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Current Views on Economic Positivism

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Few economists today will be found waving the banner of 'economic positivism' or 'positive economics'. Does this mean that economic positivism is dead? Certainly not. Positive economics is now so pervasive that every competing view (except hard-core mathematical economics) has been virtually eclipsed. The absence of methodology flag-waving is thus easy to understand. There is no territory to dispute and thus no need to wave one's flag.

The dominance of economic positivism is abundantly evident in current textbooks. Almost every introductory textbook explains the difference between 'positive' and 'normative' economics and tries to make it clear that economists are interested in positive economics and capable of fulfilling the demands of economic positivism. Why should economists be interested in positive economics? And has economics fulfilled the demands of economic positivism? These two questions will be the focus of this essay.

POSITIVE ECONOMICS VERSUS WHAT?

While every textbook clearly distinguishes 'positive' from 'normative' questions by characterizing the distinction with an 'is/ought' dichotomy, it is not clear that the history of the distinction supports such a dichotomy. John Neville Keynes is most often quoted to support the dichotomy despite the fact that, as Mark Blaug [1980: 141] points out, Neville Keynes actually provides a threefold distinction involving (1) the 'establishment of [existing empirical] uniformities, (b) the 'determination of ideals' or the 'criteria of what ought to be', and (c) the development of a practical 'art' to formulate 'maxims or precepts by obedience to which given ends may *best* be attained' [see Keynes 1917: 32–5, emphasis added]. Unfortunately, the widespread reliance on the 'is/ought' dichotomy has nullified Neville Keynes' best efforts to improve our understanding of positive economics.

While promoting 'positive methodology' in his famous 1953 essay, Milton Friedman tried to deny the 'is/ought' dichotomy by arguing that answers to 'ought' questions necessarily depend on a prior establishment of 'what is'. Nevertheless, most critics of Friedman's methodology think he was arguing against normative economics and thus assume that he was only arguing in favour of positive economics [e.g., Koopmans 1957, Rotwein 1959, Samuelson 1963 and Simon 1963]. The presumption seems to be that one must always choose between 'is' and 'ought' questions as if they are inherently mutually exclusive.

To be fair, there is a good reason to presume that 'is' and 'ought' questions are mutually exclusive. David Hume had long ago argued that 'ought' statements cannot be deduced from 'is' statements and vice versa [see Blaug 1980: 130]. The mere mention of 'is' and 'ought' in the definition of positive economics thus seems to demand a sharp dichotomy such as the one between positive and normative economics as defined in the textbooks.

In addition to the is/ought distinction, there are other dichotomies that seem to support the separation between positive and normative economics. There is the philosopher's distinction between analytic and synthetic truths – the former being ones that do not depend on empirical questions while the latter do. There is the science-vs-art distinction which motivated early economic methodologists (such as Nassau Senior) – while 'science' was alleged to be about material truths, 'art' was considered to be about normative rules [Blaug p. 59]. More recent dichotomies are the objective/subjective, descriptive/prescriptive and rational/irrational which are often considered direct correlates with the positive/normative distinction. And, of course, there is the more commonplace distinction between theoretical and applied economics that prevails in most economics departments today.

To this list I wish to add one more distinction – namely, the romantic/classical distinction often found in discussions of 19th-century British literature. Specifically, I think one can recognize a distinction between 'romantic' and 'classical' postures concerning the realism of assumptions. While it might be considered romantic to assume the world is the way one would like it to be, it would be classical to dispassionately try to make one's assumptions correspond to the way the world really is. For example, a romantic egalitarian might wish that wealth be evenly distributed, a classical realist would contend that one should not assume distributional uniformities unless there are good empirical reasons to do so.

So, given all of these various dichotomies, how does one understand the nature of positive economics and why should one ever want to promote it? I think the reason why there are so many different distinctions raised in the discussion of positive economics is that each of them represents something that positive economics is claimed *not* to be. That is, most people understand positive economics more by what it is argued not to be than by what it is argued to be in fact. Briefly stated, we have only a negative understanding of economic positivism!

POSITIVISM AS RHETORIC

There is a sense in which the distinction between positive and normative is completely confused. Positive policy advisors are in effect always recommending that their policy is the *best* way to achieve the given ends. This is evident even in John Neville Keynes' original discussion. It is difficult to conceive of a way one could ever avoid normative judgements. So, what is it that one is truly accomplishing when demanding that one's economic research or advice conform to the dictates of positivism?

The idea of 'positive' economics is mostly a matter of rhetoric [see further, Boland 1989, epilogue]. The rhetorical purpose is also evident in the use of some of the other dichotomies. One can find books titled 'System of Synthetic Philosophy' [Herbert Spencer 1896], 'Positive Philosophy' [Auguste Comte 1855/1974], 'Scientific Management' [Drury 1922/68], 'Objective Psychology of Music' [Lundin 1967], 'Rational Economics' [Jackson 1988], 'Descriptive Economics' [Harbury 1981], and so on. Whenever an author is extolling the virtues of a theory by claiming it is a positive theory, he or she is usually asserting that it is not something of a scientifically unacceptable nature. What is acceptable in these matters is usually dictated by the prevailing view of 'scientific method'. But it is not often clear why the term 'positive' must always indicate something acceptable or desirable.

Up to the time of Hume (late 18th century), most thinkers seemed to believe in the power of rational or logical thought and especially in its embodiment in science. And by the term 'science' it was usually implied, following Francis Bacon's 17th-century view, that all science can be reduced to *positive evidence* from which in turn all systematic knowledge could be shown to follow by the logic of *induction*. This, I think, gives us a clue to why the accolade of 'positive' has for so long implied something good. Any theory which offers or is based on positive evidence – that is, on observations or hypotheses which make *positive contributions* toward an inductive proof of one's systematic knowledge – is worthy of the title 'positive'. And given the common 19th-century belief in the viability of inductive science, 'positive' implied 'scientific', 'rational' and even 'objective'. The implication of objectivity follows from Bacon's promotion of inductivism as antidote to self-interested or prejudicial claims of knowledge [see Agassi 1963]. To be scientific, inductive proofs were to be based only on objective observations. Whether one's theory makes a positive contribution to scientific knowledge is solely a question of one's personal research skills. A true scientific researcher is objective, unprejudiced, unbiased to the point that any reported data will be beyond question. The remainder of science is simply a matter of objectively based inductive logic. As a corollary, if anyone errs in their scientific claims to knowledge, it could only be due to introduced biases, prejudice or injecting one's subjective values.

In economics, the association between 'positive' and 'descriptive' seems to be a direct consequence of the reliance on Hume's view of the is/ought dichotomy. One describes 'what is' and prescribes 'what ought to be'. The association between 'positive' and 'applied' economics and between 'positive' and 'synthetic' statements is rather confusing. While it is easy to claim that one's theory is 'positive', it is more often thought that pure theory is not empirical [see Hutchison 1938] and thus applied economics must be 'positive'. So, what did Böhm-Bawerk mean by the title of his 1889 book *Positive Theory of Capital*? While it might be easy to see a connection between 'positive' and 'synthetic', their opposites do not seem connected. Hardly anyone would connect 'normative' with 'analytical' – except from the perspective that a normative conclusion is a logically contingent truth that depends on the acceptance of presumed values. But if analytical truths must be tautologies then, technically, the connection is rather weak [see Quine 1953/61, Chapter 2].

The post-war influence of the Logical Positivists and the retrospective influence of Max Weber have combined to make the rhetoric of positivism even more confused. The Logical Positivists were those analytical philosophers who thought verifiable scientific knowledge is distinguishable from unverifiable 'metaphysics'. The turn-of-the-century social scientist Max Weber is now credited as being a leader in developing the idea that scientific knowledge could be 'value free'. And to confuse things still more, Karl Popper presented a critique of Logical Positivism based on the logical grounds that one's theory makes a positive contribution to scientific knowledge only if it is falsifiable (which most commentators seem to think means only that it is not a tautology). With all this confusion in mind, it may be difficult for us to determine even what 'positive' is not.

WHAT EVERYONE SEEMS TO THINK 'POSITIVE' IS

Economic positivism as it is currently practiced seems to be available in four different flavours. The first and most optimistic version is what I will call *Harvard positivism*. It is represented by the recent attempts to develop 'experimental' economics and has its origins in the early teaching of Edward Chamberlin. At the other extreme is the weak minimalist version which I will call *MIT positivism*. Its weakness is due to the methodological view that says that to be of interest a theory need only be *potentially* refutable – there is no additional requirement that says it needs to be supported or tested by empirical evidence. In between these two extremes there are two more modest versions. One is what I will call *LSE positivism* which does not require controlled experiments but does see economics as a scientific endeavour that emphasizes a necessary role for empirical, quantitative data. The other one is *Chicago positivism* which includes both the simplistic instrumentalism of Friedman and the more complex confirmationism of Becker and Stigler.

Harvard positivism

Those positivists who advocate 'experimental economics' still comprise a very small segment of mainstream economics. The current movement seems to have its origin in the experiments that Chamberlin often inflicted on his students at Harvard University. The current leader of this group is Vernon Smith [1982].

The motivation for experimental economics is to overcome the obvious fact that most mainstream neoclassical models are self-professed abstractions which employ simplifying assumptions whose realism is always open to question. Given that any typical economic explanation is of the form 'if the world is of form X and people behave according to proposition Y, then we will observe phenomena Z'. The obvious questions facing any economist who claims to offer a positive explanation of economic phenomena Z are: Is the world of form X? Do people in fact behave according to proposition Y? And do we observe phenomena Z?

Since it is usually difficult to determine whether people actually behave according to proposition Y, almost all empirical research is concerned with world X and phenomena Z. The usual approach is to build a model of the economy based on proposition Y and try to determine whether or not the model can be confirmed when confronted by the data *available* after the event. Unfortunately, the available data are seldom decisive in any direct way. Instead, many additional assumptions must be made and thus any conclusions reached are always conditional.

Harvard positivism offers a different approach. Rather than accept the limitation of available data (which are usually aggregative and thus open to many methodological questions), experimental economics proposes to create a real-world situation in which the assumptions of the typical neoclassical model are true with respect to the claimed form of world X. Specifically, the experimental economists attempt to construct a world which is in fact of form X and then determine whether the behaviour implied by proposition Y is logically consistent with the experimentally observed phenomena Z. The extent of the laboratory skill of the experimenter is always the sole determinant of whether the experiment represents a successful exercise in economic positivism.

MIT positivism

The followers of Paul Samuelson’s methodology adopt a much less fundamentalist view of economic positivism. Following Samuelson yet seeking to assure the optimistic promises of positivism, it is argued that the minimum condition for a positive contribution to economic understanding is that anyone’s positive theory must be capable of yielding to refutations based on positive evidence. In short, all truly positive theories are empirically refutable *in principle*. All that can be assured by such a weak requirement is that the proposed positive theory is not a tautology – as Hutchison [1938] recognized in the late 1930s. It should be clear that this minimalist version of positivism is serving more the interests of mathematical model-builders, who wish to avoid all of the menial unpleasantness of dealing with complex real-world empirical data, than the interests of those who are concerned with promoting truly positive economics. For the mathematical economists, the elegance of one’s model is always much more important than whether the model’s assumptions are empirically realistic or whether the model’s implications are useful with respect to economic policy.

Chicago positivism

Usefulness is the keystone of the positivism promoted by the followers of Chicago school economics. However, there are two aspects of usefulness. On the one hand, providing positive theories that can be used as instruments by policy makers is one concern. On the other hand, being useful for promoting neoclassical economics in general, and confirming beliefs in the omnipotence of the market system in particular, is another concern of the Chicago school.

In his 1953 essay, Friedman gives a compelling argument for why anyone who is only interested in providing useful theories for policy makers ought to eschew the typical philosophical prejudices associated with the group of analytical philosophers often called ‘logical positivists’ and instead recognize that questions concerning the verifiability, falsifiability, or even a priori realism of the behavioural assumptions of economic models is of much less concern than the usefulness of their results. It is easy to see that such an argument is really one favouring an instrumentalist methodology [see Boland 1979]. The interesting question is, why would Friedman or anyone else see his argument as one promoting some form of positivism?

Friedman’s essay was not an argument against positivism but only one against the more sophisticated logical positivism. Positive evidence still matters for Friedman. His only restriction is to limit the evidence to that of results or predictions and thereby exclude a priori or logical analysis of models, assumptions and theories *as a determinant of the usefulness* of positive theories. Positive data obviously play an essential role in Friedman’s methodology. But for

Friedman the only relevant positive data will be successful predictions which assure the usefulness of one's model or theory. There is nothing inherent in Friedman's methodological essay that would prevent his form of instrumentalism from being used by Post-Keynesians or even Marxists.

When it comes to ideological questions, however, other members of the Chicago school are much more prominent. In 1977, George Stigler and Gary Becker offered a manifesto for those who believe in neoclassical economics. Their argument, simply stated, was that the Chicago school economists will offer models of the economy (i.e., of world X) which do not engage in analysis of the psychological (subjective) makeup of individual decision-makers but instead offer analyses of the objective (positive) cost situations facing the individual decision-makers and thereby explain any observable, positive behavioural evidence in question (i.e., phenomena Z) – all observed changes in behaviour will be explained as consequences of observable and objective cost situations [Stigler and Becker 1977].

Each positive economic model which succeeds (they never seem to report any failures) is offered as yet more confirming evidence that one can explain any social or behavioural phenomena with an appropriately constructed neoclassical model (i.e., where proposition Y incorporates assumed maximization behaviour in a free market system). For this branch of the Chicago school, the real purpose of neoclassical model-building is once again to confirm the truth of a market-based system of social coordination [Boland 1982].

LSE positivism

Stigler and Becker may be correct in promoting neoclassical economics as the only true explanation of social and individual behaviour, but if so, it ought to be tested in a more critical manner. At the end of the 1950s, a group of LSE economists proposed a more critical approach to economic model-building. While it is easy to find positive evidence to confirm anyone's favourite model, the 'scientific' issue is one of approaching the evidence in a less predisposed manner. Such an approach does not preclude a priori beliefs, it merely cautions one to let the positive evidence do the talking.

The LSE approach to positivism was the self-conscious product of a group of young economists led by Richard Lipsey who formed what was called the 'LSE Staff Seminar in Methodology, Measurement and Testing'. The seminar was to some extent inspired by Popper's presence at LSE and his emphasis on criticism and empirical testing as the true basis for science [see de Marchi 1988]. The message of the seminar was captured in Lipsey's well-known 1960s textbook, *Introduction to Positive Economics*. The main thrust for Lipsey was the advocacy of developing an appreciation for real-world empirical data. His textbook became the major platform for all of modern economic positivism.

The combination of testing and measurement is the hallmark of LSE positivism. It is thus not surprising to find that econometrics plays a prominent role. But unlike the instrumentalist tendency found among American econometric model-builders [see Boland 1986], LSE econometrics is supposed to be helping us to assess any economic proposition that might arise. The positive/normative distinction was to play a central role since it was thought that all normative statements are untestable and thus 'unscientific'.

MODERN ECONOMIC POSITIVISM IS PROFOUNDLY CONFUSED

As we have learned from historians of science (such as Thomas S. Kuhn and Joseph Agassi), most disciplines can be defined by their leading textbooks. The foundation of modern economic positivism continues to be Lipsey's textbook, *Introduction to Positive Economics*. The evolution of this book closely reflects how the practice of positivism has developed over the last 25 years. However, if one examined the introductory 'Scope and Method' part of the *first* edition of Lipsey's famous textbook, it would be difficult to understand how this book has become the foundation for modern economic positivism. Lipsey proudly announces that his book is about

‘POSITIVE ECONOMIC SCIENCE’. The North American editions of his book play down the emphasis on ‘science’ (presumably because in North America such emphasis is considered pretentious) but then continue to share his emphasis on ‘positive’. Yet, a careful examination of his 1963 book shows that empirical evidence can be decisive *only in a negative way*. Specifically, Lipsey parrots the part of Popper’s philosophy of science that claims that truly scientific theories can be refuted by empirical evidence but can never be verified by empirical evidence. In effect then, according to Lipsey circa 1963, his book is really about NEGATIVE economic science!

This apparent inconsistency is abruptly corrected in his second edition where he says he has ‘abandoned the Popperian notion of refutation and [has] ... gone over to a statistical view of testing that accepts that neither refutation nor confirmation can ever be final, and that all we can hope to do is discover on the basis of finite amounts of imperfect knowledge what is the balance of probabilities between competing hypotheses’ [1966: xx]. While this may accord better with common notions of science, it is not clear that there is anything positive (or negative!) left in the LSE version of positivism.

In the sixth edition we are told that only positive statements are testable. Normative statements are not testable because they depend on value-judgements. Moreover, ‘statements that could conceivably be refuted by evidence if they are wrong are a subclass of positive statements’ [1983: 6]. So, practitioners of positive economics ‘are concerned with developing propositions that fall into the positive, testable class’ [p. 7]. But looking closer, on page 5 it is asserted that a statement is called ‘testable’ if it can ‘be proved wrong by empirical evidence’ and then turning to page 13 we are told it is ‘impossible to refute any theory conclusively’! Unless Lipsey meant something different from what appears on page 5, it would seem that the class of positive economic statements is empty and thus positive economics is impossible. If there is any doubt about whether the advocates of LSE positivism are profoundly confused about methodology, the 1988 Canadian edition of Lipsey’s book provides the proof: we are boldly told, ‘There is no absolute certainty in any knowledge’ [Lipsey, Purvis and Steiner 1988: 24]. I ask, how can one claim to know with absolute certainty that one cannot know with absolute certainty?

Their bold statement is self-contradictory and yet it appears to be the foundation of modern economic positivism. As is well known, anything can be proven with a foundation containing contradictions (e.g., 2 equals 1, black is white, etc) and whenever it is possible to prove contradictory things the proofs are meaningless. Thus, we would have to conclude that nothing can be accomplished with the modern positivist’s methodology if that methodology is the one described in the various versions of Lipsey’s famous book. I think Lipsey should not have simply dropped Popper in order to avoid some ‘problems that seem intractable to a believer in single-observation refutations’ [1966: xx]. While his move will please those philosophers of science who are all too eager to dismiss Popper’s challenges to logical positivism, I think that Lipsey should have tried to critically examined those ‘intractable’ problems.

POSITIVE SCIENCE OR POSITIVE ENGINEERING?

Even though the philosophy of economic positivism has not been well thought out by its main proponents, it still captures all the satisfying notions that most mainstream economists seem to desire. On the one hand, it appears to support the commonly accepted view of explanatory science. On the other hand, it appears to support the appropriate cautions for a socially acceptable practice of social engineering. Specifically, both perspectives are served by the common view that positivism represents the avoidance of value-judgements.

Explanatory science

Those economists today (including those from MIT or LSE) who see themselves as scientists offering explanations of economic phenomena will be pleased to find that adherence to positivism only requires assurances that the assumptions of one’s model are falsifiable. Falsifiability of one’s assumptions merely assures that the conclusions and explanations provided by the model will not be what economists call tautologies. To be careful here, it should be recognized that what economists mean by the term ‘tautology’ is not always what philosophers or logicians mean by that term. Economists think that if it is impossible to conceive of how a given statement could be false, then that statement is a tautology. This includes both what philosophers call tautologies (statements that are true by virtue of the logical form alone) and quasi-tautological statements that are true by definition or depend on definition-like statements such as value-judgements. It is the latter form of statements which is usually what economists mean by the term ‘tautology’.

But why are economists so concerned with avoiding tautologies? The only methodological problem solved by avoiding tautologies is the one facing economists who wish to claim that their empirical tests of their models or theories represent positive contributions on the basis that their empirical evidence verifies or confirms their models [see Boland 1977, 1989]. The problem is that there are some statements which are of the form that economists call tautologies, yet that can also appear to be confirmed. The most obvious example is the ‘quantity theory of money’. That ‘theory’ is represented by the equation $MV = PT$. On close examination it turns out that the two sides of this equation are merely what you get by reversing the order of summation between i and j for the double summation $SSp_{ij}q_{ij}$ [see Agassi 1971]. Confirming a statement which cannot conceivably be false cannot really contribute anything positive to economic science.

Social engineering

Those economists today who see themselves as providers of policy advice will be pleased to learn that adherence to positivism will assure them that their recommendations will not be easily dismissed. Policy makers seldom are concerned with whether the consulting economists are dealing with tautological models or whether any theory is falsifiable. What is important is the assurance that the advice given is not just a reflection of the biases of the consulting economists.

So, what methodological problem is solved by expecting policy advisors to be practitioners of economic positivism? Given all of the equivocation incorporated in the presentations of modern economic positivism (e.g., Lipsey’s textbook), there is no reason for a policy-maker to expect that the economist’s advice will be firmly supported by empirical evidence. It all comes down to the economic researcher making judgements about whether the available evidence should be sufficient reason to support or reject a given theory that was used to form the advice given. In most cases, it is the personal demeanor of the researcher that gives his or her research credibility. Note well, by stressing the importance of the personal demeanor of the researcher it is evident that positive economic engineering is merely a version of Bacon’s inductivism.

If economists who provide policy advice could get by with wearing white lab coats, I am sure they would parade before television cameras so attired. But again, the demeanor of the practicing economic positivist is more understood by what it is not. Nobody will believe an economist who claims to know the truth and refuses even to look at data. Nobody will believe an economist who is interested only in publicly promoting his or her personal value-judgements. Nobody will believe the research done by someone who behaves like Goethe’s young Werther. In other words, true-believers, zealots and romantics need not apply for the job of economic advisor. And it seems firmly believed that adherence to economic positivism precludes such objectionable demeanor.

POSITIVE EVIDENCE ABOUT POSITIVE ECONOMICS

Having discussed the nature of the economic positivism explicitly discussed in positivist textbooks, the next consideration ought to be about how positivism is actually practiced in positive economic analysis. The salient feature of all examples of ‘positive’ economic analyses is their conformity to just one format. Specifically, after the introductory section of a typical positive economics article there is a section titled ‘The Model’ or some variation of this. This is followed by a section titled ‘Empirical Results’ or something similar, and a final section summarizing the

‘Conclusions’. The question that should be considered is: why do virtually all positivist papers conform to this one format? Is the dominance of this uniformity the *only* success of modern economic positivism?

A superficially true explanation for why a specific format is universally used is that it is a matter of rhetoric [see McCloskey 1989]. A trivial explanation of the widespread use of a specific format would be that all journal editors require that format, but surely they are only responding to what they think the market demands. The concern here is not just why any particular individual might decide to organize a research paper according to the accepted format. Instead, the concern is why this particular format is so widely demanded.

I do not see any reason why the same principles of understanding embodied in the current practice of economic positivism – namely, model-building – would not also be applicable for the economic methodologist attempting to explain the empirical uniformity evident in the widespread practice of model-building itself. So, in order to explain or describe the practice of economic positivism, let me attempt to build a ‘model’ of the format of a typical article in the literature of positive neoclassical economics. Judging by what is often identified as a ‘model’ in positive economics, virtually every formal statement is considered a model. Nevertheless, there are some basic requirements.

In order to build my model of positive or empirical analysis, as with any model, the assumptions need to be explicitly stated. Let me begin by stating the obvious assumptions which form the visible core of the research programme of neoclassical economics. My first and most fundamental assumption is that every neoclassical model must have behavioural assumptions regarding maximization and market equilibrium. Furthermore, the results of the model must depend crucially on these assumptions.

The remaining assumptions are less fundamental to neoclassical economics but are required to provide the rhetoric of modern economic positivism. To provide the main needed ingredient of modern economic positivism, my second assumption is that every empirical model must yield at least one equation which can be ‘tested’ by statistically estimating its parametric coefficients.

My third assumption (which is required for the implementation of the second assumption) is that every empirical paper must presume specific criteria of ‘truthlikeness’ – so-called statistical testing conventions. For example, one must consider such statistical parameters as means and standard deviations, R^2s, t-statistics, etc. That is, every equation is a statement which is either true or false. However, when applying an equation to empirical data we supposedly know that the fit will not usually be perfect even if the statement (i.e., the equation) is true. So the question is: in what circumstances will the fitted equation be considered ‘true’? The use of the testing conventions implies that the investigator is not attempting to determine the absolute truth of his or her model. Rather, the objective is to establish its acceptability or unacceptability according to standard testing conventions of one’s chosen form of economic positivism.

My last assumption is that in order to be published, every empirical paper must have contributed something to the advancement of ‘scientific’ knowledge. That is, it must establish some new ‘facts’ – namely, ones which were previously unknown – by providing either new data or new analysis of old data.

In order to test this model of the methodology of neoclassical positive economics, the available data must be considered. First I must decide on where to look for mainstream ‘positive economics’. Obviously, one should expect to find it in the pages of the leading economics journals. So, to test this model, I should be able to open any leading mainstream journal such as the *American Economic Review* or the *Economic Journal* and examine the contents of a few issues. To be relevant, the examination of the data should be restricted to those articles intended to be positive analysis. That is, avoid those articles considered to be avant-garde theories or concerned with the more technical (mathematical) aspects of ‘economic theory’. Of course, one should also ignore topics such as ‘history of thought’ or ‘methodology’ if they can be found.

Actually, I performed this test for the *American Economic Review* for the year 1980 [see

Boland 1982, Chapter 7]. My examination of the articles selected as stated seemed to me to indicate that all of them conformed to the format specified by this model of positive neoclassical analysis. The only empirical question implied by this positive model is whether there are any exceptions to what I have claimed will be found in the mainstream journals. As expected I was able to report that there were none in the data considered. My model of positive analysis did fit the available data.

EXPLAINING THE USE OF THE STANDARD ARTICLE FORMAT

Despite the ease of confirming such a positive model of economic positivism, there is apparently no discussion of *why* papers *should* be written according to the observed format – apart from the recent discussion limited to the *rhetoric* of economic positivism [see McCloskey 1989]. Of course, there is no need to discuss the standard format if everyone agrees that it presents no problem and it is doing its required job. My general theory is that the reason why the format is not discussed is that its purpose is simply taken for granted. Taking things for granted is a major source of methodological problems and inconsistencies in economics, although the problems are not always appreciated. This is the case with the widespread use of one common format for neoclassical empirical research papers. Perhaps there is no discussion because the job performed is merely one of an elementary filter, one which presumes that only papers that can be expressed in the standard format could ever make a positive contribution to positive economics. This presumption is also not discussed anywhere. So, just what is the purpose of the standard format?

While there need not be anything inherent in positivism which would connect its practice with development of neoclassical economics, the two are closely related. The purpose of the standard format for those articles which purport to provide positive neoclassical economic analysis is exactly the purpose for promoting positivism in the first place. The purpose is the facilitation of a long-run inductive verification of knowledge even though the format is promoted by people who would see themselves practicing a more modest view of knowledge and method, a view which supposedly denies induction. At the root of this view is the conviction of Manifest Truth. More specifically, it is the conviction of neoclassical economists that neoclassical economics represents a true theory of society – that the real world is manifestly what neoclassical economists claim it is – and thus any model based *only* on facts generated in the real world will in the long run lead one to see the Manifest Truth which in this case is believed to the veracity of neoclassical economics. Basing models only on facts generated in the real world is, of course, the claimed purpose of positivism.

To understand the relationship between the standard format and the research program to verify neoclassical theory, we need to consider the following questions. What constitutes a successful positive analysis? What would be a failure? And, in order to determine what constitutes a success, it would seem that we ought to consider a more fundamental question: what is the objective of neoclassical model-building?

If the usual published positive neoclassical articles are actually considered contributions to ‘scientific knowledge’, then it can only be the case that the hidden objective of such positive economics is the one of *Chicago positivism*, namely, a long-term verification of neoclassical economics. Specifically, each paper which offers a confirmation of the applicability of neoclassical economics to ‘real-world’ problems must be viewed as one more positive contribution towards an ultimate inductive proof of the truth of neoclassical theory. My reason for concluding this is merely that logically all that can be accomplished by the typical application of neoclassical theory to ‘real-world’ phenomena is a proof that it is *possible* to fit at least one neoclassical model to the available data. Critics can always say that a model’s fit may be successful in the reported case but it does not prove that it will be successful in every case [see further Boland 1989, Chapters 7 and 8]. I would argue that the agenda of positive neoclassical research programs presumes that if we can continue to contribute more confirming examples of the applicability of neoclassical economics, then eventually we will prove that it is the only true theory of the economy.

POSITIVE SUCCESS OR POSITIVE FAILURE?

Clearly an examination of the format of a typical positivist economic analysis reveals that, *as a form of rhetoric*, economic positivism has been very successful. But has it been successful at fulfilling the broader promises of positivism? This is a particularly important question for those of us who reject the possibility of an inductive proof for any theory such as neoclassical economics.

While many of the proponents of the market system of prices in general and of privatization in particular are also proponents of positive economic analysis to support their views, it is seldom recognized that advocacy of either view is inconsistent with a non-romantic practice of positivism. It is not difficult to imagine the positive economist's response to a simple observation that, while the positivist economists base their analysis of economic phenomena on the presumed existence of a perfectly functioning market system of prices, the world outside our windows is not such a perfectly functioning system.

For example, if the world were governed by a market system of prices without governmental interference or private collusion, then eventually society's resources will be optimally allocated according to the desires of all individual consumers. And, we are told the world outside our window is in a state of equilibrium and specifically, all prices are equilibrium prices. For this reason, any subsequent introduction of governments into the model will usually be seen to result in sub-optimal allocations of resources. Thus it is argued that privatization and the reliance on prices (as the only information appropriate for social coordination) is to be recommended.

To be fair, it should be recognized that the advocacy of privatization is a relatively recent phenomenon and not all advocates consider themselves to be positivists. Moreover, not all positivists advocate privatization despite what may seem to be the case today. During the 1950s and 60s, most of the positivists were engaged in the advocacy of government interference in everyday economic affairs on the basis of what they called Keynesian economics. To these positivists it was enough to look outside our windows and see that the world is characterized by cyclical high unemployment and various levels of instability. Much of the academic effort in that period resulted in the development of the econometric approach to economic positivism which was intended to assist governments in the process of managing and 'fine tuning' the economy.

It would seem that truly positive economists would shun such advocacy and simply and dispassionately explain the world the way it is. Namely, they should explain how phenomena are generated in a world where governments and collusion are commonplace. The obvious fact that many proponents of economic positivism are almost always engaged in the advocacy of simplistic engineering views such as either global privatization or governmental macroeconomic management should lead one to recognize that too often today economic positivism is mostly, and perhaps only, rhetoric.

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