

ECONOMICS AND PHILOSOPHY: MORE THAN HAVING FUN AND MAKING FUN

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Blurring boundaries between economics and other disciplines?

Consider the following three examples of recently published studies.

1. It is commonly believed that if there is traffic congestion, the problem is that there are not enough roads available to accommodate the number of travelers. After all, isn't it obvious that new roads permit car drivers to choose routes that are less congested by their fellow travelers, thereby decreasing total congestion? A recently published study argues that this isn't obvious. The study points out that closing some particular roads (which are denoted by black dotted lines in the picture) might be a rational strategy to reduce average travel delays. The authors establish this paradoxical result with the use of game theory. They assume that travelers converge on Nash equilibria if they opt for strategies that maximize their personal utility. They show that in social dilemmas such as the traffic congestion problem the Nash equilibrium reached is socially suboptimal in terms of average travel delays if all existing roads are open to all drivers than if particular roads are closed.

2. Back in the nineteen-nineties criminologists in the USA predicted a spike in murders by teenagers. It didn't happen. Why not? Popular causes and explanations identified by criminologists included the then booming economy (with the availability of well-paid jobs), clever police strategies and the proliferation of gun control laws. Statistical analysis shows, however, that one event had a much greater impact: the fact that Ms. McCorvey (who is known as Jane Roe in *Roe v. Wade*) won her class-action lawsuit seeking to legalize abortion. The consequent legalization of abortion throughout the USA meant that far fewer children were born into adverse family environments than would otherwise have been born. The underlying idea is that children born into adverse family environments have a greater probability of becoming a criminal than other children (the cost-benefit ratio of engaging in criminal activities is different for them than for well-educated children).

3. For a long time it was taken for granted that simple and determinate behavior such as rhesus monkeys looking at alternating flashes of light on a screen is produced by reflex-type mechanisms in the brain. Recent research, though, suggests that the monkeys' eye movements are more complex than that. Measurements of neural activity in the so-called LIP area in the monkeys' brain in experiments in which the probabilities and rewards were changed suggest that neural activity in that area encodes something like classical expected utilities. The researchers involved make a more general plea for the use of

expected utility theory and game theory as helpful normative benchmarks in the study of neural activity in both monkeys and people.

Which of these three studies belongs to economics? I can easily imagine you would believe that none of them belongs to economics. None of them deals with typically economic issues such as wealth, poverty, economic growth, company takeovers, inflation, financial crises or something of that sort. I can also imagine that those of you who are acquainted with economic theory and its uses believe that the first and third study belong to economics. Both the first and third study make explicit use of the standard tools of economics, such as game theory and expected utility theory. No such economic tools seem to be utilized in the second study. Yet in fact only the second study squarely belongs to economics proper. That is to say, only the second study was conducted by economists (people trained in economics, working in a department or faculty of economics) and was published in an economics journal (Donohue and Levitt 2001). The first study was conducted by physicists and was published in a physics journal (Youn et al. 2008), while the third study was conducted by neurobiologists and published in a neurobiology journal (Platt and Glimcher 1999). In fact this third study can be called a borderline case, not only because the neurobiologist in question (Paul Glimcher) calls his new approach neuroeconomics, but also because he occasionally also publishes in economics journals.

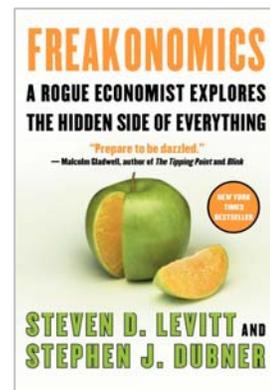
This is symptomatic, I submit, of the present state of economics and its relation to other disciplines. Many papers nowadays are written by economists and published in serious, even prestigious economics journals about subjects that do not seem to be related to what people call the economy or the economic domain in society. In the economics profession nobody seems surprised any more to see papers with titles such as “Does television cause autism?” (in the NBER Working Papers series) and books with titles such as *More Sex is Safer Sex*. Furthermore, it is not clear what role the concepts and tools of economic theory (or, indeed, any theory) play in the studies conducted. At the same time we see that concepts and tools taken from economic theory are finding their way into other disciplines. Philosophy is no exception to this trend. In particular, game theory, Bayesianism and social choice theory are increasingly being used in branches of contemporary analytical philosophy such as political philosophy and epistemology. Indeed, the uses of such theories have arguably become the third main branch in the philosophy of economics, alongside the two more traditional branches of methodology of economics and ethics and economics.

Given that such studies are not about subjects that are traditionally deemed economic, but at least at first sight use concepts and tools from economic theory, one might be tempted to argue that they have a better entitlement to be called economic studies than many studies that currently appear in economics journals. At any rate, it seems to be increasingly vague where (and on the basis of what criteria) we should draw the boundaries between economics as a discipline and other disciplines.

The second study, about the relation between the legalization of abortion and the sudden drop in crime rates, was conducted by the economists John J. Donohue III and Steven D.

Levitt (2001) and published in the *Quarterly Journal of Economics* (later 2006 NBER working paper response). It is discussed in a more accessible and entertaining way in the best-selling book *Freakonomics* that the latter author, Steven D. Levitt, wrote jointly with the journalist Stephen J. Dubner. One might be tempted to regard *Freakonomics* and similar books that appeared afterwards as a mere sideshow, one that should simply be disregarded if one is interested in serious academic work in economics. Some speak of the emergence of a new genre in economics, pop economics or cute-o-nomics, discussing it as if it were entirely distinct and totally disconnected from the serious academic work done in economics (sometimes called A-list economics). But I think we should start realizing that papers such as the one written by Donohue and Levitt fill a significant fraction of the total pages in the top academic journals.

Many papers published in leading economics journals address ‘outlandish’ issues and it is not very clear what role economic theory plays in them. In fact, all of the ‘freakish curiosities’ discussed in *Freakonomics* are based on studies conducted earlier by Levitt and various co-authors, all of them published in top economics journals. In 2003 Levitt won the prestigious John Bates Clark medal for this kind of work. The medal is awarded biennially by the *American Economic Association* for the best American economist under the age of forty. Levitt would be the first to concede that the issue of the relation between the legalization of abortion and criminality is one of the more serious subjects discussed in the book. Other issues are more trivial and frivolous, such as what sumo wrestlers and schoolteachers have in common and why drug dealers still live with their moms. Levitt was not the only one doing this type of work in the late ’nineties. Several young economists were already doing similar work, but books like *Freakonomics* surely contributed to the trend. They led quite a few high school kids to choose economics as an academic study. Moreover, doing *Freakonomics*-type research seems to be actively promoted in several graduate schools by leading economists, including Levitt. They advise graduate students to focus on subjects that no one in the profession had dealt with previously, rather than embarking on important, major issues such as economic growth. It is much easier for new researchers to write a publishable paper on a new, unprecedented subject, they argue, than it is to write one on an important, major issue.



Economics made fun; economics is fun!

If there is one message that pop economics (or cute-o-nomics) books like *Freakonomics* want to convey, it is that economics is fun. It is fun to practice economics yourself and it is also fun to read about others practicing economics. On closer inspection, the supposed fun component can be unpacked into three elements:

1. Economics, its theoretical principles and results, can be explained and taught in juicy, entertaining stories rather than in boring mathematical equations and graphs. Economics is not just for autistic nerds. It can be wrapped up in such gripping ways that it also appeals to the most social, literate and popular guy in high school class. Since this sense

in which economics is fun pertains to how it is presented rather than its contents, let's call this *pimp your economics*.

2. Economics can shed an interesting light on catchy, sexy, even exciting issues, not just on such dull issues as government budget deficits. In principle the scope (or domain of application) of economics has no bounds. Nothing in social life falls outside its purview. The fun here lies in the sorts of subjects addressed with the aid of economics; they are taken to be more exciting and interesting than the supposedly dull issues that are traditionally dealt with in economics.

3. Economics is able to uncover the hidden side of the subjects that it addresses. Most (though not all) economists working on pop economics seem to delight in debunking conventional wisdom. They cherish the *devilish fun* in showing that things are different than they initially seem, especially to the fainthearted among us.

By spreading the message that economics is fun, pop economists do not want to create the impression that what they say about outlandish subjects need not be taken seriously. On the contrary! If all they brought about with their pop economics were a jolly bout of laughter among their readers (or worse, if readers were merely to turn aside with a wry smile) they would be deeply disappointed. As a rule, these pop economists are absolutely serious about telling us how things really are. At the least they want us to reconsider our ingrained views on the basis of their "... honest assessment of the data" (*Freakonomics*, 11).

It can be argued that what is really new in pop economics is only the first sense in which pop economics is supposed to be fun. The excursions and inroads made by economic concepts and tools into foreign territory (the second sense in which pop economics is supposed to be fun) have been going on for quite a while already. What is new here is perhaps only the massive scale on which economists are tackling outlandish phenomena. And, as we shall see in a moment, attempts to counter conventional wisdom also have a long pedigree in economics.

More than making fun

It is not so clear to what extent economists succeed in convincing the wider audience that economics is indeed fun in all three senses. I think it is fair to say that they succeed in showing that economics can be explained and taught in a light, entertaining way. Most of them certainly know how to tell a good story. Virtually all of them are a pleasure to read. What is less clear is that economists succeed in convincing the wider public that it is fun to apply economics, its central concepts and tools, to outlandish phenomena. It remains to be seen in particular whether the wider public shares the pop economists' sense of devilish fun in debunking conventional wisdom. This sense of humor might well be a peculiar sort, confined to certain schools of economics. Not even all the economists in Chicago seem to be charmed by it. It is no secret, for example, that James Heckman, a distinguished labor economist at Chicago and a Nobel laureate, dislikes the pop economics way of doing economics. Quite a few economists seem to find the devilish fun that pop economists find in debunking conventional wisdom rather silly, or even

annoying and embarrassing. Some economists are not at all amused about the sloppy way pop economists treat data and rival hypotheses (cf. DiNardo 2007).

In a blurb on the cover of Tim Harford's *The Logic of Life: The Rational Economics of an Irrational World*, Gary S. Becker, who is generally considered one of the leading pioneers of economics imperialism, writes "I strongly recommend this book, especially to those who want their economics to be fun as well as important." The topics discussed are cute, outlandish and sometimes exciting. But are they all important? In these worrisome times of financial crisis and collapse, economists tackling an issue such as why milk cartons have a rectangular shape while cola cans are cylindrical (Frank 2007) is reminiscent of the Emperor Nero fiddling while Rome burned. And even though some of the issues tackled are important (such as the relation between legalized abortion and criminality), one still has the feeling that economists have more urgent nuts to crack.

What pop economists purport to do is show that the concepts and tools of standard economic theory can be exported to shed an interesting light on phenomena that are traditionally covered by other disciplines (such as sociology, political theory and criminology). In economics we nowadays also see an opposing movement: insights are being imported from other disciplines. An increasingly prominent case in point is behavioral economics, which tries to inform and enrich economic theory by incorporating insights from psychology and cognitive science. What economics can learn from such disciplines, behavioral economists argue in particular, is that people are motivated not only by self-interest but also by a sense of fairness and that emotions and heuristics often play a greater role in human decision-making than cool calculation.¹

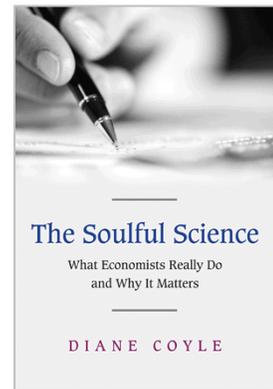
The rising popularity of behavioral economics is one of many signs that the boundary between orthodox and heterodox economics is blurred. It is no longer very clear what orthodox economics is nowadays. For decades, several heterodox schools of thought operating at the margin of the economics profession, such as institutionalist, Marxist and feminist economics, have been criticizing and distancing themselves from what they perceive to be the dominant, 'orthodox' or 'mainstream' way of doing economics. Many heterodox economists identified orthodox economics with neoclassical economics and some of them still continue to do so today. I think David Colander (2000) rightly argues, however, that economists today are not neoclassical according to any reasonable definition of the term. Given the radical transformations that economics underwent in the 1940s and 1950s, we can even argue with Mark Blaug that 'neoclassical' ceased to be an accurate characterization of mainstream economics after the Second World War. Currently we are seeing many publications by leading economists in prestigious economics journals which do not assume that individual agents are utility-maximizers, and are not structured around a general equilibrium conception of the economy. This holds in particular for cutting edge research at the frontiers. Some of them use computer simulations rather than analytical models. Others are based on experiments. In short, it has become increasingly difficult to specify what exactly distinguishes orthodox or mainstream economics from more heterodox schools of thought.

¹ Book popularizing insights from behavioral economics include Ariely (2008) and Thaler and Sunstein (2008).

To summarize, ‘economics is fun’ pop economics is not a sideshow that one can simply disregard if one is interested in serious academic work. It reflects an important trend in academic economics. Seen in the wider perspective of what is currently changing in economics, there are also other trends, though. The emergence of behavioral economics is one such development that in some respects is the opposite of pop economics. Instead of tackling outlandish subjects with the concepts and tools of standard economic theory, behavioral economics resorts to psychology and cognitive science to address issues that are more closely connected with the traditional economics issues. Many more developments in economics might be mentioned at this juncture. The variety of approaches and models in current economics is truly bewildering. Traditional boundaries are fading, not just between mainstream and heterodox economics, but also between economics as a separate discipline and other disciplines.

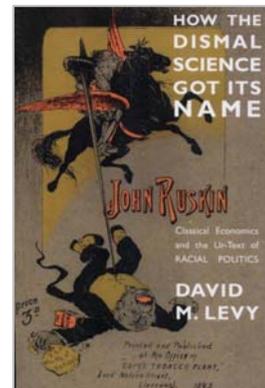
The not-so-venerable tradition of philosophers making fun of economics

One way to understand the rise of pop economics is that economics has grown sufficiently self-confident to rise against the image it has had among the intelligentsia for more than one and a half centuries: economics, the dismal science. It was Thomas Carlyle who coined the unflattering epithet ‘dismal’ for economics. Carlyle was not a philosopher but a writer and historian. I think it is fair to say, though, that Carlyle expressed a view of economics that ever since has been taken for granted by those who regard themselves as part of society’s intellectual and cultural élite, philosophers included. Some economists have grown tired of economics being branded the dismal science. They feel strongly that their discipline is misrepresented and misunderstood. Now they are ready to strike back. Economics is not the dismal science, it is the soulful science (Coyle 2007). Economics is not nasty, degrading, dehumanizing or what have you; it is fun!



What exactly did Carlyle find so dismal in economics? The received view is that Carlyle was put off in particular by the harsh policy recommendations proposed by Malthus to avoid mass starvation. Malthus assumed that the growth rate of food supply (which he assumed was arithmetic) needed to support population growth could not keep pace with the natural growth rate of population (which he assumed was geometric). Unless mankind were able to cut its population growth by artificial means, mass starvation would be bound to occur. One of the things Malthus recommended was to abolish the poor laws. At first sight, poor relief is a blessing to the poor, but Malthus pointed out that poor laws only tend to aggravate the misery of all. Thanks to the poor laws the poor can afford to raise more children, thereby only adding to the tendency of population growth to outrun the growth of natural resources. Furthermore, an accelerated growth of population would lead to lower wages. Whether or not Malthus had devilish fun in overturning the conventional wisdom that poor laws are good for the poor we do not know. But that Malthus’ policy recommendation went against the conventional wisdom of the time is clear.

A closer investigation shows, however, that Carlyle first used the epithet 'dismal' with respect to economics in a different context. In his "Occasional Discourse on the Negro Question" published in *Fraser's Magazine* in December 1849 he fulminated against the proposal made by the dismal economist John Stuart Mill to let the system of supply and demand determine the wages of former slaves. After the emancipation of slaves, plantation owners had serious problems finding personnel for the then prevailing wages, given the prevailing conditions of work. The economists argued that giving the forces of supply and demand free reign would lead to higher wages and better working conditions for laborers. Plantation owners would then no longer have a problem attracting labor. Carlyle thought it unwise to have a situation where "supply and demand [is] the all-sufficient substitute for command and obedience among two-legged animals of the unfeathered class" (p 186). He writes that the one who is "born lord" (p 205) of the other must compel the one "who is born to be a servant" (p 193) to work and if necessary compel him to work by the "beneficent whip" if "other methods avail not" (p 202). Carlyle says that "decidedly you [the Negroes] will have to be servants of those that are born wiser than you, that are born lords of you; servants to the Whites" (p 205). In short, Carlyle held the view that servitude and compulsion, rather than market forces, should regulate the supply of labor on plantations in the West Indies. Now let me ask you whose views you regard as dismal here? The view of the political economists or Carlyle's? I trust it is Carlyle's racist views that we now find dismal and even morally repugnant. Levy and Peart rightly wonder "...how it is that those who defended the wire whips and the judicial lynch - Carlyle, Ruskin, Dickens, Charles Kingsley - are celebrated by scholars in achingly correct modern English departments as "progressive thinkers" against those who protested in the name of the rule of law for people of all color." (p 5)



Carlyle more generally feared that the emphasis put on the material conditions of life by political economists and their advocacy of laissez faire capitalism would contribute to a dehumanization of society. Communal values would collapse into excessive, isolated individualism (for a recent critique of economics along these lines see Marglin 2008). This fear resonated with philosophers who for the most part were in the business of addressing less earthly matters in a more edifying way. In fact, there is a long-lasting tradition in philosophy of making fun of economists, not engagingly and with sympathy, as when you tease someone you love, but offhandedly and dismissively, as when you ridicule someone you surely do not love. I think that this condescending attitude of philosophers towards economics and economists has long stood in the way of a thorough engagement of philosophers with economics.

Probably the one thing in economics with which most critics of economics have found fault is the infamous *Homo economicus*, man as he is depicted in economic theory. This mythical creature, mostly associated with a relentless pursuit of material self-gain, is eminently dislikable. Charles Dickens' character of Scrooge in *A Christmas Carol* readily comes to mind. *Homo economicus*' is criticized on both moral grounds and on the ground that it is an unfaithful portrait of how people of flesh and blood really act. From a moral

point of view, *homo economicus* is seen as a despicable creature, devoid of fellow feeling and community concerns. Ethical doctrines associated with the economist's frame of mind, such as consequentialism and especially utilitarianism, are nowadays regarded by many ethicists as cheap, crude, unsophisticated and even degrading. We hear echoes here of Carlyle's indictment that economists are barbarians at the gate. *Homo economicus* is also criticized for being an unrealistic portrayal of how real people actually are. Real people – people like you and me – do care about others and do have a sense of fairness and justice, especially (but not only) in domains that have traditionally been regarded as non-economic. How can an economic theory, which is based on a view of man that is so obviously at odds with how people really are, ever be a good, acceptable theory?

This is the question the philosopher John Dupré sets out to answer in *Could there be a Science of Economics?*. Dupré tends to give a negative answer to this question, arguing that the most basic postulate of economic theory, that individual agents behave economically rationally, is quite plainly false. It is clear, Dupré writes, that people do not have complete rankings of all their preferences, that they occasionally make mistakes in satisfying their preferences and that their preferences change over time. Dupré argues that this makes specifying stable demand curves - something that economists not only actually do but are committed to doing, according to Dupré - a forlorn hope.²

John Dupré is a highly respected philosopher of science and rightly so. Among other things he has made valuable contributions to philosophy of biology and to how we can think coherently of the disunity of science. But here, in this specific case, I think Dupré has not rendered a service to anyone, philosophers, economists, himself or the rest. It is clear that he has not seriously and thoroughly engaged in a careful study of the role that assumptions play in economic theories and models. Instead, Dupré took at face value what he found on the surface of economics. Or worse, he mainly relied on what practicing economists (such as Lord Lionel Robbins and Milton Friedman) have said about their discipline in their sparse moments of methodological reflection and on what other philosophers have written about these methodological self-reflections. No attempt is made to relate to the actual work of practicing economists. Fun is made of economists, those naïve folk who are supposed to believe that an empirically meaningful, successful science of economics can be erected on the basis of obviously false postulates.

The principle that people respond to incentives

Philosophers are not the only ones making fun of economists. Economists themselves do so too. In fact, there is a flourishing industry of economists making jokes about their own discipline and profession. Take a look at the stand-up economist Yoram Bauman in action and enjoy.³

Yoram Bauman is quite popular and successful, especially among fellow economists. It seems that the things Bauman says about the principles of economics are similar if not identical to what philosophers and other critics have been saying about them all along.

² In fairness, it should be noted that Dupré hedges his claim by making several reservations. But this does not prevent him from making rather bold statements.

³ See <http://www.youtube.com/watch?v=VVp8UGjECt4>

But the way Bauman expresses himself appeals much more to practicing economists than the way philosophers do it. Economists have never been particularly impressed by the critiques of philosophers. Most of the time such critiques have fallen on deaf ears. It might be argued that economists have insulated themselves too much from voices from the outside. But isn't it understandable that economists have no appetite for reading papers with telling titles such as *Could there be a Science of Economics?* Bauman's success among economists undoubtedly has to do with the fact that he is regarded by fellow economists (fellow *micro*-economists, that is!) as 'one of us'. He speaks their language and also shares their sense of humor. But I think it also has to do with the fact that Bauman is not superior and patronizing; rather he works in a light, entertaining way that, moreover, is informed about how economists understand and treat their own principles. Bauman's performance is meant to provoke laughter among economists, not the feeling that they are dead wrong about almost everything.

One could still argue, of course, that the very fact that economists laugh at the fun Bauman pokes at them does not do anything to justify the continued use of the principles. But what should give us philosophers reason to pause is that in his own academic work (mostly on economic development) Bauman uses some of the very principles he makes fun of. Why do economists continue to use these principles, even though they recognize that they are strictly speaking untrue? The fact that Bauman's performance elicits so much laughter among fellow economists suggests that economists take their own principles with a pinch of salt.

This is something we methodologists of economics have indeed learned to take seriously: economists do not take all of their assumptions literally. Most of the methodology of economics arguably started out in the early 'eighties as an attempt to appraise or assess economic theory from the perspective of what was then taken to be the prevailing philosophy of science, such as Popper's falsificationism and Lakatos's Methodology of Scientific Research Programs. The prevailing philosophy of science was in turn taken to characterize best scientific practice. In due time, we have discovered that none of the pre-fabricated standards for theory appraisal taken from the philosophy of science bookshelf fits economics practice especially well (as, for example, Wade Hands and my predecessor Uskali Mäki have argued convincingly). We also discovered that economists are serious when they say that some of their assumptions in their theories and models, most notably the assumption of expected utility maximization (earlier called economic rationality), need not be true if taken literally. When it comes to economics, taking the assumptions made at face value (which is one of scientific realism's credos) might be a bad, misleading strategy. It is

The 10 Principles of Economics

1. People face tradeoffs
2. The cost of something is what you give up to get it
3. Rational people think at the margin
4. People respond to incentives
5. Trade can make everyone better off
6. Markets are usually a good way to organize economic activity
7. Gvmts can sometimes improve market outcomes
8. A country's standard of living depends on its ability to produce goods and services
9. Prices rise when the government prints too much money
10. Society faces a short-run tradeoff between inflation and unemployment

likely to lead to a misunderstanding of economic theories and models. There is more to economic theories and models than meets the eye.

Take Mankiw's third and fourth principles about thinking at the margin and incentives. I believe that Bauman's translations of these principles to the uninitiated, "people are stupid" and "people are not *that* stupid", capture the gist of how practicing economists look at these principles. What is crucial to practicing economists is that economic theory is roughly right about the changes in aggregate behavior brought about by changes in incentives. Much of contemporary economics is about incentives: how to align the incentives of different individuals, how to make them compatible with each other and so on. We can also see the centrality of incentives in *Freakonomics*. Some commentators have rightly pointed out that there is remarkably little economic theory in a book that purports to show that economics can be fruitfully applied across the board. Most of the studies discussed seem to draw more on a certain use of statistics to discern the causal forces at work (sometimes called "clean identification") than on economic theory. If there is one thing in *Freakonomics* that is undeniably related to economic theory, it is that people respond to incentives. Levitt and Dubner argue that economics is at root the study of incentives: "The typical economist believes the world has not yet invented a problem he cannot fix if given a free hand to design the proper incentive scheme" (*Freakonomics*, 16).

What is much less crucial to economists is that people actually think at the margin, as Mankiw's third principle states. Economists do not really care whether or not people think at the margin as long as the way they respond to incentives is roughly as predicted by economic theory. Baumann jokes that, on closer inspection, the principle that people respond to incentives is a tautology. But that is not quite right. To an economist, an incentive is something external to an agent that induces the agent to behave in a way that differs from how he would behave in the absence of the incentive. One can think here of rewards, which are assumed to act as a carrot, or of punishments, which are assumed to act as a stick. Financial incentives such as subsidies and taxes no doubt are the paradigm examples of incentives, but economists nowadays also recognize the existence of non-economic incentives (such as social approval). A typical prediction in economics is that people on average buy fewer goods if the price of these goods increases (because of a rise in taxes, for example).

Now, flawless thinking at the margin by people is a sufficient condition for them to respond (on average) to incentives as predicted by economic theory, but it is not a necessary condition. Note in passing that what follows immediately from this is that economic theory is not refuted by showing that people do not think at the margin. In standard textbook microeconomics the prediction that aggregate behavior responds 'rationally' to incentives is deduced from the assumption that people think flawlessly at the margin. But as several economists have pointed out, the prediction does not require that people are infallibly equating costs and benefits at the margin. It is easy to see why. If an individual for whatever reason does not respond to a change in incentives (say the price of some good increased by raising taxes) in a rational way (say the individual does not buy fewer goods), this will cost him dear. If the individual recognizes this, it might

motivate him to bring his behavior in line with economic theory's predictions. Furthermore, even if he does not do so – that is, if he continues his unresponsive, costly behavior – he will run out of money. The share of his unresponsive behavior in aggregate demand will decrease accordingly, whereas the share of predicted responsive behavior will increase. Thus either way, aggregate demand will gradually be brought more in line with economic theory's predictions (cf. Becker 1962).

Arguments like this are often presented in economics under the heading 'Evolution'. Evolution and its relation to economic theory is something on which my own research has focused. The basic idea on which economists capitalize is that individuals can be expected to behave in such a way that population behavior is as predicted by economic theory only after some evolutionary process has come to an end. 'Evolutionary process' here can mean a variety of different things. Sometimes it refers to something like competitive selection in market economies, analogous to natural selection in biology, as when the relative performance of firms and households affects their budget constraints and thereby also their market shares. Note that this gets us back to Malthus' ideas about competition for scarce resources (which, as is well documented, helped Darwin in developing the principle of natural selection). At other times, 'evolutionary process' refers not to something analogous to natural selection but literally to biological natural selection. Recall the third study I discussed at the beginning of this talk. The neurobiologist and neuroeconomist Paul Glimcher argues that expected utility theory and game theory can help understand neural activity in hominids because hominid brains are the products of natural selection. At yet other times, 'evolutionary process' refers to a process of cultural evolution, as in game-theoretic accounts of the evolution of efficiency-enhancing conventions, norms and institutions. A key assumption here is that cultural items are transmitted from individual to individual via social learning. Finally, 'evolutionary process' sometimes refers to a process of individual learning. Here it is assumed that individuals only learn from their own experience and not from the experiences of others (as in social learning).

Thus 'Evolution' turns out to be some sort of catch-all, compendium notion. Not only the processes referred to are different; what is supposed to evolve also varies from case to case. And yet, as indicated earlier, the basic idea is that in all these different cases what evolves eventually is what perfectly rational creatures would choose. This basic idea is buttressed theoretically in particular by formal work done in evolutionary game theory, in which one of the key assumptions is that individuals are neither fully nor perfectly rational. Changes in the frequencies of strategies are governed by some explicitly modeled evolutionary dynamic. There are several variants of evolutionary dynamics, among them the replicator dynamic (sometimes also called Malthusian dynamic). What all these variants have in common is that the growth rate of a strategy in a population is proportional to the strategy's relative fitness. Thus the frequencies of fitter strategies spread through a population at the expense of the frequencies of strategies with a lower fitness. One of most celebrated results in evolutionary game theory (Mailath 1998) is that if an evolutionary process has an asymptotically stable rest point, it must be a Nash equilibrium. The concept of a Nash equilibrium is traditionally associated with a situation of coordination on which perfectly rational players with common knowledge might

converge. What evolutionary game theory is taken to show, in other words, is that evolutionary processes tend to produce frequencies as end-states that players would have chosen if they were perfectly rational. Or, as Hammerstein and Hagen put it, the strongest conceptual link between economics and biology is that "... although there are important exceptions, rational choice, evolution by natural selection, and learning often arrive at the same result" (Hammerstein and Hagen 2005, 608).

How much solace does this analytical result offer to the principle in economic theory that people respond to incentives? The theoretical support given seems to be conditional and qualified at best. First of all, strictly speaking, evolutionary game theory does not show that individuals will necessarily converge on a Nash equilibrium in games that have Nash equilibria. But even apart from this subtle point, using evolutionary game theory to study processes of biological evolution, cultural evolution and individual learning alike involves making heroic assumptions. Often it is simply assumed without any further argument that the same evolutionary dynamics that are posited in the study of biological evolution through natural selection can be posited in the study of all the rest. And, as we have just seen, what is supposed to evolve also differs markedly. It makes a difference whether what evolves are institutions, behavior or proximate causes underlying behavior such as neural activity, basic preferences, mental modules, or fast and frugal heuristics, to give a few more examples of what one can find in the literature. As Glimcher stresses, for example, arguing that 'rational' patterns of neural activity evolve does not imply that the behavior of the individuals in question is always rational. If individuals encounter problems that their brains did not evolve to solve, individual behavior cannot be expected to be rational.

So the theoretical support that reference to evolution gives seems to be limited. What about the empirical evidence? Many experiments conducted in psychology and economics seem to show clearly and consistently that individuals behave irrationally in many situations. Confronted with such findings, economists such as Ken Binmore (1999) argue that evolutionary processes, this time understood as processes of individual learning, will gradually eliminate irrationality in individual behavior. Binmore claims that such anomalies in decision theory only occur if individuals are not given sufficient time to become acquainted with the artificial laboratory setting into which they are put. If the stakes in the experiments are high enough and if people are given sufficient time to learn how to respond to the laboratory situation, these economists argue, then the anomalies will disappear. To date, however, we only have mixed evidence about experiments in which the stakes are high and that are repeated several times so as to give subjects ample opportunity to learn. In some of the experiments, irrational individual behavior disappears, while irrational individual behavior persists in others (Bruni and Sugden 2007). Thus all in all neither the theoretical nor the empirical evidence lends unconditional, unqualified support to the claim that evolution causes individuals to respond on average to incentives as predicted by economic theory.

What this brief excursion into evolution and how it relates to economic theory also shows is that attempts to pay close attention to actual practice in economics do not at all obviate our subjecting practice to critical scrutiny. Taking the economists' arguments seriously

and criticizing them (for being non-conclusive, for example) can go hand in hand. But I hope it is clear that this sort of criticism is different from poking fun at the dismal science *ex cathedra*. Misplaced feelings of superiority only stand in the way of a thorough engagement with actual practice in economics. Without a thorough engagement with actual practice in economics there is no hope that the methodology of economics will be a serious, worthwhile endeavor. Just as there is no excuse for practicing economics without a thorough engagement with economic reality, there is no excuse for practicing the methodology of economics without a thorough engagement with actual practice in economics.

If I zoom out now to look at the present state of the art in the field of methodology of economics, considered more broadly, I believe there are many hopeful developments. Without pretending to be exhaustive I see promising new work on models, causality, explanation (and the different virtues they could have), experiments and evidence, for example. (And I am happy to have some of the brightest new people working on this in my own team.) But I also see relapses (or should I say remnants of the past?). There are still methodologists out there, especially those who believe that their own *a priori* ontological intuitions provide a rock-bottom basis for assessing economic theories, who are making fun of economics in the disengaged, offhand way I have criticized here. Old habits die hard.

Ik heb gezegd.

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