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# Friedman's Methodology: A Puzzle and A Proposal for Generating Useful Debates through Causal Comparisons (with a postscript on positive vs. normative theories)

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## Abstract

Milton Friedman’s “The Methodology of Positive Economies” is still one of the most widely read pieces on economic methodology. One reason for this might be Friedman’s attractive proposal that economists use theories and hypotheses as pragmatic devices to summarize data and make predictions over the relevant range of observations. Logically, this should lead to a fair minded comparison among many contending theories. However, Friedman's actual examples and discussion of these examples raise a puzzle. The field of comparison seems unduly narrow from the beginning. In my attempt to resolve this, I consider some logical and ontological problems for Friedman's position. I end up by suggesting a scientific realist approach to testing theories by causal comparisons over a wide field of contending theories.

## 1. Introduction

Milton Friedman's "The Methodology of Positive Economics" was originally published in 1953. After more than fifty years it is still one of the most widely read pieces on economic methodology. Although criticisms abound, the basic framework is still widely accepted by most mainstream economists.[1] One reason for this might be Friedman's attractive proposal that economists use theories and hypotheses as pragmatic devices to summarize data and make predictions over the relevant range of observations.

Indeed, at first glance, Friedman's proposed approach seems practical, impartial and liberating. "[T]he 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 than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been "confirmed" by experience….The validity of a hypothesis in this sense is not by itself a sufficient criterion for choosing among alternative hypotheses…"[2]

Friedman continues helpfully, "… [o]bserved facts are necessarily finite in numbers, possible hypotheses infinite…The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria 'simplicity' and 'fruitfulness', notions that defy completely objective specification."[3] Here, provisionally at least, Friedman seems to be open to considering a number of different hypotheses consistent with a given body of data. Although notions such as 'simplicity' and 'fruitfulness' as Friedman himself notes are complex ideas, there is no indication at this point in his argument that these will stand in the way of being open-minded about hypotheses to consider.

However, he goes on to add that '…to suppose that hypotheses have not only 'implications' but also 'assumptions' and that the conformity of these 'assumptions' to 'reality' is a test of the validity of a hypothesis is fundamentally wrong and productive of much mischief."[4] Such anti-realism may also be interpreted as a plea for tolerance and good

[1] From my discussions with colleagues in political science and sociology, it seems that although the methodological debates in these fields are quite lively, there are sympathetic practitioners if not strict adherents in these fields as well

[2] Friedman does not distinguish between theories and hypotheses. This can lead to some "mischief", to use Friedman's own locution. There is also a confusion about theories and models as well. On these distinctions see Khan (2003).

[3] Milton Friedman, "The Methodology of Positive Economics ", in *Essays in Positive Economics* (Chicago; 1953), p. 8f, 10 and 14.

[4] Ibid, p14.

empirical procedures for testing the hypotheses in light of their predictions.

Such an openness to considering empirical fit and predictive success could presumably lead to a consideration of several competing hypotheses and theoretical frameworks in the “market place for ideas” to use a metaphor economists love. However, Friedman ends up very quickly by advocating the familiar neoclassical assumption that economic agents behave *as if* they are able to maximize (utility, profit) subject to certain economic constraints. This poses an interesting puzzle. Why does Friedman’s initial position of seeming tolerance so quickly restrict itself to endorsing the *status quo*? To put it in somewhat provocative economics language, why does the possibility of a competitive marketplace for ideas approach the monopolistic market structure so rapidly? We can also ask whether Friedman's seemingly tolerant anti-realist position might have anything to do with this bias for the *status quo*. More precisely, does Friedman's seemingly tolerant anti-realism have a logical or axiological connection with his firm endorsement of the neoclassical maximizing of utility (or minimizing of cost) postulate? The rest of this essay will attempt to answer these questions.

In what follows I first take up the idea of hypothesis testing without a realist bias. This is the best case scenario for an anti-realist. Next, I ask what leads to a limiting of alternatives under the best case scenario. Finally, I offer a well-hedged scientific realist argument for field-specific open examination of theoretical alternatives. It can be shown that such an openness requires a respect for well-confirmed theories if the science in question is mature enough. At the same time, imaginative alternatives that offer relatively more causal depth compared to the existing theories and elucidate important puzzles in the field need to be considered seriously by the social scientists in their specific areas of research

## 2. The Best Case for Friedman’s Anti-Realism[5]

Friedman proposes a seemingly straightforward test for accepting hypotheses. It is really a fit with data ultimately and an ability to predict better. Along the way, he gives an ingenious defense of the “*as if*” position of the Chicago school and argues convincingly against “behavioral” alternatives which would uphold more descriptive (but less economical) approaches to characterizing economic behavior. In the context of late 1940s, the most prominent of these “behavioral” alternatives was the theory of the firm.

Friedman’s argument here rests on the claim that these rivals to the neoclassical theory of the firm used *ad hoc* assumptions and implied much

[5] As Kaushik Basu (personal correspondence) has pointed out, one could also think of Friedman as an 'arealist'. As I discuss later, Friedman is not always consitent. However, in the essay under discussion at least, Friedman does seem to take a strong anti-realist position.. See also Basu(2000).

less of the *explananda* than did the neoclassical theory of the firm. That is the best case scenario for Friedman. Given the menu from which Friedman seemingly had to choose, the theoretical alternatives on offer were indeed poorer by comparison in spite of their claims for greater realism in their assumptions.

However, in his critique of these alternatives Friedman does not consider the tendencies of competitive markets to give rise to oligopoly or monopoly, or the problems of innovation in a perfectly competitive market. Even during the fifties there were at least two competing theories in these domains, namely the Schumpeterian theory of the innovating and creatively destructive firm and the closely related but clearly differentiated Marxian theory of extraction of relative surplus value at the point of production and capital accumulation in a classically competitive market system. The market structure of competitive capitalism in this theory gives rise to concentration. In fact, this is a striking prediction and thus this theory would seem to be a serious contender by Friedman's own criterion of predictive success. Both the theories have subsequently been refined.[6] But even in Friedman's date of writing these would have been serious contenders by his own standards defended in his essay.

Friedman's failure to consider useful and serious alternatives could be attributed to several factors ranging from the state of debate in the field which existed then (here Friedman was not alone in ignoring these theories) to Friedman's training as a Chicago economist with a greater familiarity with and an acceptance of the neoclassical postulates. Indeed, Friedman wrote a number of insightful papers on Marshallian demand curve and other related neoclassical topics. However, his methodological position still remains puzzling. How could a well-trained economist of Friedman's ability ignore two of the more serious contenders from a scientific view point in good conscience? More positively and importantly, how can the plea for tolerance become realized in the practice of the social scientists today?

## 3. (Lack of) Familiarity breeds conservatism

The key to answering the above question is in assessing the actual depth of existing theories in economics and by extension other social sciences and their actual state of empirical success. In economics, as in other social sciences, predictive success is conspicuous mostly by its absence. Although econometric techniques are continually being refined, testing hypotheses from a complex web of contaminated data is challenging at best. There are no constants in economics or other social sciences, as there are in physics or chemistry. Therefore, the key empirical

[6] See for example Iwai(2001), Khan (2002) and James and Khan (1998) and the references cited in these.

relations keep changing over time. One of the most telling critiques (by Lucas) of Keynesian macroeconomics produced by the neoclassical synthesis of Hicks-Samuelson-Solow variety has been based precisely on the shifting of key parameters in economic models as agents anticipate changes in policy.

Thus Friedman and others fall back at the end on simple models that are familiar from training and field-specific professional socialization. Departures from the familiar principles of market produce complications. The virtue of anti-realism in this context is also clear. The anti-realist does not claim that the theoretical concepts are true. In early twentieth century philosophy of science, Poincare's *Science and Hypothesis* (1903) is the classic defense of this position.

However, in physics failure to predict by one theory and predictive success of alternative theories can frequently lead, and historically has led, to theory changes. Even Poincare acknowledged the reality of molecules after Einstein's characterization of Brownian motion within a kinetic-molecular theory of fluids was experimentally verified by Perrin and his associates.

In case of economics the situation has actually become worse than it was in Friedman's youthful days when he penned his "Methodology" piece. A less exuberant Friedman laments without any sense of irony that "economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems" (Friedman, 1999, p.137; see also Leontief (1982), for an even earlier lamentation by a Nobel laureate about the state of economics without being able to pinpoint the causes); Another Nobel laureate Ronald Coase complains: "Existing economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world" (Coase, 1999, p.2). A prominent game theorist ruefully remarks (Rubinstein, 1995, p.12):

"The issue of interpreting economic theory is... the most serious problem now facing economic theorists. The feeling among many of us can be summarized as follows. Economic theory should deal with the real world. It is not a branch of abstract mathematics even though it utilizes mathematical tools. Since it is about the real world, people expect the theory to prove useful in achieving practical goals. But economic theory has not delivered the goods. Predictions from economic theory are not nearly as accurate as those by the natural sciences, and the link between economic theory and practical problems... is tenuous at best."

Rubinstein goes further:
"Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question 'where does it lead?'"

The methodological problem extends beyond just the discipline's lack of direction and predictive power that turns out to be much more limited than Friedman's generation had believed. Honest econometricians despair about the divergence between econometric theory and empirical practice. Thus arises another lamentation from Leamer (1978, p.vi), a widely respected econometrician:

“The opinion that econometric theory is largely irrelevant is held by an embarrassingly large share of the economics profession. The wide gap between econometric theory and econometric practice might be expected to cause professional tension. In fact, a calm equilibrium permeates our journals and our meetings. We comfortably divide ourselves into a celibate priesthood of statistical theorists, on the one hand, and a legion of inveterate sinner-data analysts, on the other. The priests are empowered to draw up lists of sins and are revered for the special talents they display. Sinners are not expected to avoid sins; they need only confess their errors openly.”

Given the lack of predictive success in economics as well as in other social sciences, Friedman’s methodology as well as the current practice of economists both tend to be biased towards the familiar. Friedman knows as a creative scientist that familiarity is field-specific and depends largely on early training and continuing socialization through career-building incentives, and hence the need for approval from more established colleagues within the field. He contrasts the approaches of neoclassical economists with the theoretical inclinations of sociologists in the following example:

“Consider … the hypothesis that the extent of racial or religious discrimination in employment in a particular area or industry is related to the degree of monopoly in the area or industry in question… This hypothesis is for more likely to appeal to an economist than to a sociologist. It can be said to ‘assure’ single-minded pursuit of pecuniary self-interest by employers in competitive industries. And this ‘assumption’ works well in a wide variety of hypotheses in economics… It is therefore likely to seem reasonable to the economist that it may work in this case as well… *[T]he background of the scientists is not irrelevant to the judgment they reach*… [T]he weight of evidence…can never be assessed completely "[7] (Emphasis mine)

Friedman’s admission and defense of this field-specific bias, together with the demonstrable lack of predictive success in economics and in other

[7] Friedman, Ibid. p29

social sciences would seem to result in an impasse. If we follow Friedman in practice and support familiarity then we do not seem to have any way to go beyond the neoclassical postulate in microeconomics and orthodox postulates in other social sciences.

Note that Friedman makes no claim about the truth-value of his neoclassical postulate. But in reality, we are to act *as if* the postulate is true. Therefore, there is an axiological connection with how an economist might correctly analyze specific modes of economic behavior. In case of Friedman's own school, this also led to specific policy positions as well.

**4. What then remains for the theorist to do?**

Friedman’s own move is to use this anti-realism to make the diversity of frameworks irrelevant except in a very narrow professional way that shuns controversial theories often for ideological reasons and then offers a pseudo-philosophical justification for this practice. His dictum is to opt unambiguously for the familiar overall hypothesis. He could argue (although he does not) that choosing this familiar theory within which to work can efficiently allocate research time and facilitate collaboration.

Although there are situations where sticking with the familiar can save us from crackpot theories and economize on scarce research time, it can be argued that the social sciences as a whole are not mature enough to require researching under the assumptions of only one theory without even bothering to test whether the theory is even approximately more true than other reasonable alternatives. In the present state of the social sciences, foreclosing alternatives too soon can only lead to an impoverished menu of alternative hypotheses.

Before I move on to present an alternative methodology, let me quickly point out one strength of Friedman when compared with the logical positivist literature on explanation and prediction at the time of his writing. In the literature on the covering law model of explanation and prediction (these were taken to be symmetric in logical form) that was prevalent then, a great deal of effort was expended on defining the logical forms of explanation and prediction. According to this model, an adequate explanation of why something happened fit one of two patterns. For the sake of brevity, I will discuss only the first, non-statistical explanatory pattern here.

In the first, "deductive-nomological pattern,” or D-N model, empirical general laws and statements of initial conditions are premises which jointly imply the statement that the event in question has occurred. Therefore, logically (not necessarily ontologically), it had to happen.

In the illustration presented by C. G. Hempen[8,] the thinker who probably developed the covering law model in the greatest detail, with

[8] See for example, Hempel (1965)

the greatest clarity and philosophical acumen:

> Let the event to be explained consist in the cracking of an automobile radiator during a cold night. The sentences of group (1) [i.e., the "set of statements asserting the occurrence of' certain events . . . at certain times"] may state the following initial and boundary conditions: The car was left in the street all night. Its radiator, which consists of iron, was completely filled with water, and the lid was screwed on tightly. The temperature during the night dropped form 39°F, in the evening, to 25°F in the morning; the air pressure was normal. The bursting pressure of the radiator material is so and so much. Group (2) [i.e., the "set of universal hypotheses"] would contain empirical laws such as the following: Below 32°F, under normal atmospheric pressure, water freezes. Below 39.5°F, the pressure of a mass of water increases with decreasing- temperature, if the volume remains constant or decreases; when the water freezes, the pressure again increases. Finally, this group would have to include a quantitative law concerning the change of pressure of water as a function of its temperature and volume. From statements of these two kinds, the conclusion that the radiator cracked during the night can be deduced by logical reasoning; an explanation of the considered event has been established.

This appealing approach taken entirely from physics and generalized to apply to all sciences has misled many generations of social scientists. The basic problem is that many perfectly good explanations in history, anthropology, sociology, political economy and related fields do not fit the D-N model even approximately. Even in the natural sciences geology and biology can offer explanations that are seriously discussed within the fields but also do not fit this model.

It is to Friedman's credit that he stays away from putting explanations and predictions in such a logical straitjacket. His claim that predictions should be the ultimate arbiter also does not rest on any claim about the symmetry between the D-N model's explanatory scheme and predictions. Friedman has been accused of being an "instrumentalist" by commentators who want to label him according to the accepted terminology of philosophy of science developed during the great logical positivist debate.[9] While the label may not be inaccurate, whether or not Friedman fits some philosophy of science label better than others is largely irrelevant from the

[9] However see Boland (1979) and Hirsch and de Marchi (1990) for a fair-minded treatment of Friedman on this issue.

point of view of substantive research in social science. In fact, as Kevin Hoover and some others have noted, Friedman seems to have been both untutored in philosophy of science and an entirely original thinker who thought he had developed a pragmatic test for validity of theories and hypotheses based on predictive success alone.[10] Although flawed in many ways, this was probably a better move than importing sterile philosophy of science terms and debates into economics and more broadly, the social sciences. While informed methodological discussion is valuable for its own sake and there are some excellent examples in economics[11] and other social sciences, it is a regrettable fact that a mechanical application of trendy philosophy of science terms characterizes much methodological discussion even today. Quite predictably, this move has so far failed to advance the social sciences even methodologically, leaving alone any methodological aid towards making substantive progress in specific areas of controversy. From this perspective, in order to be fair to Friedman, it could be said Friedman's methodology fails on a higher ground than most methodologists today claim to succeed on.

Friedman fails in mainly two broad areas. First, he does not consider enough options with regards to the menu of theories and hypotheses to be tested. Second, and equally important, he fails to consider criteria for testing other than predictive success which may point nevertheless to explanatory validity. Thus we can reformulate a criticism by Daniel Hammond (1991) of Friedman's argument so that it emphasizes this exclusionary aspect. According to Hammond, Friedman's argument is:

1. A good hypothesis provides valid and meaningful predictions concerning the class of phenomena it is intended to explain. (Premise)
2. The only test of whether a hypothesis is a good hypothesis is whether it provides valid and meaningful predictions concerning the class of phenomena it is intended to explain. (invalidly from 1)

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[10] Hoover (2004) goes even further and offers an intriguing argument that Friedman was a causal realist with evidence from Friedman's substantive scientific work. However, even Hoover is aware that the dominant interpretation of Friedman's methodological essay--- which certainly has influenced actual practice of subsequent researchers--- is anti-realist.

[11] See for example, Lawson(1997, 2003). In particular, the characterization by Lawson and others in the critical realist school of the task of methodologists as that of philosophical underlaborers is both sensible and modest. The scientific realist idea of causal comparison presented here owes much to Miller(1987) who also acknowledges his debt to earlier works. For example, Richard N. Boyd, "Realism, Underdetermination, and a Causal Theory of Evidence,"*Nous* (1973)pp . 1-12; Alvin I. Goldman, "A Causal Theory of Knowing,"*Journal of Philosophy* (1 967), pp**.** 355-72; and Gilbert Harman, "The Inference to the Best Explanation,*" Philosophical Review* (1 965), pp. 88-95. But both Miller and I depart from certain generalizing and *a prioristic* tendencies of these works by stressing the role of particular debates in specific substantive areas of a particular science

3. Any other facts about a hypothesis, including whether its assumptions are realistic, are irrelevant to its scientific assessment. (Trivially from 2)

If (1) the criterion of a good theory is narrow predictive success, then surely (2) the test of a good theory is narrow predictive success, and Friedman's claim that the realism of assumptions is irrelevant follows trivially. This is a tempting and persuasive argument.

But it is fallacious. (2) Is not true, and it does not follow from (1). Thus predictability may not be sufficient for testing. In some cases, the theory of evolution being the most famous, it may not even be necessary or possible to predict in order to validate the theory.

Furthermore, Friedman is only willing to consider hypotheses that conform to the unstated (but implied by his examples) constraints that exclude many contenders for causal comparison. Such comparisons are necessary especially when new alternatives are presented in any science. Darwin's arguments about origins of species, Newton's mechanical theory of gravitational force and the kinetic-molecular theories all engaged in such comparisons for a protracted period and were finally declared to be the better alternatives. In economics neoclassical economics is entrenched in an uncontested position largely as a sociology of science phenomenon, not because of an ability to furnish better causal mechanisms compared to those of all serious contenders, or because of better ability to predict.

Finally, Friedman's influence has also discouraged economists from asking ontological questions. In fact through the escape clause "as if" it has permitted the economists to act in good faith and ignore crucial ontological questions regarding the status of key theoretical entities. Thus the Canadian philosopher Maurice Lagueux correctly points out:

> Friedman's methodological attitude was effective in legitimizing economists' refusal to raise cumbersome questions about the character of the world they were analyzing, but such a systematic flight from ontological questions could hardly satisfy everybody in the profession for very long. Consequently, in the last decades, various attempts were made to anchor this theory in a more concrete world. The recent revival of sympathy for institutionalism was surely a manifestation of dissatisfaction with the purely formal approach to economics. However, in the context of a science rather inimical to sociologism, institutionalism is easily associated with the psychological analysis of behavior. Thus, with researchers like Herbert Simon, the analysis of beliefs and goals came back in the foreground; but this analysis was developed in the institutionalized context of bounded rationality. There is no doubt that such analyses are not of an ontological character, but they do go a long way toward bridging the gap left by Neoclassicals between economic analysis

> and the psychological entities which are supposed to play the role of the exchangers in mathematical models.[12]

The thrust of the argument so far has been to show that Friedman's practice in the methodology piece itself is too narrow. Wider comparisons on the basis of the causal structure of explanations could overcome this difficulty. Although in the methodology piece itself Friedman holds to a 'prediction only' point of view, there is textual evidence from Friedman's own substantive scientific work that he has at least at times a causal approach. Thus there is at least some inconsistency between his preaching in the methodology essay and his scientific practice. Friedman's own attempts at causal explanation are far from trivial or oversights on his part. Kevin Hoover has argued that in spite of his methodological stance and his reluctance to use causal language, Friedman's best scientific work does invoke causal mechanisms at crucial points in his explanatory frameworks. One example is the velocity of money which plays a significant causal role, in this view, in Friedman's monetarist theory. Another example is the theory of permanent income. According to Hoover,

> Permanent income provides another – and perhaps more compelling – example.
>
> Like velocity, permanent income is not directly observable, but is indirectly measured and validated in the context of the consumption function. Permanent income is not only a causally significant category in this context, but Friedman regards it as sufficiently freestanding and independent of its original context, that he routinely uses the measured quantity in other contexts. For example, Friedman takes permanent income to be a causal determinant of the demand for money (i.e., permanent velocity = permanent income/money).[13]

One could multiply such examples drawing upon Friedman's treatment of the role of information, his invocation of 'the natural rate of unemployment' and other features in his explanatory schemes of various macroeconomic phenomena. Therefore, Friedman's own practice illustrates the key role causality plays in our efforts at explaining. If one accepts this proposition and takes causal explanations seriously, then from hereon, the argument for causal comparisons is really an argument for scientific tolerance within the limits of reasonableness.

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[12] Lagueux (undated), p.14
[13] Hoover (2004), p. 11

At this point, the question may legitimately be raised as to what a fair causal comparison could be like and ask for some detailed examples. Fortunately, many examples are available from the history of physical and biological sciences. Richard Miller(1987) cites several of these. I have chosen his example from Darwin because it is particularly lucid and requires no mathematical background to understand it fully.

As Miller reminds us,

> Good arguers are only as explicit as they have to be, given actual dangers of question-begging and confusion. One would expect the underlying nature of confirmation to be most explicit when the dangers of evasion and confusion are at their highest because an investigator is defending a novel hypothesis or seeking to resolve in a novel way an issue that has divided scientists into different schools of thought. In fact, it is at these junctures that the surface facts of argument best illustrate the idea that confirmation is causal comparison[14]

.

He then goes on to quote the famous passage from Darwin where he reasons about his theory and the most important alternative. His argument about bats is, as Miller correctly states,"… especially elegant, but otherwise typical example of Darwin's reasoning from his data:"

> I have carefully searched the oldest voyages, but have not finished my search; as yet, I have not found a single instance, free from doubt, of a terrestrial mammal (excluding domesticated animals kept by the natives) inhabiting an island situated above 300 miles from a continent or great continental island; and many islands situated at a much less distance are equally barren. . . .
>
> Though terrestrial mammals do not occur on oceanic islands, aerial mammals do occur on almost every island. New Zealand possesses two bats found nowhere else in the world: Norfolk Island, the Viti Archipelago, the Bonin Islands, the Caroline and Marianne Archipelagoes, and Mauritius, all possess their peculiar bats. Why, it may be asked, has the supposed creative force produced bats but no other mammals on remote islands? On my view this question can be easily answered: for no terrestrial mammal can be transported across a wide space of sea, but bats can fly across. [More specifically, bat transport occurs to provide a basis for speciation through natural selection, but occurs so infrequently that variants on remote islands are not overwhelmed by migrants from the more competitive mainland.][15]

It is important to emphasize here that Darwin is engaging in a particularly significant type of causal comparison in this example. As Miller points out:

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[14] Miller(1987) p.164

[15] Quoted from ***The Origin*** *of* Species in Miller(1987) p. 164

> Here as throughout the book, Darwin is comparing his favored hypothesis of speciation through natural selection not with the mere supposition of its falsehood but with rival hypotheses about the factors at work in the phenomena. The existence of islands with terrain hospitable to terrestrial mammals lacking such endemic species is important because the main rival is the hypothesis, dominant among the best-informed secular-minded scientists of the time, that species are created, without ancestors, by a force that makes them well-adapted to their environments. Also, Darwin makes his argument on the basis of principles he shares with the other side, for example, the shared principle that offspring are like their parents but subject to small variations, not the tendentiously anti-creationist, though plausible principle that a complex organism must be the offspring of another. Finally, Darwin is not claiming to have a complete explanation of the phenomena in question, although he certainly thinks that the complete answer would entail the approximate truth of his natural-selection hypothesis. Elsewhere, he makes it clear both that the mechanisms of heredity and variation are mysterious to him and that there is no way of predicting how an observed advantage will affect the actual course of speciation. The issue for him is whether the best available account of the data, however vague or incomplete, entails the superiority of the natural selection hypothesis over its current rivals.[16]

This type of argumentation is also common in physics when the contest among rival theories demands causally explicit comparisons. An example is Newton's contrast of the causal mechanisms in his celestial mechanics with rival accounts such as Cartesian vortex theories and Tycho Brahe's system. His discussion of comets in his summary pamphlet, " The System of the World", makes this clear. Indeed, comets are most appropriate for the purpose of causal comparison in this context. All the rival theories in this example share the principles of geometric optics. By using these non-controversial shared principles Newton could derive important features of the orbits of comets. Once these orbits are derived mathematically, it then can be argued that the Ptolemaic celestial spheres found in Brahe's descriptive geocentric theory can not really exist. For if they did, then surely comets would collide against them. Likewise, the Cartesian vortices can not be the agents that move the planets and other celestial bodies. Newton observes that comets follow a dynamic trajectory through all parts of the sky which is inconsistent with the dynamics resulting from a vortex.

One could easily multiply such examples from the mature sciences. I hope the above examples demonstrate the need for taking rival theories seriously and for establishing alternative causal mechanisms which can be examined by using techniques of observation and logical procedures

[16] Miller (1987) pp.164-65.

which are accepted as fair by all sides. It should also be noted that this is not simply a plea for mainstream theorists to take seriously the causal mechanisms postulated by the non-mainstream theories. The argument is symmetric. In fact, there are grounds for stressing that the challengers to the mainstream theories need to spell out the causal mechanisms of both their own alternative theories and the rival mainstream theories as well as the shared principles among these alternatives.  Much dogmatism in social sciences can be avoided if rival theorists were to make explicit the causal mechanisms and the grounds for what would comprise a fair causal comparison among rival theories. Of course, even after clarifying shared principles, there will be substantive areas of disagreement among contending theories. However, in this instance at least, the discussion of substantive disagreements and their possible resolution can proceed without talking at cross-purposes.There are more difficult cases where the framework principles themselves are in dispute.For such cases, it is necessary to develop a detailed theory of confirmation that would rely on topic-specific rules within a field or sub-field of inquiry rather than some global a priori or deductivist general rule ( for example, the failed logical positivist attempt to offer such global rules of confirmation for all sciences). While such a theory of confirmation for economics is yet to be fully developed, the approach defended here would call for a consideration of specific debates in substantive areas in order to develop such specific principles of confirmation. In particular, the demands for causal depth in specific theories would have to play a critical role in developing these topic-specific principles.

## 5. Conclusion: the need for open-minded causal comparisons

The conclusion that we are led towards by considering the logical, ontological and sociological issues involved in Friedman's methodology is that an open-minded and tolerant approach to multiple contenders for "the most approximately true" explanatory framework for any given social science phenomenon ( or a set of such phenomena) is essential for progress in these sciences. Friedman's plea for being more aware of methodological issues in the social sciences is well taken. However, his seeming lack of awareness of the many serious contenders aside from neoclassical theory in his own field exposes a serious weakness in his approach. The idea of following a two step procedure where all or almost all contenders are allowed in the first stage as possible *explanans* candidates is to overcome this narrowness. But this still leaves us with a serious issue to resolve at the next stage, namely, confirming which one of the contenders is most nearly a true explanation.

Here, I have argued for a tolerant scientific realism which will proceed through causal comparisons. While the first stage in my suggested alternative methodology is quite permissive and open-minded, this second stage of causal comparisons will need to be truly rigorous given the standards of the particular field in question at the time of comparison. The role of auxiliary principles relating to issues such as instrumental reliability, the reliability and usefulness of historical as well as statistical and other methods in the social sciences is particularly important here. While it was not the intent of this note to develop an alternative theory of confirmation for the social sciences, enough has been said about the narrowness of Friedman's anti-realism and his exclusive focus on prediction to suggest that rigorous causal comparisons will allow comparisons at several levels including the assessment of the realism (or more precisely, the ontological status[17]) of theoretical entities along with explanatory and predictive successes of alternative theories.[18] Spelling out these criteria further and illustrating them with examples from both the natural and the social sciences will be the task of a future paper.

## A Short Postscript on Positive vs. Normative Theories

Friedman explicitly refers to "positive" economics in his essay. The intended contrast, of course, is with the so-called "normative" economics. In standard neoclassical formulation, "normative" merely refers to a specific type of valuation exercise through the concept of Pareto optimality. In the Hicks-Pareto ordinal utility formulation, this is a very weak utilitarian valuation principle. There are stronger principles available including Rawlsian and capabilities approaches.

Friedman's move to isolate only the positive part of economics reflects a misguided belief among some economists that only "positive" economics is amenable to scientific testing. An extreme view is that all normative theories are subjective. Although the Pareto principle relies on a radically subjectivist formulation of utilitarianism, even this version,

[17] For helpful discussions by a philosopher, see Lageux (undated, 1994)
[18] For a very helpful general philosophy of science discussion see Miller, *op. cit.*

arguably, can be defended by relying on metaethical arguments. Aristotlean and Hegelian formulations of capabilities approach can be and indeed have been defended from an "objectivist" standpoint(Khan1998).

More broadly, the narrowness of Friedman-type positive-normative distinction would exclude much interesting normative economic theory including most of Adam Smith's defense of the liberal sytem of political economy. Ironically, Friedman who is a self-described"classical liberal" seems to be unaware of the implications of his discourse on the methodology of positive economics for Smith's classical version of liberalism.

I am aware that the above remarks are woefully inadequate to deal with the many facets of the positive-normative distinction. These remarks also touch merely the surface of the deep subject of normativity in economics. I intend to write a longer piece on this in the future. If the few remarks here stimulate further thinking in this direction, I will consider my present labors amply rewarded.

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