

THIRTEEN THESES ON PROGRESS IN ECONOMIC METHODOLOGY*

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In the last few years there has been an explosion in the literature on economic methodology. Speciality journals like *Economics and Philosophy* have appeared, traditional economics journals have increased their coverage of methodological work, and numerous books have been published on the topic. The fact that many of these recent books are actually textbooks on economic methodology clearly indicates that methodology courses are becoming a more frequent part of the academic curriculum.

Despite this increase in research and interest, many economists still claim that economic methodology is sterile, that progress never occurs, and that debates go on and on without the participants ever reaching a consensus. This lack of consensus is often cited as a reason for not participating in methodological discourse or for disregarding those who do. Economists who raise methodological or philosophical issues often lose intellectual status roughly in proportion to the fraction of their work in which such issues are raised. As Caldwell (1990a) demonstrates there are many factors contributing to this negative attitude

about economic methodology, but certainly the perceived lack of consensus is one of the more important issues.

This criticism of economic methodology is unjustified. We have learned quite a lot from the recent methodological literature and there is now a consensus on a number of important points. In order to defend this claim I would like to provide a list of some of the things which have been learned from recent methodological discussion. The list is comprised of things which are now generally accepted by those writing on economic methodology, but things which were not generally accepted by those writing on economic methodology before the late 1970s. Of course this list (my thirteen theses) does not demonstrate meta-methodological »progress» in anything but the consensual sense, and I do not mean to imply that there is absolutely complete agreement on any one of these thirteen points. The list is only meant to show what »most» economic methodologists have learned from the last decade or so of work.

The list is not presented in any particular order of importance or degree of consensus and the references cited are only indicative and not exhaustive of where the arguments can be found. The one exception is thesis thirteen, which, unlike the others, is not actually a point of consensus but rather one of my own personal arguments which I hope will become a point of consensus in the not too distant future.

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2. *Thirteen Theses*

1. *Econometric work does not »test» fundamental economic theory in a way which would satisfy most philosophers of science or in the way suggested by the standard rhetoric of econometric testing.* Econometric work, by far the most frequent type of economic work, generally provides only a weak corroboration of economic theory. The core theory of individual maximization is never challenged by econometric evidence, and while applied theories are often disciplined by the econometric evidence, there is seldom a crucial test which causes the theory to be abandoned. In applied subfields the facts do matter but they matter in a much more subtle and complex way than either philosophy of science or econometric rhetoric would lead us to believe [deMarchi and Gilbert (1989), Hendry (1980), Leamer (1983), Morgan (1988)].

2. *Falsificationism, the methodology of bold conjecture and severe test, is often preached in economics but it is almost never practiced.* This point requires little discussion; it is one of the most generally agreed upon of all thirteen theses. Controversy certainly remains over whether a falsificationist methodology »should» be practiced in economics, but almost everyone agrees that it very seldom is [Blaug (1980), Caldwell (1984), Hausman (1985, 1988), Salanti (1987)].

3. *Though »hard cores» and »positive heuristics» abound, »novel facts» as defined by the Lakatosian school have been few and far between in the history of economic thought.* Economic methodologists seem to agree that most economic research programs do contain »hard cores» (metaphysical presuppositions which are taken as beyond dispute by the program's participants) and »positive heuristics» (guides as to what constitutes an interesting problem within the program); there continues to be disagreement over exactly what these hard core and heuristic propositions are, but there does not seem to be any disagreement over the fact that they exist and that they define alternative research programs. On the other hand, the prediction of novel facts (facts which »had never been thought of before»), which is absolutely necessary for scientific progress in the Lakatosian framework, has seldom played an important role in the development of economic theory [Blaug

(1980, 1987), Hands (1985b, 1989), Latsis (1976), Weintraub (1985, 1988)].

4. *The »Duhemian Problem» is particularly difficult in economics; the complexity of economic phenomena and questions about the empirical basis of the discipline make empirical testing an extremely complex affair.* The Duhemian problem [Duhem (1954)] arises because a particular theory is never tested alone, rather theories are always tested in conjunction with auxiliary hypotheses (boundary conditions, regularity conditions, ceteris paribus clauses, simplifying assumptions, etc.). This means that a negative observation only falsifies the »test system,» the theory conjoined with the auxiliary hypotheses, not necessarily the theory itself. This is a particularly problematic issue in economics where phenomena are complex and the data questionable [Blaug (1980), Cross (1982), Hausman (1988), Hayek (1967)].

5. *It is not the case that one particular general theory or research program in economics (neoclassical micro, Keynesian macro, American institutionalism, Marxian economics, etc.) is clearly »science» while the others are clearly »nonscience.»* For many years economic methodology was used primarily to attack opponents in theoretical debate. Theorist A, who did not like theorist B's theory, would use a convenient philosophy of science to »prove» that theorist B's theory was not really »science.» Seldom did one try to show that their own pet theory really was science; the general strategy was to win by disqualification of your opponent. Hopefully this kind of »economic methodology» is a thing of the past. The nature of scientific knowledge is a subtle and complex thing; there are no simple rules which allow us to simply »accept» or »reject» any of the major economic research programs on purely methodological grounds. There is no specific reference here since the argument dominates most of recent methodological discussion.

6. *Milton Friedman's famous (1953) essay on economic methodology is most coherent if it is interpreted as an argument in favor of some form of instrumentalism.* It is now relatively standard to interpret Friedman as an instrumentalist: the view that scientific theories are merely instruments. Since there are a number of different interpretations of instrumentalism, not everyone agrees on exactly which

type Friedman is, but most agree that in his methodological writings he is an instrumentalist of some type [Boland (1979), Boland and Frazer (1983), Caldwell (1980, 1990b), Hirsch and deMarchi (1984), Mäki (1990b), Musgrave (1981)].

7. *Discussions in economics, like discussions everywhere, have a fundamentally rhetorical component.* While most methodologists would not subscribe to the view that the tools of classical rhetorical analysis are the only tools appropriate to analyze the writing and speaking of economists, almost everyone would accept the argument that economic discourse has a fundamentally rhetorical component [Klamer (1988), Mäki (1988), McCloskey (1983, 1985, 1988, 1989)].

8. *The axiomatic mathematical structure of modern general equilibrium theory (the Arrow/Debreu/Walras model) does not relate to empirical economics in the same way that mathematical physics relates to empirical work in its domain.* There is still a lot of methodological controversy regarding general equilibrium theory and mathematical economics and (in particular) how they relate to applied economics and econometrics. What has been determined is that the relationship is fundamentally »different« than the relationship in physics [Balzer (1982), Hamminga (1983), Handler (1980), Hands (1985a), Hausman (1981), Nelson (1989)].

9. *Regardless of how modern theorists view neoclassical microeconomics, the founders of the theory (in the 1870s) viewed the theory from a realist perspective.* In particular, the founders of neoclassical economics were *not* instrumentalist as many modern neoclassical economists claim to be. The two best examples of this early neoclassical realism are Menger and Walras; Menger was clearly an essentialist realist and Walras seemed to hold a Plantonic view [Jaffé (1980), Koppl (1989), Mäki (1989, 1990a), Rosenberg (1980)].

10. *Neoclassical explanations of the behavior of individual economic agents are a particular form of »folk psychology« (explanations in terms of beliefs, aims and desires), thus neoclassical explanations share many of the philosophical problems and/or blessings of folk psychology.* »Folk psychology,« the explanation of human behavior in terms of the »intentions« of agents, has been a topic of much recent discussion in the philosophy of

psychology and the philosophy of mind. Some philosophers argue that such intentional explanations can not be legitimate scientific explanations while others argue that such explanations are not particularly problematic. However this debate in the philosophy of mind ultimately comes out, there may be implications for microeconomics since microeconomic explanations of individual human action are a very specific form of intentional explanation. This does not mean that microeconomics will necessarily stand or fall with folk psychology since microeconomics is concerned with things other than individual behavior (like the unintended consequences of such behavior), but it does mean that the relationship requires careful examination [Mäki (1990a), Nelson (1990), Rosenberg (1981, 1988)].

11. *The maximization assumption is a basic methodological presupposition of neoclassical economics (it is not a tautology, but it is seldom falsifiable).* The assumption that agents and firms maximize is not a tautology, it is not true simply on the basis of the definitions of the terms. On the other hand, the general proposition that something is always being maximized is seldom falsifiable by observing behavior of agents or firms. The assumption is simply one of the fundamental methodological presuppositions of the neoclassical research program [Boland (1981, 1983), Caldwell (1983)].

12. *Neoclassical explanations of the behavior of agents or firms are a special case of »situational analysis.«* Situational analysis explanations are explanations of an agent's behavior in terms of the agent's »situation« and the »rationality principle« that all agents act rationally given their situation. Neoclassical explanations are special cases of this type of explanation [Caldwell (1988), Hands (1985c), Langlois (1989), Latsis (1983), Popper (1985)].

13. *Neoclassical microeconomic theory is primarily an explanatory rather than a predictive theory, while Keynesian macroeconomic theory is primarily a predictive rather than an explanatory theory.* There are no references here; this is clearly not a current point of consensus. My argument is that the so-called »symmetry thesis« — the proposition that explanation and prediction are merely two sides of the same coin — does not apply in economics. What makes neoclassical microeconomics most successful is its apparent ability to pro-

vide acceptable explanations of microeconomic phenomena; on the other hand, what makes (made) Keynesian macroeconomics most successful is (was) its ability to predict the behavior of aggregated economic variables.

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