a science, and *not* in accordance with any standard of the philosophy of science, as I have suggested in this and all preceding chapters. Economics is systematic, yes, but not systematic *knowledge* in any positive sense. Debreu proved rigorously that the authority of rigour supports neither interpretation. GET is systematically beyond politics. What Debreu hurrahs – that economics is not (politically) biased – others such as Boulding began to hoot at – the fact that economics is (politically) irrelevant. The suspicion of ideology turned into the lament of insignificance.

But what then about the 'the risk of (political) misinterpretation' Debreu mentioned? Did he lower it? Was Debreu's discreet intervention really so sobering that it disillusioned all political associations of GET? There is at least one economist of the neo-Walrasian community who thought so: Frank Hahn. He was little more consequential in turning the formalist void of GET into a positive result. We are now prepared to understand the full irony of his "ju-jitsu" defence of GET, as Mark Blaug has characterized his move (2003: 152, 1992 [1980]: 164 ff.). Although Blaug agrees that "the best way *not* to learn how markets function (…) is to study general equilibrium theory" (2003: 154), he was not able to appreciate that this negative role can actually be *critical*.

> ...this negative role of Arrow-Debreu equilibrium I consider to be almost sufficient justification for it, since practical men and ill trained theorists everywhere in the world who do not understand what they are claiming to be the case when they claim a beneficent and coherent role for the invisible hand (Hahn 1974: 52).

GET according to Hahn, precisely in its axiomatized, and thus void form proves rigorously what one *cannot* argue with it. GET is critical about political misunderstandings. But the political misunderstanding of what? Of GET itself! With Debreu, GET clears up the misunderstandings that happened during its own tradition, and thus to a great extent the misunderstanding *of* this very tradition, namely that there is a positive claim to be made about the invisible hand, and furthermore that "the economy" can be an object of epistemic concern. Debreu showed, if I push Hahn's argument little further, that if GET ever was bestowed with meaning, this meaning did not stem from an epistemic concern, but from ideological motives. Debreu showed that GET as an economic theory can only be ideological.

Thus, was Debreu successful in the sense Hahn envisioned? Are all misunderstandings cleared up? After Nicolas Bourbaki, are economists back at the time of Nicolas Barbon? Not quite. It is true that the association of mathematical rigour with full determinability, and thus planned economy – that scientific socialism is outmoded. This happened, to say the least, no less due to McCarthy's violent politics than due to Debreu's sobering Bourbakism. But how about the other misunderstanding that was actually the initiating misunderstanding of invisible hand theorizing, namely to associate the intellectual elevation of economic theory with the political virtue of freedom? Is it not the riddle of post-war economics that despite of its inner critique to be politically irrelevant it came to be associated with a neoliberal advocacy of the market?

Thus is the suspicion of ideology really past? In the previous pages I came along such ideological infestation of formalism in economics at several points. The formalism of GET is only one slight step away from supporting a particular politics. This was the same basic misunderstanding I have located at the times of the rise of epistemic liberalism: scientific aloofness and discreetness – the distance one takes *to* politics – plays out *in* politics as a *particular* political program for *freedom*. What Poovey, Foucault and I said about 18th century liberalism could be said similarly about 20th century neoliberalism, as Mirowski, too, incites his readers to consider seriously:

> A mathematized world – say, a mathematized economy – by extension then also seems capable of policing itself, since it is being portrayed as existing independently of the way any analyst might characterize it, puttering along on its own terms (forthcoming).

This "by extension" is not a hair-splitting analogy of the relation of economists and economics, on the one hand, and politicians and "the economy", on the other – an analogy for which nothing but literary fantasy could speak. The key to this analogy is the insight in the historical initiation of an epistemic concern for "the economy" that *the economist* avoids being subjected to the economic suspicion. Only since Smith's scholarship epistemic arguments and economists' ethos are disassociated. The association of formalism and liberalism is as old as economics itself.

Again, for the sake of clarity, let me oppose this argument against that of Backhouse, who recently tackled the "neglected agenda" to inquire the secret alliance of scientific practices and neoliberal politics of the last decades.

> The conventional view is that the use of mathematics protects economists from ideology, as well as from being accused of being driven by ideology. However, there is another case that can be made. This is the intellectual value judgments that underlie technical economics, *as it currently exists*, bias one toward conservative conclusions. (…) Individual optimisation and perfect competition have been, for the most part, adopted not because economists believe them to be correct but because they permit rigorous analysis (Backhouse 2005: 382 f.).

Rigor biases neoliberalism? Why? Because, according to Backhouse, theories easily utilizable for neoliberal politics are *by chance* just the same as those that are easily amenable to the intellectual value of rigour. Other values and other technologies (for example simulation as opposed to axiomatics) could bias in another political direction. Are economists neoliberal by virtue of *analytic convenience*? Are they irresponsible enough to take the costs of supporting whatever politics for the sake of rigour? For the Sake of Science: Left or Right – Who cares?

No! At least this charge cannot be directed to Debreu. Debreu is not irresponsible in this sense. Backhouse does not consider that the same intellectual virtue of rigour can also evoke associations of transparency of GET, and indeed has biased for most of the time of the 20th century towards socialism. Rigour does not support a particular political ideology for reasons of philosophical taste. If Backhouse were right, I would have to charge Debreu with the following Marxian critique:

> Most of the orthodox modelling of the effects of NAFTA has been based on either some or all of the assumptions relevant in the construction of a proof of the existence of general equilibrium under perfectly competitive (Walrasian) conditions. (…) In the briefest from, this construct assumes that all markets clear (therefore, by assumption there is no unemployment), all products are divisible, there are rational maximizers of independent utility functions, all firms face competitive factor and product markets, all participants are endowed with perfect knowledge (costly attained), banking and finance operate seamlessly thanks to perfect knowledge of the future … (Cypher 1993: 153).

"Assuming the mantle of scientific objectivity", economists "introduce only those assumptions which enable modellers to 'prove' that Free Trade Agreements are mutually beneficial" (Ibid.: 146). But since all the assumptions are either wrong, or at least distortions, NAFTA is ill-founded. The Marxists Resnick and Wolff argued on the same grounds of assumptions that "in the award to Professor Debreu, the Nobel committee made a choice between the two traditions [class and non-class theories]" (1984: 30). Assumptions of "neoclassical" GET exclude the consideration of class. After the preceding chapter it should not be difficult to see that none of these "assumptions" Debreu ever considered, let alone spoke about. In short, the alliance between science and politics is not a matter of the philosophy that informs science.

Given Cypher's, Resnick's and Wolff's as well as the Nobel Committee's conception of Debreu's work, how could we share Hahn's hope that Debreu's formalism helped in clearing up the political misunderstandings of GET? According to what I argued in this chapter about his philosophical ambiguities, Debreu did both. He supported such misunderstandings in that he played with the confusions such as that of axioms and assumptions. But he also worked against them by insisting on the separation of mathematical form and economic content. The former is the result of little philosophical care, but the latter is the result of the anti-philosophical appeal of mathematical practices. Only there one can associate the separation of form and content with the separation of politics from "the economy". After mathematical form and economic content are separated economic content sneaks into economic theory through the backdoor without Debreu knowing. Is Bourbakism thus the last affective hallmark of the alliance between scientific monism and neoliberal hegemony?

The answer, of course, is not a matter of yes or no. The question is whether the Debreuvian economist could possibly take *social responsibility* for such association. Does economic theory allow economists to reflect on the ideological use of their work? As long as the self-understanding of the "Bourbakian economist" is to be prior, beyond, aloof, or in any case separate from political concerns, political meaning will be assigned to economists against their self-understanding. And therein lies the tragic aspect of Debreu's assertion that economics became ideology-free: precisely because Debreu felt to be free from it, others could freely find some murky ways to mobilize the aloofness of rigour as a symbol of the superiority of markets.

The phenomenological critique of Debreu's "oblivion" does not result in the charge of ideology! By not making any economic claim Debreu did not claim a truncated version of liberalism. He separated mathematical form and economic content as a way to avoid this association. Only in this way could he be successful! If one asks Debreu: Who are You – Arguing This! What could he answer? Would Debreu understand the question at all? Would it not simply be indiscreet? The critique of Debreu results rather in the charge that he is not responsive to such a question. The problem of economic (pause) science is not a particular ethos of economists, but the diminishing of their ethos.

The tragedy is that Debreu, after he received the Nobel Prize, was indeed confronted with the misunderstandings he caused. Debreu knew exactly that the ideology issue was not closed. He felt it under his own skin after receiving his honour. He was addressed as an authority of meaning, not as an authority of structures. For nobody in economic talk was ever interested in structures! Here is where the infestation of "applying Bourbaki" ultimately took place. In the embarrassment of not being able to live up to the ethos of a Nobel economist my affective history of his intellectual life ends.

After 1983, Debreu was held responsible for the misunderstandings the Nobel committee revealed, and which Debreu, as opposed to Hahn's hope, could not clear up. The economic suspicion – this makes Gerard Debreu a tragic character – was thus not made silent, but in the opposite, was reinforced by the appearance of being free from it. Are the cultures of economic suspicion, after all, not reinforced by the presence of those who pretend to be beyond them? After Debreu had avoided political questions for his entire life, following 1983, they befell him with the unbearable weight of the Nobel ethos: Mister Debreu, so he was asked by the entire world: *What does that mean*? What does it mean that you have proven that "the market works automatically"? Should we position more or less rockets toward East? With such penetrating questions I close the affective biography of Gerard Debreu in the next chapter.

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Let me close the preceding methodological considerations with a disturbing quote from Bourbaki. The problem of the axiomatic method, I argued in length, is *not* of a philosophical nature. The problem is rather that it downplays, if not excludes, intellectual ethos. The problem is *ethical*. When Dieudonné alias Bourbaki replied to the question how the "application of mathematics to something of a different nature [reality]" is possible – which is the philosophical riddle of the axiomatic method – he replied:

> Why do such applications ever succeed? Why is a certain amount of logical reasoning occasionally helpful in practical life? Why have some of the most intricate theories in mathematics become an indispensable tool to the modern physicist, to the engineer, and to the manufacturer of atom bombs? Fortunately for us, the mathematician does not fell called upon to answer such questions, nor should he be held responsible for such use or misuse of his work (Bourbaki 1949: 2f.).

At least we know that Bourbaki alias Claude Chevalley would not have agreed with Bourbaki alias Jean Dieudonné.

(6) Debreu's Retreat after 1983

In the speeches Debreu held after his Nobel Prize, there are some slight signs that he felt uneasy about the indirect effects, the unintended consequences, and the misunderstandings surrounding his work. After he passed his 50s, he showed some awareness of the ambiguity that accompanied his life – in two words, "applying Bourbaki".

Debreu's Last Corollaries about the Surprise, Regret, and Encumbrance Wrought by the Invisible Hand of Formalism.

Notably, he showed this awareness already in his Banquet speech. After he had shaken hands with the King of Sweden, and expressed his gratitude to the Bank of Sweden for the Million Crowns, in the few words he had chosen for his banquet speech he reflected on his peculiar position in post-war economics. In order to explain the success of mathematical economics, he used the metaphor of the invisible hand. He himself thus draws the analogy of the relations between economists and their theories on the one hand, and between those conducting an economic life and "the economy" on the other – as though now the economist came to feel the character of his own theories on his skin.

> (A) scientist knows that his motivations are often weakly related to the distant consequences of his work. The logical rigor, the generality, and the simplicity of his theories satisfy deep personal intellectual needs, and he frequently seeks them for their own sake. But here, as in Adam Smith's famous sentence, he seems to be 'led by and invisible hand to promote an end which was not part of his intention', for his personal intellectual fulfilment contributes to promoting the social interest of the scientific community. (…) It was my great fortune to begin my career at a time when economic theory was entering a phase of intensive mathematization and when, as a result, the strength of that invisible hand had become irresistible (Debreu 1983b: banquet speech).

Debreu's use of the metaphor of the invisible hand has subtle connotations. They sum up the terms with which I have discussed the role of mathematical reasoning in post-war economics.

With the metaphor of the invisible hand, Debreu, first, admitted that the primary concern of his intellectual life was less the "social interest of the scientific community" (whatever that may be). Instead, he was engaged with mathematical economics *for its own sake*, lost in the aesthetic appeal of rigor, rigor, and nothing but rigor. Like the baker man in the Smithean market, the economist pursues rigor only for his own interest – for the intrinsic appeal and pleasure of the mathematical experience. Although the mathematical economist is irresponsive to the social task of the economic profession, he, mysteriously, 'led by an invisible hand', turns out to promote it. Debreu therewith reacts to the common critique of post-war economics, to the mourning that mathematics has become an end in itself, is overused excessively, and leads economics to the edge of irrelevance – just as once the clergy lamented about the use of money, to which economists replied with the invisible hand.

What could Debreu have meant by the "social interest of the scientific community" other than the domination of economic talk by epistemic concerns on the one hand, and the participation of economics in the hype of Big Science on the other? Did he ever ask himself during his intellectual life whether such domination is socially desirable? No, he was only concerned with his deep personal intellectual need for logical rigor. Debreu was always rather shy in claiming for himself the institutional power of economic science. At this evening, in a moment of celebration in front of the Swedish King, how could he not show trust in the beneficiary results of the epistemic culture in economic talk?

With the metaphor of the invisible hand, Debreu acknowledged, moreover, the "irresistible" influence mathematical economics had on post-war economics. Since his intervention, the profession has moved ever further away from being able to connect to a pre-Debreuvian way of intellectual life. Whatever the intellectual motivation, the economist ends up reinforcing the current state, as if one engages in economics only for the sake of the mathematical value of rigor. Debreu thus acknowledged the tragic aspect of the influence of mathematical economics: that attempts to provide a theoretical alternative only reinforce the incontestable social status, and the discursive closure of economists.

With the use of the metaphor of the invisible hand, Debreu, thus, admitted that his influence went far beyond his own intentions when entering economics. Debreu was surprised by his success. Hence, I could interpret the use of this metaphor also as an "apology" not in the sense of a defence of mathematical economics, but as an *excuse* for having caused a misunderstanding about mathematical economics. Did his Banquet Speech not have an undertone of, in Hahn's words, 'Sorry – 'This is not what I meant, this is not what I meant at all'. His methodological defences of the axiomatic separation of mathematical form and economic content could likewise be understood as such an excuse for having caused a misunderstanding: Sorry, dear economist, mathematical reasoning does not suffice in economics. Mathematics is not All There Is.

With the metaphor of the invisible hand, Debreu did both: He defended the importance of mathematical economics because it has beneficiary results for the profession beyond the theories it is applied to; and he excused it, showing that it is insufficient for any economic theory. With his defence, he thus acknowledged the pervasiveness of mathematics in post-war economics, while with his excuse he acknowledged its irrelevance. It was in this sense that Debreu cast a "bright shadow" on post-war economics. It is not trivial to know whether one stands under his sheltering and threatening influence or not. Debreu acknowledged with the invisible hand of formalism both the success and the unpopularity of mathematical economics. Between guilt and defence, he felt the ambivalence of "applying Bourbaki".

Most other economists in the early 1980s would agree that something happened to the profession, which nobody ever really wanted. Recall the AEA presidential addresses held in the 1970s (Leontief, Gordon, among others). Obviously, Debreu knew about them and could not avoid referring to them when he, already in his 70s, came to hold his own presidential address in 1991. He acknowledged that the critique of the 1970s was and still is important, yet that he did not believe such critique could be effective. Leontief's and Gordon's speeches were

> relevant when they are made in 1970 and in 1975. They still are today, for, in spite of their authorities, enhanced by the platform from which they were speaking, and in spite of the wide diffusion of their critiques, neither Leontief nor Gordon altered the course of the development they were assessing (Debreu 1991: 6).

Why could Gordon and Leontief not change the course of economics, according to Debreu? Debreu referred to the intellectual values of the profession, and acknowledged the conflict with the intellectual values of the mathematics department. In his presidential address, Debreu showed considerable doubts about the beneficiary results of the invisible hand of formalism. Now he acknowledged that mathematics pursued for its own sake also has undesirable consequences.

> (e)ssential to an attempt at a fuller explanation [of the success of mathematical economics although nobody really wanted it, T.D.] are the values imprinted on an economist by his study of mathematics. When a theorist who has been so typed judges his scholarly work, those values do not play a silent role; they may play a decisive role. The very choice of the question to which he tries to find answers is influenced by his mathematical background. Thus the danger is ever present that the part of economics will become secondary, if not marginal, in that judgment (Debreu 1991: 5).

That the questions tackled by the neo-Walrasian community stem from mathematical structures rather than from economic intuitions, I have shown sufficiently in the previous section. Mathematical values are not only silent, they are exclusive: rigor, rigor and nothing but rigor – economic meaning is marginalized. While engaging in the practice of rigor, other intellectual values are absent. The economist has to forget them *in order* to be rigorous.

Debreu acknowledged this oblivion in his later years. He even shows some soft signs of guilt. Guilt is one of the connotations an invisible hand argument can have: If social reality is the result but not the design of human action, we can be guilty for all the misery of all world without even being able to take responsibility – terrible. In age, Debreu developed a soft sense of having made a mistake, perhaps even a sense of remorse. It was as if a conformist realized he had unwittingly caused a revolution. He was most explicit in the last lines of his *Frisch Memorial lecture* in front of the *Econometric Society* in 1986.

> In its pursuit [of mathematical economics], research may be *tempted to forget* economic content and to shun economic problems that are not readily amenable to mathematization. I do not intend, however, to draw a balance sheet, to the debit side of which I would not do justice. Economic theory is fated for a long mathematical future, and at other World Congresses of our Society Frisch Lectures will have the opportunity, and possibly the *inclination*, to choose a theme 'Mathematical Form vs. Economic Content' (Debreu 1986: 1268/9, *e.a.*).

Debreu may have felt sorry. For he could not do justice to what others forgot because of him. Speaking of the 'temptation to forget' and 'inclination' to lament, Debreu came close to what I explicated along the oblivion of the life-world. There is a *temptation to forget*, which lies in the very nature of the mathematical experience and its suggestive force – rather than in a philosophical position about mathematics. Just as Debreu was tempted to make an economic claim when entering economics, but could not, the economist is tempted to forget economics in light of mathematics but cannot, as economist. This uneasiness of one's project in light of one's experience is the last affective trace of the intellectual responsivity of economists after all philosophical awareness, historical consciousness, and theoretical expressiveness faded away.

And What in God's Name Did The Pope Want from Debreu?

Something must have happened to Debreu after 1983. Did he at last face the dilemma of "applying Bourbaki"? In his public speeches, he does not display any more signs of self-critique than those quoted. According to my phenomenology of his intellectual life, however, we should not be surprised if Debreu had greater difficulties than those visible in public. After he already half-heartedly withdrew from the stage of economics, how could he live up to the Nobel ethos? How did he deal with the publicity he earned? From one day to the next, he stood in the spotlight of the world *as an economist*. How did he deal with the misunderstandings that lines like 'Debreu proved that the market works automatically' necessarily must have evoked? What should the layman, the journalist, the politician, and other 'second-hand dealers of ideas' think of Debreu? The unease Debreu must have felt when being addressed with the penetrating questions of such people concludes my affective biography of Debreu.

Clearly, Debreu received the prize contrary to his own self-understanding. The irony of the Prize was that although he himself discreetly separated mathematical form and economic content, he could only enter economics by causing an equivocation of mathematics and economic scientificity. Thus, again, after 40 years of being half-heartedly economist, he had to clarify the misunderstanding he had always avoided facing. Now there was no authority to help defend him against the rest of the world. Now he himself was addressed as an economist. Debreu was confronted. He was confronted by those people who he would never thought could possibly be interested in his work, who have not the slightest concern about the scientificity of economics, but exclusively about the question that Debreu always carefully and rigorously avoided: What Does That Mean?

> The world's expectations didn't help either. Suddenly because he had done some remarkable work in Economics my father was contacted by politicians, political activists, physicists and scientists in other disciplines and even by the Pope (de Soto 2005).

Political activists? The Pope? What did they want from Debreu? Without knowing the answer – John Paul's letter may be explained by the *Pontifical Academy of Social Sciences* that he has launched with Arrow – we can imagine that Debreu's answer was a string of excuses: No, sorry, I did nothing that could help you in your mission, John Paul! We hear the slight nerviness of being addressed in that way in his opening words at the press conference at Berkeley University following the announcement of his Noble Prize. 'I do not want to discuss my views on the Reagan Administration's economic policies' (in Hayes 1983).

Debreu was in the spotlight of that world, in which he always tried to keep a low profile. The world that granted him the prize was the same world he knew only from primitive concepts that he immediately substituted with mathematical objects. How then did he deal with this sudden honour? How was it possible to meet the expectations that now rested on him?

> But from that day forward I believe he never felt he could live up to the honor that had been done to him. His esteem for his own work did not match the high esteem that others had put upon it (…). So he examined everything from then on through the lens of 'Is this worthy of Nobel quality work? (de Soto 2005)

Did Debreu finally face the dilemma? No. As he did not solve it in his young years, he never acquired the intellectual ethos to deal with it later in his life. His only reaction to the feeling of having made a mistake was to go back to the desk and check again whether it was rigorously rigorous. He never learned to deal with mistakes in any other way. But what could he find in his articles? The errors he may have found probably made things even more severe, while all the beautiful proofs only proved that he "has nothing to say", either to The Pope, nor to anyone else – and increasingly less to his closest fellows. What a healing effect it would have had for Debreu, for the profession of economics, and for his family, if he had only refused the Prize!

> It was from that time onward that I saw my father withdraw from us. He was unwilling for any of us to see him as less than he had been judged in that brief shining moment in Stockholm. He could not live up to the myth that had been created around him. We deprived him and he deprived himself of his humanity, of his right to be flawed (de Soto 2005).

Implications

After having marked some of the coordinates of the horizon from which the practice of economic science arises and in which it takes place, the reader may have the impression that the matter came close to a "theory of economics". Some readers may wish to see this implicit theory conceptualized or even operationalized more rigorously than being applied in a haphazard socio-historical narrative. But this would miss the character of the text.

Before coming to hastily wrap up the preceding material in a couple of lines, I thus would do well to recall one of the mottos of Husserl's phenomenology: 'World is not a being'. Or, phrased differently, 'life-world cannot be reduced to world'. World is horizon. If there is a philosophy of phenomenology, it is this idea of the involvement and enmeshment of the reality of experience: that this reality affects and is always a step ahead of our epistemic efforts that try to calm the lived world in a claim or a judgement. With the notion of the life-world, I referred to precisely this: the inhering of intellectual life in what it comprehends.

According to such philosophical intuition, to conclude is rather to show the implications than to sum up. The Phenomenology of Economics needs to be judged on what it discloses rather than on what it pins down. In order to do justice to this disclosing character, I must be able to turn the basic tone from critique to invitation, and from pessimism to optimism. Intellectual life according to this philosophical intuition is an ardent, devoted, constant, patient, and, yes, even a festive activity. Such pathos of intellectual life shimmers through all the limitations it suffers when subjected to the corset of authoritative claims – to science. It is thus now the time to turn over the critical tone to the optimism underlying my skepticism.

What did I promise? The preceding exercise was meant to be prolegomena for a renewed sensibility in economic writings. How did I show the possibility of such renewal? I did not do so by means of a phenomenological theory of economics, but by addressing the necessity of economists as those who accomplish economics, and therefore address a sensibility that is "forgotten" as long as one is gagged by science (though not willingly – no one forgets on purpose). To "remember" that sensibility meant to disclose the horizon from which intellectual efforts in economics gain their forces. To do so, to use the philosophical jargon, is nothing but to carry out a phenomenological reduction on the transcendental constitution of economic science. Only when considering this character of my text can we arrive at the point at which we can appreciate the liberating aspect of my core critique – namely, that that the practice of economic science is possible only as the oblivion of the motives that gave rise to it.

There certainly could be other phenomenologies of economics with perhaps less emphasis on the difference between phenomenology and economics – here their attitudes have been diametrically opposed; another could be less historical and closer to the philosophy of science; another closer to the writings of this or that economist or phenomenologist. The one at hand was written *for* the economist and *against* science. Accordingly there are two concluding tones, a pessimistic tone about economic science and an optimistic tone about liberation from it. I first sum up why economic science is phenomenologically unbearable in order to then move to the optimism for a renewed sensibility for economic life – at least for the sake of social responsibility in economic talk.

Pessimistically Speaking, Economic Science is Insignificant – Necessarily

The most least explicit object of concern of the preceding exercise was economic talk. It provides the discursive and historical environment of economic science. What economic talk consists of is rendered rather blank throughout the text. I did not say more than that it is affected by a notion of economic life as characterized at the beginning of the second part.

The main patent concern was rather the scientific attitude that economics demands: the distance one takes and the aloofness one adopts toward economic talk in order to claim epistemic authority. How do economists distance themselves from the rest of economic talk? How do they demand to be listened. What is their relationship to their audience, to their profession, to their discipline, and to their past? Such questions circled around the notion of the *ethos* of economists. With the notion of the life-world I have promised an inquiry into the transcendental constitution of this ethos. The ethos of economists, phenomenologically speaking, is the locus of intellectual responsivity and, moreover, the condition of the significance of economics. What then is the result? What describes the ethos of the economist?

If I had to draw a conclusion on the implicit image I presented of the "average ethos" of economists – on the *what-kind-of-people* question – then it would be that economists tend to be something of "moderate reformers". They are moderate because they do share, in some way or another, the discreetness of Debreu. One never could make a career by calling out for revolutions. Neither the red nor the black flag was ever hauled up in the institutions of economics (liberals were labeled anarchists in the 1920s). It was rather the white flag on which was written: Please Don't Ask: Who am I – Arguing This! Economic science, for the most part, was a defensive project.

But economists always maintained their reformative spirit since, contrary to Debreu, they never completely gave up the belief in the expressive possibilities of their science. They continue believing that the theoretical perception of "the economy" *has* a meaning – be it the production and consumption of wealth, efficient resource allocation, information processes of a complex society, or whatever goes through one's economic mind. I would even presume when speaking to an outspoken neoliberal economist that he or she never entirely gave up the belief that the market *is* a political institution, and moreover that economic life *is* a concrete part of life and does not pervade all of it. How else could one maintain one's self-perception as an economist?

While holding on to their belief in the expressiveness of economic science, economists also believe that they are entitled to provide some economic services that are meaningful in a specific social and historical context without being mere expressions of special interests. Most economists may agree with Martin Bronfenbrenner "that academic life can be more than a consolation prize for not 'mattering' in the Real World. But also, that racial, religious and ideological quotas made Ivory Towers difficult to climb" (in Szenberg, Lamrattan 2004: 97). Economists are fairly "moderate reformers", who believe themselves to be somewhat aloof to the pragmatic and all the more moral naiveties of economic talk, and for this reason avoid being too literary in their writings.

In the course of the preceding remarks, I did, of course, encounter some economists who indeed deliberately used scientific authority in order to *justify* special political interests. If such economists have a strong mission, however, they show it openly and do not hide behind scientific authority. In these cases, if there is anything to reveal, it is rather trivial. If you do not like the Cato economists, then launch your own think tank. The Catos of this world came here to argue, and they need to be argued against. In more general terms, as long as the world that informs a piece of economic theory is in some way associated with the world that it envisions, it is sufficiently vulnerable to other economic talk. It should count as intellectual honesty if these writers were to oppose science and promote a partisan scholarship. Friedrich Hayek and Deirdre McCloskey are friends in this respect. Both believe that "Smithean" scholarship was and still is possible – I had only wanted to intervene in the party when Hayek flirted with von Neumann, and equally had called back McCloskey when making theory admissions to Arrow (2000: 224).

In the other case when science is indeed used as a tool to *serve* politics in one's own interest or that of the politicians who pay, that is, in the case of *social engineering*, it is the task of the discursive environment to unravel these intentions. For this purpose the work of historians and science studies are important for the operation of democracy, at least to the extent that they pose the critical question: Who Are You – Arguing This? Here, also, economic discourse beyond scientific authority is possible. Google "von Neumann" and "Target Committee", and every single layman should have something to say about the application of GET to the "efficient allocation of resources".

Be they moderate reformers, partisan scholars, or social engineers, the preceding exercise went further than asking what kind of people economists are. The point was not to *determine* the ethos of economists and their social significance, but to consider the diminishing role and contestability of any ethos. The degeneration I exposed referred to the increasing irresponsivity of the ethos of economists to both their discursive origins and effects. Whatever the motives of the economist, it is difficult to live them out; whatever are the discursive effects, it is difficult to take responsibility for them. The incontestability of the scientist overshadows economists' social character. Simplistically stated, if a motive for gaining this or that ethos in economic talk is "to be on the good side" – whatever that side is – economic science will not be very helpful, since it does not reward such motives. For economic science does not reward any motive that stem from economic talk – necessarily. To the contrary, one will more likely become indifferent to the question of who is on the good side of economic talk. Why?

The notion that emerged as the key to this argument of the diminishing ethos was *the economic suspicion*. Science was operational for liberating economists from this suspicion. This is the great attraction of the elevation of science. Economic science developed as that discursive practice which was able to *avoid* and to *divert* from the economic suspicion by means of arriving at a level of reflection that is beyond the question: Who are You – Arguing This! The scientification of economics happened as a series of *manoeuvres of diversions* from this blame. For this reason I stated a tendency toward formalism, in particular but not limited to mathematics, as well as to structuralism, in particular but not only in GET. Formalism as the title of lowering one's tone, and the invisible hand as the title of saying less both stand for the withdrawal from the culture of the economic suspicion.

The definite result of my narrative was: Yes, economics was successful in these manoeuvres. Yes, economic science came to be. It is there, fully situated within the institutions of science. The devastating result was, however, that this achievement had its drawback. The economic suspicion was the motive for the formation of a discursive identity of economic scientists, but at the same time accounts for the degeneration of this identity. Economists overcame the imposition of economic motives, however, only with the reversal of being haunted by another suspicion, the *suspicion of meaning* – that claiming anything must be beyond scientific authority.

This reversal I have traced along the genetic code of economists' ethos: scientific modesty is instituted as an opportunity to gain attention in a world where there is nothing but moral clamor. This modesty turned into aloofness and further into discreetness rather than scholarly respect for matters of concern. This discreetness applied not only to the heated concerns of the others, but also to one's own concerns, which, after all, made economists forget their own motives for engaging in science. As a result, irony and cynicism about the belief in the worth of one's effort is systematically induced. In this sense, economic science is the liberation from the economic suspicion, and has its drawback in the liberation from the weight of meaning. For this reason, economics, after the formalist revolution, continuously evoked jeers of irrelevance. If the very fact of meaning – its weight, as I said – is experienced as a limit to science – which I indeed stated for the case of Gerard Debreu – then a "scientific claim" turns out to be an affective and existential acrobatic maneuver beyond phenomenological imagination.

The case I made against science is clear: Economic claims cannot be made with scientific authority. Scientification required becoming insensible to all possible sources of meaning bestowal for economic theory. Economists have never managed to separate their (political) bias and their (political) relevance. Such a distinction is only possible by excluding all meaning from the structure of economic theory. This, however, constitutes such a *rupture* that it is impossible to maintain one's self-understanding as an economist. In this sense, the scientification of economics is at the same time the degeneration of the economic claims possible in science. As a consequence, economics enters its discursive surrounding only as a result of "misunderstandings" for which no economist is responsible. The locus from which economists speak is always about to lose itself in either irrelevance or mere talk beyond science. The 'seat of economic science in life' is thus inherently fragile – has, as it were, only one foot – namely, the misunderstanding that saying less with a lower tone suffices as authority. A light shove, as I promised, is enough.

The *rupture* represented by economic science is not of a philosophical, but rather – though I have carefully avoided this term – of an existential kind. In their theories, economists cannot tell by any means about their motives for theorizing. Economists have to theorize at the edge of irrelevance, and so at the edge of not understanding oneself in ones practices. Like in the case of the petition of the French students, there is a clash between one's economic interest and the demands imposed on its expression in the name of science. The interest of economists is the *supplement* of their pursuit of science. Economists are the blind spot of their own universe of discourse, the trace of their commitment to science. As long as these motives are that through which economics can possibly "refer back" to the life-world, my pessimist tone in its most propounded version is: *the phenomenological conditions of the possibility of economic science are the condition of the impossibility of it significance.*

My pessimism cannot be stronger. When I reverse this argument, however, we gain a feeling for its constructive implications. If economics became insignificant by avoiding the economic suspicion, then the economic suspicion is that instance which makes economic talk significant: *the economic suspicion is the condition of the possibility of intellectual responsivity in economic talk*. An expressive intellectual practice is inconceivable without the ambiguity of (political) bias and (political) relevance – that is, only as long as one is vulnerable to the economic suspicion. If I said that the life-world is the world through which an intellectual interest is possible, the economic suspicion functions as this *demand of meaning*. The economic suspicion, in its threat for discursive identity, in its ability to bind all economic solutions, judgments, and claims back to their motives, denotes the very possibility for economic *reflection* – reflection as opposed to the abstraction from the economic suspicion. If reflection means for Husserl being able to ask at each point 'What am I truly seeking?' then this question in economic talk translates to 'Who are You – Arguing This!'

Life-world, I said at the beginning, is the locus where the problems come from. The economic suspicion is constitutive of the "having" of a life-world when engaging in intellectual efforts invoked by economic life. To face the economic suspicion is to acknowledge a level of discourse that always remains to be considered and that never ceases to give us to think. It keeps an economic claim from being forgotten in the archives of truth. The life-world is not the world from which science *abstracts* and later comes back again. This is the empirical world. The life-world, as I announced at the beginning, is not the world that is *left* to be considered, but it is the world that *demands* to be considered. It is the experienced world of the scientist that lets him dither about the success of science, and makes him vulnerable to the question Who are You – Arguing This. It is there that sensibility in economic talk comes from. From this point on I can turn my pessimism into optimism.

The Phenomenological End of Economics and the Diminishing Need for Economics Departments: The Waning Love of Economics and Science

Let me dwell further on this reversal in order to appreciate its implicit optimism. First, which I suppose to be the greatest obstacle for any economist in order to appreciate my conclusion: in what sense did economic science come to an *end*?

What exactly came to an end? Economic theory? No. Innovations of its scientificity? No. Although my historical narrative was phrased along a seemingly methodological and theoretical issue (formalism and the invisible hand), neither issue could let me conclude on the end of economics. Formalism as well as structuralism are titles of the ethos of economists. Formalism was the title of the search for a level of reflection beyond special interests, while the invisible hand was the title of the cultivation of the theoretical perception of "the economy". The problem of formalism and the invisible hand is not a matter of a feature of theory erroneously developed in the history of economic thought. Rather, I have discussed them as a matter of the very possibility of economists' scientific ethos. In this sense, the formalist revolution of the 1950s was not only a blunder that never happened before and will never happen again, but showed something of the constitution of economics as a science.

In what sense, then, have I argued that economic science has passed the peak of its life cycle? What exactly came to an end? The time when economics made sense in its environment (the historicist point)? Or the time when economists were deceived on the contradiction of economics (the philosophical point)? Neither. For my notion of the *oblivion of the life-world* was a historiographical guide *beyond* the history and philosophy of science. But did I really avoid the ambiguity between history and philosophy?

(a) Did I not present a historical argument in the sense that the life-world qua *culture* was moving ahead of the institutions of economic science? Perhaps once economic talk was arranged in such a way that there were plenty of opportunities to claim scientific authority, above all during the century of the battle of ideologies, but today increasingly less? Did I not argue that economic science, given our present times after 1945, is no longer an appropriate institution? Are economics departments remnants of modernity in an increasingly "postmodern" world beyond the triad of science, growth and technology? Did I not suggest that one no longer could believe, today, after 1945, after the fall of the wall, after the fall of the two towers, in the scientific engineering of freedom? The drawback of the belief in science, which I illustrated with Gerard Debreu's life – is it not a symptom of the end of modernity as observed everywhere in Western culture? Is economics thus outdated because it can no longer be critical for the present culture? Was it appropriate perhaps one or two centuries ago, but not today? Time passed, and with it the time of economic science?

(b) Or did I present a rather essentialist and teleological narrative that economic science was ill-founded since its beginnings? Did I not argue that the conflict between political relevance and political bias was already present at its beginning? Did I not show that at each point in the modern history of economic science (since the epistemic revolution in the 17th century down to the Bourbakian trauma of the 1950s) there is a latent conflict between the interest in an economic claim and the commitment to science? Did I not argue that already the *Urstiftung* of economics entailed the germ of an instability that could only be hidden as long as one diverted too-indiscreet questions? Did I not argue that Nicholas Barbon and Nicholas Bourbaki merely represent two sides of the same misunderstanding: the misunderstanding that referential truth-claims about "the economy" are possible? Did I not suggest that economic theory was doomed anyway to dissolve into the void of the axiomatic method? Was it not only history that blurred the contradictory reality "of" and *of* economics, which finally in the formalist revolution could become evident? Was not everything already predetermined from the outset?

These two arguments are neither plausible nor intended. The historiography of the oblivion of the life-world was designed in order to avoid the choice between these alternatives. Reducing the narrative to one or the other would miss the *transcendental materialism* of the oblivion of the life-world. It is true that I stated a steady history of a misunderstanding that began in the 18th century and later multiplied, reproduced, and developed a life of its own. It is true that I stated a, say, Serresian association between the 17th century cynicism of giving up the pretence to do all for the mere honor of the Kingdom, and the 20th century cynicism of letting all humanistic laments crash at the wall of formal analysis. Yes.

But this does not impose an essence or undermine the historical reality of economics by any means. Stating a misunderstanding that made possible the contests over the epistemic character of economic talk of at least one century, if not three, is neither to reduce matter to philosophy nor to history. If there were an essence of economics, then these historical renewals of the practices that made it possible to hide the fragility of economics and thus to evolve in a rich history. The oblivion of the life-world accounts for this historical richness rather than subsuming it – which is the philosophical failure. And neither does it subsume this history under a specific historical a priori of meaning, after which there could be simply another phase of epistemic culture in economic talk – which is the historical failure.

As long as I focused on the contests that constituted economics rather than the contest about the true essence of economics, my narrative thus presented both the rise of the appearance of an essence in economics as well as the rise of the difference between economics and the socio-historical realities that surround it. My narrative was thus transcendental in the sense that it asks for the conditions of the possibility of the identity of economics. But it was material in the sense that these conditions are rooted in nothing but the concrete practices of economists as the attempts to appropriate the socio-historical environment. Speaking of the end of modern economic thought means that these contests no longer trigger a deepening of the modernist belief in science. The basic tension between the reality "of" and *of* science is no longer productive, but, to the contrary, befalls and works against scientific optimism in economics.

If my tone seemed radical, then it was because I did not argue against science *in light of an idea of true science,* as all modernist critiques did, including Husserl. I did not make a plea for a new economic science that could emerge after economists began to reflect. I ventured that such a reflection would be a move away from *any* image of true science – *whatever* epistemic principle. There is certainly a lot to say about the future of "technocratic empiricism", of neuroeconomics, or about the future of heterodox economics. But there is no reason to believe that that these innovations of the scientificity of economics will strengthen the discursive identity of economic sciences. Rather, I would expect that they contribute to the loosening of it.

I have suggested that there is no reason to expect another, third wave of scientific optimism in economics. Which political vision (in Heilbroner's and Milberg's sense) could nourish this scientific optimism? Commercial freedom, freedom from social miseries, and other forms of the modern liberation of man – who is willing to invest such visions into a renewal of scientific optimism in economics? Recall Hayek, who in 1949 called for a new vision of liberal intellectualism:

> [W]e must be able to offer a new liberal program which appeals to the imagination. We must make the building of a free society once more an intellectual adventurer, a deed of courage. What we lack is a liberal Utopia, a program which seems neither a defense of things as they are nor a diluted socialism, but a truly liberal radicalism (1949: 384)

In the last six decades since Hayek's call, have there been any steps taken within academic economics toward such a renewed vision, be it a liberal or any other hope?

As radical as this anti-scientism seems, it is not great news. Many of the political implications regarding the institutions of economics have been drawn more or less explicitly many times. Most economists share a general skepticism about science in the same sense as they know that being *merely* an economist does no good to their intellectual life. Is not *not* being *merely* an economist *the* new intellectual virtue after Debreu? Economists share the perception that economic theory never does the actual work of arriving at a claim, for which one rather needs intuitions called on to be "sociological" or "psychological" or any other "interdisciplinary" ally economists have taken since the 1970s. Among many others, Heilbroner and Milberg conclude similarly that economics should be a sub-discipline of other social sciences which "follows in the wake of sociology and politics rather than proudly leading the way for them" (1995: 126). Is this not, even if secretly, common sense among economists? If so, let us get serious about it.

The commentary of economics agrees. Here, for example, is Colander on the teaching of "New Millenium Economics":

> [The] increased specialization has been accompanied by a redefinition of boundaries of graduate economics programs within institutions. In the 1990s, firm institutional boundaries existed between public policy schools, arts and sciences schools, engineering schools, business schools, law schools, and medical schools. In 2050, these boundaries have broken down. Most of the existing specialities that comprise economics evolved of a combination of schools or programs within schools (…) In fact, one might say, that in 2050 there are no longer 'economists', but, instead, health economists, statistical specialists, simulations experts, who focus on economic issues, public finance specialists, and so on (Colander 2000: 124)

Economists from economics departments, according to Colander, will lose their power in other departments, where economic scientists no longer teach the so-called "economic aspects" of social life. Economic scientists, anyway, never had authority over the meaning, let alone scope of the "economic aspects" of life. Economic theory will increasingly be replaced by the economics that stems from outside economics departments, and, peu à peu, the very notion of an epistemic economic domain vanishes. Weintraub agrees:

> In the major American research universities, economic statistics has migrated to departments of statistics, research and teaching in economic policy are now moving to departments and schools of public policy, political economy has moved to political science departments, and managing studies are migrating to departments of management and of sociology (2002: 12).

To which I should add philosophers of economics moving to the philosophy of science department, and historians of economics moving to the history of science department. Let the commentary of economics not be the backbone of the remnant skeleton of economics otherwise falling into itself – a blame to which the present exercise should be immune.

Consider again what identifies economics departments today. Economics of (rational) choice, for example, can be and is indeed conducted just as well at the psychology or sociology department. If it comes to the market, the actual determinants of choice do not matter anyway (take the Bourbaki lesson!). The methods of economists too – be it game theory, experimental economics, or complexity studies – can be and are used by all social sciences. Notions other than choice that represent the research of economists simply do not warrant a noteworthy profile in economic talk – such as institutional change, emergent properties, information entropy, prospect theory, let alone oxytocin when speaking of trust. Who recognizes economists in such ideas? Who would want to maintain economics departments on the basis of such notions? The research done at economics departments could increasingly be done just as well at other departments. One could design – in order to arrive after all at a real *hypothesis* – an index of the materialism over which economists compete with other sciences. My hunch would be that there are diminishing sources on which economists exclusively rely.

Or consider the notion of (dis)equilibrium, which is *the* homemade concept of economic theory of the 20th century. Has economic theory without equilibrium not been the desire of economists since 1954? But is not this desire only held secretly, because economists know that they would lose their discursive identity if they were to give up this notion entirely? Given the negative closure the rejection of equilibrium economics provides, is it not time to face up to the consequences of giving up the notion of equilibrium? Backhouse (2004) tried to rescue the notion of equilibrium in its opposition to history by arguing that "equilibrium is one of the many tools that can be used to understand history" (303). But what if (dis)equilibrium theorizing has the inherent tendency to make the economist forget about this "tool"-character, as I argued? Is not the lesson to be drawn that one should recognize that economic reason (for which the notion of equilibrium stands) cannot be anticipated by a theory, but needs the intellectual care that is only possible within history?

Consider, conversely, economics departments from the outsider point of view. The concepts that identify economics are less and less reflected by the practices of economists. Are there any academic economists left who actually exert epistemic authority in favor of the (largely neoliberal) policies that are associated with economic theory? Did such economists not to a large extent move to think tanks? The time of the scientific engineering of liberty has passed. The notion of freedom becomes public good in all economic talk, and there are many signs that the liberal tradition in its neoliberal shape loses its discursive monopoly over it. Who, in Western countries, would still want to rely on scientific optimism when claiming authority on the meaning of freedom? Would that not be one of the most liberating moments for the modern history of economic discourses, let alone Western democracies, if "freedom" became contested rather than violently instrumentalized?

The authority economists rely on, therefore, increasingly does not stem from their own science. Whatever one intends to argue for, whatever is one's discursive interest in economic talk, one can do just the same without claming science. Scientific authority is no longer critical for the economist. Even if the opportunities of science hang high, even if one can doubtless still attract attention, make politics or even business with scientific authority, there is simply a *diminishing need* for it. There is a diminishing need for economics departments. It is time to get serious about what most economists know anyhow, and tear down the discursive walls around economic science. My radicalism boils down to the simple claim that economics, as demanded by almost all economists in the form of a plea for more "interdisciplinarity", will *dissolve into* other disciplines. What makes my argument seem more radical than the commonsense skepticism about economics is thus merely that economists have not yet drawn the consequences from their skepticism. Face up to it, there is no longer any reason for a further development that could reinforce or even renew the institutions of economics. What could be the next guarantee of its power? Who could enforce it? Of course, the triad of Chicago-MIT-Stanford is Big. But it is not old.

How could I better show how harmless the post-scientific world is than by showing that it no longer merely lies in the future? Economics departments *are already about to close down*. The actual divorce of economics and science, without economists having realized it, has factually already taken place. The peak of the efforts to unify economics analytically has already passed. Economics can disappear into the many economics of's, thus merging into sub-disciplines with their own theoretical interests – which makes it less and less necessary to study economic theory. What remains in economic departments is this black hole of the analytic core of "economic theory", which, as I have argued throughout, will collapse as soon as it loses its supporting flesh of those who continue interpreting it – the skeleton of "the economy".

The institutions representing scientific authority in economic talk – economics departments – are not in a sustainable situation. They are free to dissolve. The age of science in economic writings has passed. Economic science passed its peak, running downhill, fun but effortless; everything that could have been achieved has been achieved, everything that could be said, is said.

Economists, to be more illustrative, relate to the authority of science like waning lovers in the days before they part. Afraid of losing the authority of science, one embraces it in a last attempt even stronger, but by doing so only stirs up the burden of the relationship that neither side can continue to bear. The only way out is to come to the point at which an anxiously awaited decision comes down to the simple recognition of a past: that the actual divorce has *already* taken place. Then relationships can fall in the twinkle of an eye – a light shove, I promised, no more.

Yet the Optimism Lies in the Reversal: The Liberation from Science to a Renewed Intellectual Sensibility

Implausibility remains, in particular in light of the many economists who crowed year after year at the ASSA meetings and put intellectual efforts into the worth of their profession of considerable affective density. This, granted, cannot be a misunderstanding. Affects cannot lie! And economics matters, yes! How can I then propitiate the economists' minds? What is the good news?

The task of my critical exercise was to address the economist (rather than to provide a philosophical or social justification or explanation). Obviously this does not mean to address the economist *in person* – which could only be done formally (Dear, Mr. X). Addressing was meant in a strictly phenomenological sense of pointing to the subjective constitution of economics that is manifest in the *ethos* of economists. Certainly, I hardly address economists by stating that they lack any intellectual ethos, as long as addressing means always to address the other *in* his or her ethos. I hardly invite someone to a renewed reflection by undermining his or her discursive integrity. I can exploit the skepticism and the irony of economists to some extent, but as long as I support economists in their attempts to make sense of themselves, such critique risks even reinforcing the dilemma I charged.

Yes, economists do try to show a positive attitude about the social world they live in. Most have found their place within their institutions somewhere between theory and politics, neither really here nor there. They can rely on a considerable biography of interest and care about particular problems that partially stem from general economic talk, mostly from the century of economics proper, and sometimes also from a theoretical sense. The usual talk among academic economists is to arrive at an economic claim *with* economic theory. These conversations work as long as one is able to maintain, to remember and also to share one's intuitions about the world of economic life.

In no line did I doubt it. Even some mathematical (no pause) economists manage to live a considerable expressive life in various political institutions. Read, for example, the astonishing biography of Graciela Chichilnisky, a student of Debreu and maker of the Kyoto protocol (in Szenberg, Ramrattan 2004: 108 ff.). Somewhat better known, Gary Becker, the personification of the insensibility of "the economic approach", also showed strong emotions about his work:

> I began to lose interest in economics during my senior (third) year because it did not seem to deal with important social problems. I contemplated transferring to sociology, but found that subject too difficult. Fortunately, I decided to go to the University of Chicago for graduate work in economics. My first encounter in 1951 with Milton Friedman's course on microeconomics renewed my excitement about economics. He emphasized that economic theory was not a game played by clever academicians, but was a powerful tool to analyze the real world. His course was filled with insights both into the structure of economic theory and its application to practical and significant questions. That course and subsequent contacts with Friedman had a profound effect on the direction taken by my research (Becker 1992: autobiography, nobelprize.org).

If even Becker was able to believe in the relevance of his "economic approach", why not those economists who engage in conversations on the basis of those theoretical innovations of the last two decades which tried to cope with the irrelevance associated with Becker's work?

Economists do engage in decent expressive activities. Many learned to associate economic life with "rationality" or lack of it – whatever that means, for most it does mean something! Economic theory may commit the economist to some caution, but does not withhold him or her from making actual claims that are identifiable as stemming from his or her expertise. In order to also propitiate the minds of those who believe in the theoretical innovations of the last decade: Yes, certainly, the expressive life of economists may flourish more likely in a world in which Cobb-Douglas functions and topology are no longer the gate-keepers of publishing. For some, emotions heat up when reading 'neuroeconomics has shown that emotions matter for decision making', although it is not clear who has ever had doubts about that. Debreu would turn red if someone said he assumed so. And Becker would simply laugh.

Now and then some economists may even come to the point of really being convinced about this or that social agenda or political reform. Then they draw from their intellectual experiences the forces to enter one of the countless battles in economic talk. Some know why they are liberals, why Marxism can no longer be updated, why the minimum wage ruins the state of the poor, why monetary institutions need to be more centralized, why foreign aid is better than trade agreements, why agricultural policies in the U.S. ruin developing countries, etc. Others perhaps enjoy arriving at the point of debating how economic anxieties nourish the political violence of the day. The discursive depths of the culture of the economic suspicion are wide. And everywhere the economist can run into those who have an opinion on why the talk of economic scientists is biased or irrelevant. There are plenty of opportunities to respond.

In no single line have I deprived economists of such a *factual* ethos. It was not up to me to describe or in any sense scrutinize their factual ethos, let alone to show what exactly are the depths of economic talk. This *the economist* has to know! Precisely this ability and necessity of entering economic talk is the locus where a renewed reflection can take place. The punch-line of the preceding exercise is thus: I did *not* show that economists lack ethos, that there are no economic claims made in economics, that economists are not more than Bourbakians or Debreuvians, that economics is mere mathematics, that economists are like Taylor workers thoughtlessly cramming discovered interpretations into ready-made axioms, etc. I promised *not* to contribute to this choir of mourning for economics. Indeed, what I have shown instead is that there *never has been* any "economic scientist", that there never was any "Bourbakian economist", that no economic claim could ever be made because of the commitment to science, that no economist could ever truly avoid his or her political bias when making politically relevant claims. *Impossibly.* Otherwise, one cannot live up to an actual self-perception as an economist. My narrative, specifically the parable of Debreu, showed the *necessity* of a factual ethos of economists.

What I have shown is that *if there is any ethos of economists, it is not nourished by scientific authority.* If economists make claims, then its authority is never guaranteed by science but by other sources. The necessity, with which I made this case, is of a transcendental kind. It concerns economists as those who "accomplish" and "achieve" economic theory. It concerns themselves. Hence, with transcendental discreetness, I avoided talking about the actual ethos of economists. My aim was not to show that there are no economists present in their economic theories, but that such presence *is at risk* in economic science. At stake was not *what* economic science, but its very possibility as an intellectual practice. The worth of The Phenomenology of Economics needs to be judged regarding how it disclosed this locus of criticism.

Could I not have come to such a conclusion much more easily without the phenomenological roundabout of the theoretical experience? I could have simply shown that in each actual economic claim there are always *irreducible* economic intuitions at work that do not stem from, nor are justifiable in the context of, science – even in the case of Leon Walras, John von Neumann, Kenneth Arrow, Gary Becker, etc. There is always a hidden intuition of economic life that may give rise to scientific optimism in economics, but, when looking closely, actually works against it. Arguing so was a matter of a philosophy, rather than a phenomenology, of economics. Only as long as the object of economists' concern remains implicit – is "put into brackets" – this concern itself could be made an object of a history. Only then could economists' involvement in economics be made the object of a narrative.

Here, thus, is the reversal of my pessimism about the presence and future of economic science: *Economists are free from its phenomenological contradiction if they experience their interest expressed or in any sense at stake when doing economics.* Such a benchmark cannot serve as a criterion to delineate good, phenomenologically enlightened, significant theories from others. It needs to be answered by economists themselves! The Phenomenology of Economics results in this question, the urgency, not the answer of which, this text was dedicated to. Perhaps some economists can nod without hesitation. Then we could close this book with a deep sigh. Yet I have given sufficient reason to believe that not all do so. It is in this sense that I charge economists with being the accomplishing subjects of their intellectual lives. It is a call for the economist to reflect on the motives which give rise to an engagement in economic science. To do so remains the task of economists. And so the present exercise was indeed a truly phenomenological reduction to the accomplishing intellectual life of economists.

Some Prospects of a Post-Scientific Culture of Economic Talk and the Further Task of a Genetic Phenomenology of Economic Life

Can I say more about this promise for a new intellectual sensibility than that it is suppressed by science? Are there *concrete* prospects for a post-epistemic period of economic talk? It should be clear that the punch line I just formulated disappears if I now urge myself to propose a different, say, principle that is exploitable in just another "theory". All I can do is to propose a different attitude than the scientific one.

In order to appreciate the liberating effect of losing scientific authority, one may follow this guide to a new culture of economic talk. It is the reversal of what I called the genetic code of the oblivion of the life-world. Practicing economics, I argued, is the unlearning of how to mobilize the forces of meaning to such an extent that one become irresponsive to them. Science, by means of the separation of the reality "of" and *of* science, directs these forces into channels so that they are experienced as *too* weighty and thus appear repellent. Instead of mobilizing the weight of meaning as the pathos of intellectual life, meaning evokes the uneasiness of an inadequacy, if not an estrangement. The liberating insight is thus: *Only in science, only under the necessity of exerting epistemic authority, is the weight of meaning experienced as a burden.* In the words of my narrative, only as long as the economist is committed to theorize on a level where the economic suspicion is not at work do economic claims appear too big to be worth science. The pessimistic tone I largely adopted can thus be easily reversed into an optimistic tone – yes, an enthusiasm about the *liberation from the burden of science*.

Whence I can depart with some stronger implications for a new post-epistemic culture. Rather than an invitation to an "everything goes" attitude, it invites us to look further into the phenomenological constituents of economic life, in that it affects the intellectual efforts of articulating it. The notion of the life-world implies that intellectual activity is experientially *late*. It needs to be affected. If it is true that all intellectual life is a response to…, then the economic suspicion tells us something of *economic lived experience*. This would be the first question of a genetic phenomenology of economic life: to present a (transcendental) genesis of the economic suspicion.

The open question is thus: whence the cultures of economic suspicion? And how do they constitute the weight of meaning in economic talk? How is economic life experienced that it serves as a seemingly infinite source for this suspicion? What of economic life makes it so brittle that it continuously gives rise to so much clamor – particularly but not exclusively in the last four or five centuries of capitalism? Such was the question of a phenomenological *genealogy* of economic life. "The generation of the economic suspicion" could be a historical guide through modern economic history, just as the "oblivion of the life-world" was for modern economic science. How do the experiences of needs and desires, acquiring means and having ends, private and political life – how do these experiences become contested within the various cultures of economic talk of today, after WWII, before the 19th century, in Britain of the 18th century, as well at the times when economic modernity settled down?

Such a project could certainly not justify the existence of economic science. It could not be an inquiry that resulted in a claim to referential truth. It would rather arrive at an economic transcendental materialism in that intellectual life itself appears in colors for which modern science as such is blind. For did I not show that economic life cannot be matter of epistemic concern, since as soon as one does, one slides off into the structuralism of "the economy"? Economic life is a matter of the transcendental genealogy of intellectual life, not an object of inquiry. Economic life does not satisfy epistemic concerns, for it is constitutive of it – which I believe not even Marxists have ever fully internalized. With this final claim I have responded to Aristotle's motto that stands at the beginning of this exercise: Regarding economic life a "proof" is not necessary. Thus, *the end of epistemic authority of what is and what ought in economic life.*

That such a genealogy happens "below" any epistemic concern of science is clear when considering another substantial implication of my critique – namely, considering the status of "the economy". Regarding the broadly-conceived narrative from the temporal order of the *oikonomia* to the structural order of "the economy", there is a clear conclusion: to the contrary of what the formalist revolution supposedly has accomplished, "the economy" is *not* a generic object to be studied, but rather the correlate of the intellectual discreetness of economists. The structuralist turn in economic talk from the *oikonomia* to "the economy" has never been fully carried out. I thus propose strongly that we give up speaking about "the economy".

As long as no economic claims ever stem from the theoretical perception of "the economy", economic talk can be liberated from it without restricting its expressive potential. To the contrary – reference to "the economy" hinders our ability to speak out concretely, frankly, and openly. Did my narrative not suggest that there is no such thing as an abstract, dissolved core of an analytic, scientific object, "the economy" in that it could satisfy an interest in its own right? Do we not have to give up the belief that one could talk about "the economy" as a way of talking about economic life? The renewal of intellectual sensibility requires such "bracketing" of "the economy". As a new phenomenological imperative for economic talk: Never speak about "the economy"!

Economic talk "after science" will be organized less by the authority of claims than by the various cultures of the economic suspicion. Recall what I have said about the nature of economic talk before there was economic science. Economic talk did not have a distinct discursive identity. People appealed to various authorities and therefore participated in different discourses. This is what economic talk in a post-scientific world could return to. The unity of economic discourses depends on the presence of scientific authority – that is, on the presence of those believing that "the economy" is indeed an object rather than a rhetorical strategy of diverting attention from something else. Economic talk is not unified because of its object, but because of a particularly nagging suspicion that can potentially show up at any moment. Economic talk is inherently plural – or better: there is no economic talk, but only the nagging of a suspicion.

Having said that, I do not have much to say in reply to those philosophers with deconstructive inclinations who suspect me as having hidden a dichotomy of life and structure, as Derrida once suspected Husserl (1980). Did I work with an operational opposition of life and structure, of materialism and formalism, or even of science and philosophy that I have not argued for because it allowed me to argue? To the extent that the preceding exercise had a disclosing character – rather showing and asking than proving and persuading – I did at last rely on that opposition, yes. But there was nothing to hide, for nothing remained a promise. The promised sensibility was *at work* in the preceding exercise, even if not made an object of inquiry. In this way this text has been, as noted above, a *prolegomena* to another sensibility – no more.

Economists of the World: Leave Science – at least for the Sake of Social Responsibility

Just like the first words, so are the last reserved for indulging in pamphleteerism. I argued against scientism on the background of a clear political vision. What may seem like epistemic nihilism is actually an urgent political agenda. Whether one argues in light of an idea of real science or not is not a matter of how "post-modern" we are in our epistemology, but is a matter of a political conviction underlying The Phenomenology of Economics: that economic science does not deserve the institutional power it exerts today, be it in the university or in non-academic political and social discourses, or on whatever level of economic talk. Arguing against science was a way to argue against discursive power and violence. Even for those economists who did not learn a thing about their intellectual sensibilities, I hope the preceding exercise was suggestive enough for them to seriously reconsider the epistemic authority they claim – not for themselves, but for *others*. Claiming authority happens after all *against* others. In their name, I would like to conclude.

The intellectual responsibility in science, which represents the moral horizon of the preceding contemplations, is the capacity to give a response to the experiences that make science interesting. Social responsibility of science requires being able to respond to the motives of others who are subjected to epistemic authority. Reflection on the motives that give rise to an intellectual life is a condition of becoming socially responsible for one's claim. In this sense economic science *must* fall for the sake of social responsibility.

The reasoning is clear: After the discursive gaps that I described in the first part, after the history of the diversion of the economic suspicion presented in the second part, and after the scandal of Debreu's biography covered in the third part, I concluded that scientific authority is achieved by means of ignoring one's own motives. Economic science is constituted in such a way that the motives for "doing" it and "using" it exclude one other. Economics is therefore necessarily socially irresponsible. It excludes *as such* a reflection on its social use. A change of the connotations of an economic concept can change the world without economists even noticing. The problem of social irresponsibility, therefore, cannot be solved *within* economic science. If it is inherent to scientific authority to be irresponsive to the motives that give rise to it, but this authority nevertheless finds its bypass into economic talk, then economists cannot be charged for the consequences of their work. Being intellectually irresponsive for the sake of science, economists avoid all responsibility for the social effects of their scientific authority.

To avoid the economic suspicion is thus not problematic in that it makes economics politically irrelevant. Economics always finds its way into its discursive surrounding where there are people who (unknowingly or secretly) find associations with a political position. And if the very phenomenological constitution of economic science requires that it not be blamed for being the delinquents of ideologies, economists *have to be charged* with this responsibility. Economic science must fall for the sake of moral integrity in economic talk.

Should it not be at the heart of the discussion of economic theory how it enters the public debate? Should economists not direct all their energy to the uses and misuses of their theories? Should it not be the first historical concern of economists how they contributed to the project of modernity, insofar as it ended up with a tombstone reading "1945"? Should it not be the first question of every history class which possible violence the project of a science of wealth did to the moral architecture of the household? Should it not be a primary concern whether the economic suspicion comes from or is the result of the many political diasporas of the last centuries? Should it not be a nagging question in economists' political awareness how liberty in markets was supposed to free us from those who lead, but took alliance with the greatest violence of the last decades? Should not the first question in an introductory course be what epistemic authority could possibly amount to in a world that has seemingly left behind the opposition of socialism and capitalism? Should not the first question of every new generation of economists be: What are the burning questions of today? Should not be the first and permanent task of economists be to be responsible for how their own reality affects the reality they claim? Should economists not finally *face* the economic suspicion instead of *evading* it?

Reverse this argument: Not claiming authority, not being certain, not attempting to persuade are the acts of social responsibility. Economic talk draws its intellectual force from its *vulnerability*. Exposing oneself to the question of Who Are You – Arguing This! is to show such vulnerability. Economists are most capable of taking social responsibility when they do not rely on the authority of science – when they expose their own skin.

The walls around economic science have to fall *for the sake of social responsibility*! As soon as one claims scientific authority, one risks being incapable of taking responsibility for what others will do with this authority. To take social responsibility requires one to be within, not beyond economic talk. Members of the AEA and all their friends: Resign!

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Op. cit.: McCloskey 2000 (I); Weintraub 2002 (P3); Colander 2000 (1.3); Derrida 1980 (P2); Hayek 1949 (1.2).

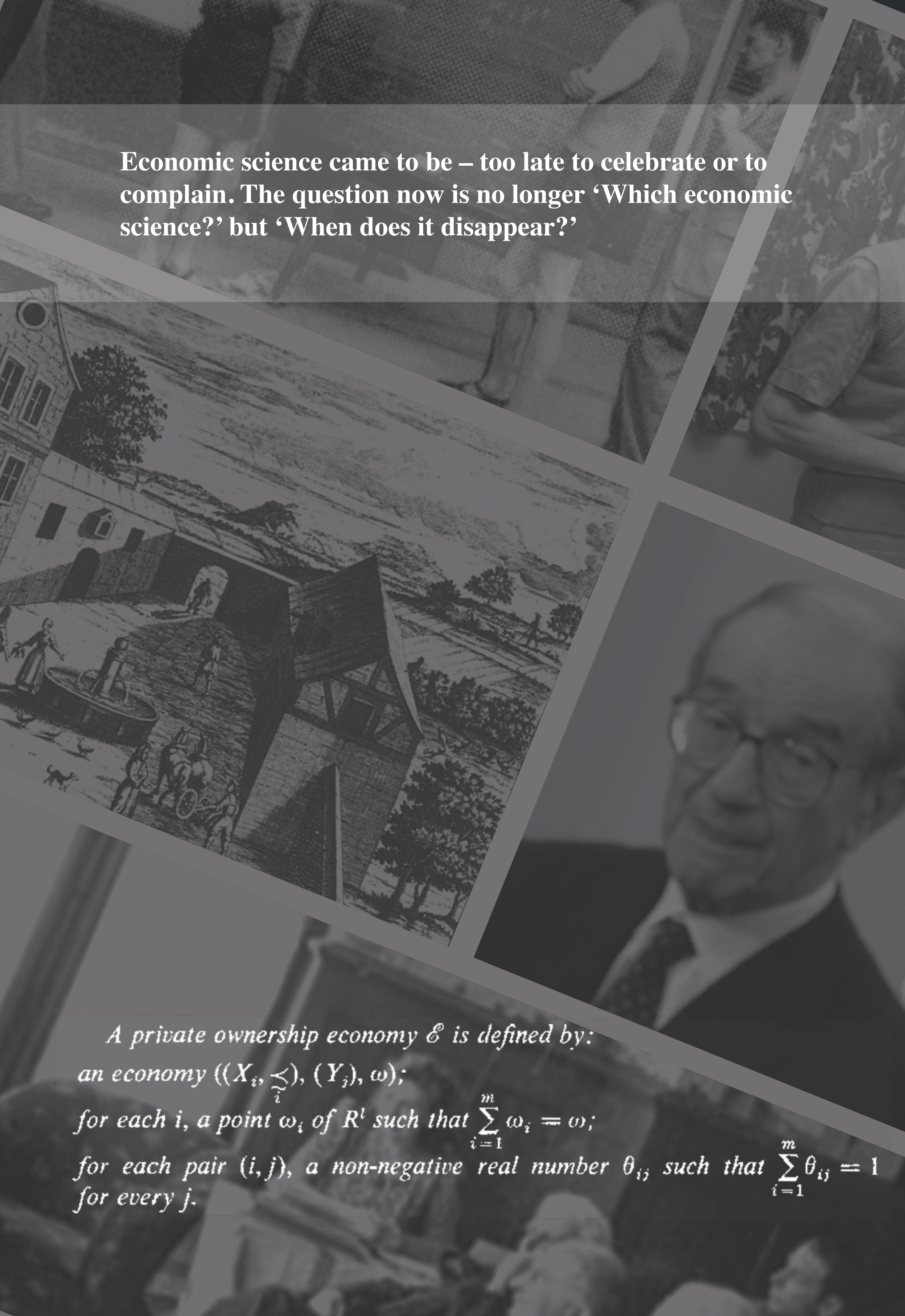
Samenvatting

Aan de hand van de fenomenologische notie van 'de levenswereld' formuleer ik in deze dissertatie een kritiek op de economische wetenschap. Deze kritiek werk ik in drie delen op drie verschillende manieren uit. Het eerste deel (*Discours*) beschrijft de discursieve situering van hedendaagse economen met betrekking tot hun openbare, professionele en pedagogische ethos. Het tweede deel (*Geschiedenis*) reconstrueert de sociale geschiedenis van de verwetenschappelijking van de economie. Betoogd wordt dat verwetenschappelijking plaatsvond door het vermijden van economische claims en dat de formalistische revolutie van de jaren 1950 het einde is van deze geschiedenis. Deel drie (*Biografie*) beschrijft de levensgeschiedenis van Gerard Debreu als de centrale figuur in deze formalistische revolutie. Het schandaal van zijn biografie is dat hij ondanks het feit dat hij zich nooit als een econoom zag, in 1983 de Nobelprijs voor de economie ontving.

Op deze drie manieren toon ik aan dat de economische wetenschap wordt geconstitueerd door een vergeten van de levenswereld. Nauwkeuriger gezegd, economen kunnen alleen dan aansprak maken op wetenschappelijke autoriteit wanneer zij de motivaties die hen tot het bedrijven van deze wetenschap drijven, vergeten. Op wetenschappelijke autoriteit aansprak te maken betekent dan tegelijkertijd een conflict in het intellectuele leven van de econoom.

Curriculum Vitae

Till Düppe was born in Mutlangen (Germany) on June 7 1977. He received his degree in philosophy (Mag. Phil) from the University of Vienna in June 2001, and in economics (Dipl. VW) from the University of Freiburg i. Br. in June 2003. He then obtained an M.Phil. degree at the Erasmus Institute for Philosophy and Economics, Erasmus University of Rotterdam, in June 2004. He is currently working at the Institute for the History of Economics (IWWT) at the University of Hamburg.



Economic science came to be – too late to celebrate or to complain. The question now is no longer 'Which economic science?' but 'When does it disappear?'

A private ownership economy $\mathscr{E}$ *is defined by:*
an economy $((X_i, \precsim_i), (Y_j), \omega)$*;*
for each i, a point ω_i *of* R^l *such that* $\sum_{i=1}^{m} \omega_i = \omega$*;*
for each pair (i, j)*, a non-negative real number* θ_{ij} *such that* $\sum_{i=1}^{m} \theta_{ij} = 1$ *for every j.*