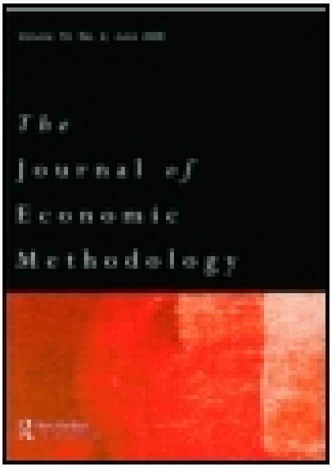

Journal of Economic Methodology
Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/rjec20

'The methodology of positive economics' (1953) does not give us the methodology of positive economics

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Published online: 17 May 2010.

Journal of Economic Methodology 10:4, 495–505 December 2003

'The methodology of positive economics' (1953) does not give us *the* methodology of positive economics

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Abstract It is argued that rather than a well defined F-Twist, Milton Friedman's 'Methodology of positive economics' offers an F-Mix: a pool of ambiguous and inconsistent ingredients that can be used for putting together a number of different methodological positions. This concerns issues such as the very concept of being unrealistic, the goal of predictive tests, the as-if formulation of theories, explanatory unification, social construction, and more. Both friends and foes of Friedman's essay have ignored its open-ended unclarities. Their removal may help create common ground for more focused debate in economics.

Keywords: Unrealistic assumptions, predictive tests, truth, as-if, unification, social construction

1 FROM F-TWIST TO F-MIX

My title is not intended to suggest that there is a single authentic doctrine of the methodology of positive economics, and that Milton Friedman's 1953 essay (F53 for short) failed to describe it. It is rather to suggest that the essay describes (ingredients for) too many methodological doctrines, hence fails to capture a single doctrine. This also implies that if there is something like Milton Friedman's methodological view with a distinct and singular identity, it is not described in F53. Thus I take popular references to things such as 'Friedman's instrumentalism' or 'Friedman's as-if methodology' or 'Friedman's argument in support of unrealistic assumptions' at best to highlight limited aspects of the 1953 essay and at worst to denote nothing but fictions. The message of F53 has tended to become a straw man, for friends and foes alike.

My contributions to the discussion on F53 have emphasized its ambiguities and its lack of coherence.[1] This suggests the need for clarification and reconstruction as a prerequisite for setting out to evaluate any definite methodological claims and arguments possibly ascribable to Friedman or his opponents. To suggest that economic theories should be judged by the accuracy and scope of their predictions, or that the claims economic theories make should be formulated in terms of 'as-if', or that the assumptions of an acceptable theory do not need to be true provided it predicts well – as

Journal of Economic Methodology ISSN 1350-178X print/ISSN 1469-9427 online © 2003 Taylor & Francis Ltd
http://www.tandf.co.uk/journals
DOI: 10.1080/1350178032000130484

in Paul Samuelson's (1963) attribution of the F-Twist to Friedman – is insufficient for determining anybody's methodological or philosophical outlook or for capturing what is distinctive of F53.

F53 appears as a mixture of ingredients many of which are ambiguous and some of which are hard to reconcile with one another: we are served an F-Mix. In consequence, a variety of readers with different intellectual preferences will be able to find their own selection of ideas that they will endorse or oppose. On my own favorite reading – my preferred selection of ingredients – Friedman emerges as a realist and social constructivist rather than, say, a positivist and instrumentalist.

The following brief remarks provide a selective review of puzzling ambiguities and inconsistencies in F53 and its readings. I should add that while these features are puzzling, I find them very useful and instructive – and that by identifying and removing them I believe we can, at the end (but not here), refine Friedman's deep insights into a coherent and sound methodological account of economics. Fifty years after its publication, I remain fascinated by the insightfulness of F53 as well as challenged by the multiple directions towards which its various ingredients can be developed.

2 ASSUMPTIONS AND PREDICTIONS

If there is one basic thesis conveyed by F53, it is this: economic theories should not be judged by their assumptions but by their predictive implications – and in particular, the unrealisticness of the assumptions of a theory is no reason for complaint or worry about the theory. The methodological advice given to economists by F53 appears simple: when assessing a theory, don't pay attention to its assumptions, instead focus on the predictions it yields. As the much cited statement puts it, 'the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted ('frequently' or more often that predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted' (pp.8–9). This makes three important points about testing scientific theories: testing is by predictive implications not by assumptions; failed predictions play a key role: acceptances are just failures to be rejected; and testing is comparative: what matters is the predictive performance of a theory relative to that of alternative theories.

Understandably, Friedman's main focus of attention in F53 is on assumptions, not on predictions. After all, he has set out to convince the skeptical or hesitant reader that unrealistic assumptions are just fine, and the arguments of F53 revolve around this idea. Yet, in some cases the reader would expect to be shown the full force of the basic thesis in its entirety, including the role of predictive performance. Friedman does not hide his hostility towards Edward Chamberlin's theory of monopolistic competition – an inferior theory that economics does not need at all in addition to the superior models of perfect competition and perfect monopoly. The argument of F53 is straightforward: the creation of the theory of monopolistic competition was 'explicitly motivated, and its wide acceptance and approval largely explained, by the belief that the assumptions of "perfect competition" or "perfect monopoly" said to underlie neoclassical economic theory are a false image of reality' (p. 15), and it is this misguided motivation and flawed basis of acceptance that speaks against the theory. There is no appeal here to the superior predictive capacity of Friedman's favorite theories in contrast to the predictive failures of Chamberlin's theory. The realisticness or unrealisticness of assumptions was not supposed to matter, but it seems they do, after all. The key thesis of F53 is thereby turned into a modified non-predictivist torso: hail unrealistic assumptions, proscribe against the pursuit of realistic assumptions.

This I believe is how many practicing economists have received the message of F53. This explains its emancipatory effect on economists: it helps liberate those employing models with unrealistic assumptions from a sense of unease (see Mayer 1993). This also suggests why there is an easy link between F53 and the defense of 'blackboard economics': that top ranked part of economics that is mathematically highly refined and rigorous but accused for being unconnected to real world facts and issues. Even though Friedman is an opponent of formalistic blackboard economics, it is not surprising to see appeals to F53 in justification to the assumptions that help create the model worlds on the blackboard. A tension appears here between Friedman's intended methodology and the torso version of the basic thesis of F53.

3 UNREALISTICNESS AS IRRELEVANT AND AS A VIRTUE

Friedman's claim about unrealistic assumptions appears at least in two versions: one is in terms of irrelevance, the other is in terms of virtue. The weaker version is the claim that unrealisticness is irrelevant for the goodness of theory: that it does not matter even if a theory's assumptions are unrealistic. A consistent irrelevance thesis would have to imply that the actual degree of both unrealisticness and realisticness does not matter. In other words, no matter how unrealistic or realistic the assumptions of a theory, this is irrelevant to the assessment of the theory. But the bulk of F53 is not symmetric in this way, thus not in line with the weak version: the essay keeps stressing that it is high degrees of *un*realisticness that do not matter. As the attack against more realistic behavioral assumptions and the theory of monopolistic competition indicates, the pursuit of high degrees of realisticness is regarded as a serious demerit of both a theorist who does so and of a theory that is pursued.

The strong version acknowledges this asymmetry and does it in a radicalized manner: it is the claim that unrealisticness is a virtue, that the better the theory, the more unrealistic it is in its assumptions. Many readers have found the strong version unacceptable, even outrageous. Indeed, as a general rule, it must be mistaken. Even in some cases where violating the truth may seem recommendable – such as in assuming a vacuum for falling cannon balls and assuming profit maximization for certain purposes – the strong version goes too far. Friedman's examples of excellent scientific theories assume zero air pressure and profit maximization. But there are no better theories assuming that air pressure is infinitely large or that businessmen aim at maximizing their losses – these assumptions would be more unrealistic than the ordinary ones, but surely not epistemically virtuous, thus contradicting the strong version.

There are obvious cases in which both versions appear questionable. In the study of the used cars market, it is neither irrelevant nor virtuous for a theory to falsely assume that information is symmetric, and in the study of the computer software industry, one is not well advised to assume diminishing returns. Qualifications and further conditions are needed: neither version of Friedman's thesis can be defended as a general principle.

4 INDIRECTLY TESTING APPROXIMATION TO THE TRUTH

It appears that Friedman himself does not hold either version of the thesis consistently or without qualifications. It appears that, for him, predictive tests serve as indirect tests of the approximate truth of assumptions. The required degree of approximation is relative to the purposes that a theory is supposed to serve: 'the relevant question to ask about the 'assumptions' of a theory is ... whether they are sufficiently good approximations for the purpose at hand' (p. 15). And the way to measure whether the required degree has been achieved is to put the theory in predictive test:

> Complete "realism" is clearly unattainable, and the question whether a theory is realistic "enough" can be settled only by seeing whether it yields predictions that are good enough for the purpose in hand or that are better than predictions from alternative theories.
>
> (p. 41)

This implies that the unrealisticness of assumptions is not irrelevant at all, something to be ignored. On the contrary, one is advised to pay attention to their actual degree of realisticness and to judge whether it is sufficiently high for the purposes at hand. It appears two kinds of considerations shape these judgements: pragmatic and ontological. Pragmatic considerations enter in the form of purposes: the appropriate degree of (un)realisticness is relative to the purposes for which the theory is put. Ontological considerations are concerned with the causal powerfulness of various factors: some of them are too weak to be included in the model, while others play major causal roles and should be included. In other words, it all depends on the difference those factors make:

> Why is it more 'unrealistic' in analyzing business behavior to neglect the magnitude of businessmen's costs than the color of their eyes? The obvious answer is because the first makes more difference to business behavior than the second; but there is no way of knowing that this is so simply by observing that businessmen do have costs of different magnitude and eyes of different color. Clearly it can only be known by comparing the effect on the discrepancy between actual and predicted behavior of taking the one factor or the other into account.
>
> (p. 33)

Another way of putting these ideas is to say that some of the assumptions of a theory are to be paraphrased as statements about the negligibility of a factor, and that predictive tests are a way of assessing such claims about negligibility (Musgrave 1981, Mäki 2000). The vacuum assumption in connection to predicting the behavior of a freely falling cannon ball should be paraphrased as the statement that the impact of actual air pressure is negligible, given one's purposes (while it is not negligible in the case of a falling feather): the deviation from the truth is negligibly small, or the degree of approximation to the truth is sufficiently close. Such statements about negligibility ('the causal significance of deviations from the vacuum is negligible for the purpose at hand') should be required to be perfectly true – while the non-paraphrased assumptions ('there is a vacuum') only provide more or less distant approximations to the truth. These considerations do not fit smoothly with an instrumentalist view of theory. They are rather realist considerations, but they do not require the (non-paraphrased, yet paraphrasable) assumptions to be perfectly or even moderately realistic. Yet they stress the relevance of the issue of the realisticness of assumptions.

5 THE WHOLE TRUTH AND NOTHING BUT THE TRUTH

A theory can be unrealistic in a variety of ways, such as by violating 'the whole truth' and by violating 'nothing but the truth' – by being incomplete and by being false proper. These are separate notions, but F53 conflates them – and so does much of other commentary about the realisticness issue. In particular, the violation of the whole truth does not imply the violation of nothing but the truth about some part of the whole.

Friedman implies the idea of the whole truth – or comprehensive truth – when he envisages 'a completely "realistic" theory of the wheat market' that mentions the color of the traders' and farmers' eyes and hair, antecedents and education, the physical and chemical characteristics of the soil on which the wheat was grown, the weather prevailing during the growing season, etc., etc. (p. 32). He implies the notion of nothing but the truth when referring to the empirical criticisms of the maximization assumption that conclude that businessmen do not or cannot maximize their expected returns (p. 31). And he implies a connection between the two when suggesting that criticizing the maximization assumption is as ridiculous as insisting on such a completely realistic theory. The reasoning is not sound: it is trivial that nobody will insist on having such a 'theory' – but it also should be trivial that this has nothing to do with the insistence that it is no recommendation for the maximization of returns assumption as such that it violates nothing but the truth.

The distinction has implications for the assessment of the basic thesis of F53. One may argue that good theories violate the whole (or comprehensive) truth in that they isolate just narrow slices of the world and leave out most of it – such as most of the things listed in Friedman's 'completely realistic theory' of the wheat market. One may go further and argue that, subject to further (perhaps ontological and pragmatic) constraints, the more a theory leaves out the better it is. One may also point out that such theoretical isolations are often accomplished by way of false idealizing assumptions (such as those of vacuum or homogeneous goods) that help exclude factors (such as actual air pressure or product differentiation) that are viewed as irrelevant for a problem at hand. These are some of the ways in which good theories may be unrealistic.

6 TRUTH OF ASSUMPTIONS AND TRUTH OF THEORY

Most commentaries of F53 are based on the presumption that the truth value of a theory is a function of the truth values of its assumptions or, more directly, is equal to the truth value of the conjunction of its assumptions. On this picture, the falsehood of assumptions implies the falsehood of the theory. Friedman and others admit that many of the central assumptions of the theories at stake are false. This then has been interpreted as implying a special kind of instrumentalist view of theory: economic theories are false instruments of prediction.

Friedman himself does a better job, and we can see this if we look a bit more carefully. He uses expressions such as a theory being 'descriptively false in its assumptions' (p. 14). We are not compelled to read this as admitting that such theories are false. Indeed, it has been my contention that theories with false assumptions may be true, and that realism (as a theory of theories) is perfectly comfortable with unrealistic assumptions. The truth value of a theory is not a straightforward function of the truth values of its assumptions. The key for understanding the gap between the two is to ask the question: what is the theory about, what claim does it make about the world, if any? A theory may be true about the functioning of some important causal factor while making false assumptions about the existence and functioning of other factors. Galileo's law is true about, well, what it is about: namely the causal role of gravity in determining the behavior of falling bodies. The core assertions of the law are not about air pressure, magnetic forces, or the shape of the earth, but about the gravitational field of the earth in relation to freely falling bodies. False assumptions about factors other than gravity imply nothing about the truth value of the law statement itself.

7 TWO KINDS OF 'AS-IF'

It seems to be generally believed that the use of 'as-if' in formulating theoretical claims commits one to an anti-realist (fictionalist or instrumentalist) view about scientific theory. This is a mistake. The as-if formulation is a flexible tool that can be used for expressing a number of ideas about a theory and its relationship to the world. In F53, Friedman appears to be of two minds as to the import of 'as-if'. It is one thing to say that

> (a) phenomena behave *as if* certain ideal conditions were met: conditions under which only those real forces that are theoretically isolated are active;

and it is quite another thing to say that:

> (b) phenomena behave *as if* those forces were real.

The difference between the two is striking and has important philosophical implications. Of the two, (a) is in the spirit of realism, while (b) allows for a fictionalist reading. Formulation (a) says that the behavior of certain phenomena is shaped by a real force isolated by the theory, and that it is not shaped by any other factors, so that those phenomena behave as if the theoretical isolation were materialized in the real world. This is what Friedman is effectively saying in a general passage:

> A meaningful scientific hypothesis or theory typically asserts that certain forces are, and other forces are not, important in understanding a particular class of phenomena. It is frequently convenient to present such a hypothesis by stating that the phenomena it is desired to predict behave in the world of observation *as if* they occurred in a hypothetical and highly simplified world containing only the forces that the hypothesis asserts to be important.
>
> (p. 40)

In his concrete illustrations, on the other hand, Friedman is closer to formulation (b). The imaginative hypothesis of the leaves of a tree is the most striking of his examples. Friedman suggests the

> hypothesis that the leaves [around a tree] are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives, given the position of its neighbors, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position.
>
> (p. 19)

However, 'so far as we know, leaves do not "deliberate" or consciously "seek", have not been at school and learned the relevant laws of science or the mathematics required to calculate the "optimum" position, and cannot move from position to position' (p. 20). This means that we are aware of the fictionality of the forces postulated, and that this awareness is expressed in terms of the 'as-if'. This rules out yet another way of using the 'as-if', namely its epistemological use in expressing uncertainty: what follows an 'as-if' is a hypothesis describing one possibility, and it is the task of future research to establish whether what appears possible is also actually the case. This is not an option for the fictionalist use of 'as-if'.

8 THEORETICAL ISOLATION AND ONTOLOGICAL UNIFICATION

In line with the realist use of the 'as-if', F53 contains passages suggesting that theory construction is a matter of theoretical isolation whereby economists 'abstract essential features of complex reality' (p. 7). This is a widely endorsed idea in economics and elsewhere: the real world is complex, therefore we need to build simple models that theoretically isolate causally significant aspects of the world. This general idea also provides justification for the employment of false assumptions in such models. The simplest version of Galileo's law isolates the significant impact of gravity on the falling body. It excludes the impact of other forces by false idealizations such as those of vacuum, absence of magnetic forces, flatness of the earth. On this picture, it is the task of false assumptions to help isolate major causal factors.

Friedman's remarks about these matters seem to be ontologically motivated. The economist is supposed to theoretically isolate 'essential features of complex reality' – which suggests that it is a feature of reality itself that some of its features are essential. The same idea is conveyed by another important passage in F53. This one introduces the realist notions of deceptive appearances and ontologically grounded explanatory unification:

> A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure.
>
> (p. 33)

One is invited to read this roughly as follows. The world is not as it appears to be: appearances are deceptive manifestations of more fundamental structures. Scientific theories are required to capture those fundamental structures. A theory unifies apparently disconnected phenomena by showing them to be manifestations of the same fundamental structure. Unification amounts to showing that those phenomena are really connected and only apparently disconnected, and this is accomplished by successfully representing how things are related in the way the world works. This is a matter of ontological unification (see Mäki 2001), and is only attainable by using a theory that truthfully manages to isolate the key causes and relations in economic reality.

9 FROM POSITIVISM TO SOCIAL CONSTRUCTIVISM

Those who have read F53 as a positivist or falsificationist statement will be disappointed upon being pointed out a few passages, completely ignored by most commentators. If one takes the methodology of positive economics to amount to a set of fixed and explicit rules for reasoning from given evidence (or evidence fixed by convention or agreement), one should be frustrated by the statements in F53 that emphasize the role of subjective judgement, of the background of economists, of tradition, and of consensus amongst them. In these statements, F53 acknowledges that strict predictivist tests are unavailable in economics. Here is a representative passage, making reference to something like the methodological culture of economics in comparison to that of sociology:

> Of course, neither the evidence of the economist nor that of the sociologist is conclusive. The decisive test is whether the hypothesis works for the phenomena it purports to explain. But a *judgement* may be required before any satisfactory test of this kind has been made, and, perhaps, when it cannot be made in the near future, in which case, the judgement will have to be *based on the inadequate evidence available*. In addition, even when a test can be made, *the background of the scientists* is not irrelevant to the judgements they reach. There is never certainty in science, and the weight of evidence for or against a hypothesis *can never be assessed completely "objectively."* The economist will be *more tolerant* than the sociologist in judging conformity of the implications of the hypothesis with experience, and he will be *persuaded* to accept the hypothesis tentatively by fewer instances of "conformity".
>
> (p. 30; emphases added)

In passing, F53 uses falsificationist jargon by referring to the 'repeated failure to be contradicted' as evidence for a theory, but it simultaneously acknowledges the role of social factors in shaping the fate of a hypothesis. It recognizes the *tenacity* with which hypotheses are held against negative evidence and the powerful role of *tradition* and *continued use* in creating the image of an acceptable hypothesis: the discipline has its own epistemologically forceful *folklore* that supports a theory but is irreducible to an explicit list of successful empirical tests. Here goes a key passage:

> [The evidence for the maximization-of-returns hypothesis] is extremely hard to document: it is scattered in numerous memorandums, articles, and monographs concerned primarily with specific concrete problems *rather than with submitting the hypothesis to test*. Yet the *continued use* and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony to its worth. The evidence for a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its very nature is difficult to document at all comprehensively. It tends to become part of *the tradition and folklore of a science* revealed in the *tenacity* with which hypotheses are held rather than in any textbook list of instances in which the hypothesis has failed to be contradicted.
>
> (pp. 22–23; emphases added)

The core idea is that acceptances and rejections of theories are not strictly rule-governed responses to empirical evidence, they rather depend on the subjective judgements of economists whose behavior is shaped by their background and social setting. Whatever label one may wish to use for these ideas – pragmatism, collective conventionalism, or perhaps social constructivism – it is obvious that they reflect the practitioner's actual experience that cannot be easily corrupted by positivist or falsificationist textbook teachings about the scientific method. With these statements, Friedman has shared his highly agreeable insights into the reality of scientific work, but this has a price: tensions emerge with some other ingredients in F53.

THE METHODOLOGICAL VIEW CONVEYED BY 'THE METHODOLOGY OF POSITIVE ECONOMICS': UP TO THE READER

So, what is the methodology of positive economics? If you choose to consult Milton Friedman's 'The methodology of positive economics' as your main source of information in answering the question, you will be put in a situation of choice. The menu provides ingredients for a number of doctrines, such as fictionalism, instrumentalism, positivism, falsificationism, pragmatism, conventionalism, social constructivism, and realism. You can then choose any permutation of them, either coherent or incoherent. My favorite choice is a coherent combination of realism and moderate social constructivism (or whatever one may want to call the latter). You may have different intellectual preferences. And are free to choose, Friedman might add.

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ACKNOWLEDGEMENTS

Earlier versions have been presented at the ASSA Meetings in Washington DC, January 2003, and at the Universidad Autonoma de Madrid, seminar jointly organised with the Urrutia Elejalde Foundation. Thanks to the two audiences for stimulating discussions and to Tom Mayer for helpful written comments.

NOTES

1 See, e.g., Mäki (1986, 1992, 2000). My first paper on Friedman's methodological essay ('Inconsistencies in Milton Friedman's methodology for economics') in 1980 was prompted by, and took the shape of a critical response to, Larry Boland's (1979) *JEL* paper that had argued that F53 provides a coherent argument for instrumentalism.

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