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Positive Economics as Optimistic Conventionalism

The venerable admonition not to quarrel over tastes is commonly interpreted as advice to terminate a dispute when it has been resolved into a difference of taste, presumably because there is no further room for rational persuasion. Tastes are the unchallenged axioms of a man’s behavior....

... On the traditional view, an explanation of economic phenomena that reaches a difference in tastes between people or times is the terminus of the argument.... On our preferred interpretation, one never reaches this impasse: the economist continues to search for differences in prices or incomes to explain any difference or changes in behavior.

George Stigler and Gary Becker [1977, p. 76]

Attacking any theory is easy enough, since none is perfect. But the wide class of empirical observation that *is* explained by economic theory should caution one against sweeping that theory aside and setting up new *ad hoc* theories to explain *only* or *primarily* those events the standard theory will not explain. What is wanted is a generalization of economic theory to obtain an expanded scope of validity without eliminating any (or ‘too much’) of the class of events for which it already is valid....

Armen Alchian [1965]

To abandon neoclassical theory is to abandon economics as a science....

Douglass C. North [1978]

Our discussions in the previous six chapters have centered on the hidden agenda of neoclassical economic theory; our evidence was the nature of avant-garde neoclassical research programs. It is fair to question whether those considerations shed any light on the mainstream of neoclassical economics. By referring to them as 'avant-garde' we clearly indicate that they are not viewed as mainstream research programs. In this and the next two chapters we will examine the hidden agenda of mainstream neoclassical economics and show that in many ways the hidden agenda items are the same as those of the avant-garde programs but that the research programs of the mainstream are much more primitive.

The mainstream can be divided into two separate currents. One moves under the overt banner of 'positive economics', although not too many years ago it was merely called 'applied economics'; the other under the pretentious title of 'economic theory', although it is merely what was called 'mathematical economics' twenty or thirty years ago. Their differences are essentially analogous to the differences between optimistic (or 'naive') Conventionalism and pessimistic (or 'sophisticated') Conventionalism [Agassi, 1966a]. Optimism in matters of neoclassical economics tends in some circles to lead to anti-intellectualism. Pessimism too often leads to silliness. But we are getting ahead of ourselves. Let us begin this chapter with an examination of the available evidence.

Positive Evidence about Positive Economics

The salient feature of all the applied or '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 we should consider is why do virtually all applied papers conform to this one format? As we shall explain, the reason is that this format satisfies the dictates of Conventionalism.

A 'model' of neoclassical empirical analysis

A trivial explanation for why a specific format is universally used is that all journal editors require that format, but they are only responding to what they think the market demands. Our concern here is not just why any particular individual might decide to organize a research paper according to the accepted format. We wish to examine why this particular format is so widely demanded.

One way to understand a methodological format is to emulate it – so let us attempt to build a 'model' of the format of a typical article in the

literature of positive 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 our model of neoclassical empirical analysis, as with any model, the assumptions need to be explicitly stated. Let us begin by stating the obvious assumptions which form the 'visible agenda' of neoclassical economics. Our first assumption is that every neoclassical model must have behavioral assumptions regarding maximization or market equilibrium. Furthermore, the results of the model must depend crucially on these assumptions. Our second assumption is that every empirical model must yield at least one equation which can be 'tested' by statistically estimating its parametric coefficients.

Beyond these two explicit requirements almost anything goes when it comes to building the model. But there are two more requirements that are part of the first item on the hidden agenda of neoclassical research programs. Our third assumption is that every empirical paper must presume specific criteria of 'truthlikeness' – so-called 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 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.

Our 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' – that is, ones which were previously unknown – by providing either new data or new analysis of old data.

An 'empirical analysis' of neoclassical literature

In order to test our model of the methodology of neoclassical positive economics, we must consider the available data. First we must decide on where to look for mainstream 'positive economics'. Obviously, we should expect to find it in the pages of the leading economics journals.

So, let us sample one arbitrary year, say 1980, and examine the contents of a few issues of such journals for that year. Further, let us restrict our examination of the data to those articles intended to be positive analysis. That is, we are not interested in those articles considered to be avant-garde theories or concerned with the more technical (mathematical) aspects of 'economic theory'. We should also ignore topics such as 'history of thought' or 'methodology'. Let us examine the topics that remain:

- The Market for New Ph.D.s
- Family Size and the Distribution of Income
- Wages, Earnings and Hours of First, Second, and Third Generation American Males
- Foreign Trade and Domestic Competition
- Taxing Tar and Nicotine
- Murder Behavior and Criminal Justice System
- Optimal Order of Submitting Manuscripts
- Effect of Minimum Wage in Presence of Fringe Benefits
- Economics of Short-term Leasing
- Federal Taxes and Homeownership
- Decline in Male Labor Force Participation
- Open Market Operations
- Effects of State Maximum Hours Laws
- Job Queues and Layoffs
- Relative Capital Formation in the US
- Potential Gains from Economic Integration in Ghana
- Effects of the EEC's Variable Import Levies
- Unemployment, the Allocation of Labor, and Optimal Government Intervention

Our examination of the articles on these topics seems to indicate that all of them conform to the format specified by our model. The only empirical question implied by our positive model is whether there are any exceptions to what we have claimed will be found in the mainstream journals. We can report that there are none in the data considered. Our model of positive analysis does fit the data.

Some questions raised by this positive analysis

Now we do not wish to push this mockery of neoclassical positive analysis any further, as it is not clear what positive contribution it would make. Nevertheless, it does emphasize the point raised that there is an amazing empirical uniformity among positive neoclassical articles. Empirical uniformities beg to be explained.

There is apparently no discussion of why papers should be written according to the observed format. 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. But what is the purpose of the standard format?

Our 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 format of neoclassical empirical research papers. We are going to argue here that the purpose of the standard format is the facilitation of an inductive verification of neoclassical theory even though the format itself serves a more modest Conventionalist view of knowledge and method, a view which supposedly denies induction.

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 empirical analysis? What would be a failure? What is presumed in the use of 'testing conventions'?

The Logic of Model-building in Positive Economics

Every applied model in neoclassical economics is a specific attempt to model the essential idea of neoclassical theory – independent individual maximization with dependent market equilibria. In a fundamental way each model is a test of neoclassical theory's relevance or applicability to the phenomena of the real world. At the very minimum, each model is an attempt to make neoclassical *theory* testable.

Since our idea of applied models is still not universally accepted, perhaps we should be more specific about our view of the nature and purpose of model-building. While some economists use the term 'model' to specify the idea of a formal model as conceived by mathematical logicians, our use of the term reflects the more common usage in positive economics [e.g., Lucas, 1980]. Although we have discussed the nature of models elsewhere [Boland 1968, 1969, 1975, 1977a, 1977b], it would be useful to review the essentials here.

The role of models in testing theories

One way to determine if a theory will work in a given practical situation would be to build a 'model' of our theory much in the spirit of design engineering. Design engineers might build a small model of a new airplane wing design to test its aerodynamics in a wind tunnel. In other words, engineers commit themselves to specific models. Of course, many different models may be constructed (all based on the same new wing idea) by varying certain proportions, ingredients, etc. Unfortunately, such opportunities for testing in this manner (i.e., with scaled-down models in wind tunnels) seldom arise in economics.

Schematically, in model-building we traditionally start with a set of autonomous conjectures as to basic behavioral relationships which must include an indication of the relevant variables and which of them are exogenous and which are not. To these we add specifying or simplifying assumptions, the nature of which depends on what is being simplified or specified (i.e., on the behavioral assumptions). One reason why we must add these extra assumptions is that no one would want to make the behavioral assumptions of our neoclassical theory of the consumer (or producer) as specific as would be required in order to make it (or predictions deduced from it) *directly* observable. Applied models add another set of assumptions designed to deal with the values of the parameters either directly specifying them or indirectly providing criteria to measure them. This gives us the following schemata for any model (in our engineering sense):

(1) *A set of behavioral assumptions* about people and/or institutions. This set might include, for example, the behavioral proposition $Q = f(P)$, where dQ/dP is negative. The *conjunction* of all the behavioral assumptions is what traditionally constitutes a 'theory'.

(2) *A set of simplifying assumptions* about the relationships contained in the above set. For example, the demand function stated in the theory might be specified as a linear function, $Q = a + bP$, where 'a' is positive and 'b' is negative.

(3) *A set of assumed parametric specifications* about the values of those parameters created in the second set above. For example, the parameter 'b' above might be assumed to have the value $b = -4.2$; or the specification that the above model fit the available data according to certain statistical criteria.

Observing that any empirical model is a conjunction of these three sets of assumptions leads us to consider some problems concerning what constitutes a success or failure. Whenever it is shown that one of the predictions is false, then, by *modus tollens* (see below, p. 166), we can conclude that at least one of the assumptions (the constituent parts) must be false. Note, however, there is a certain ambiguity about which type of assumption is responsible for the false prediction. If any one of the assumptions is false, then some of the predictions are going to be false. But since any of them could be the false assumption, just noting that one of the predictions is false does not necessarily tell us anything about which assumption has 'caused' the false prediction [see further, Boland, 1981b]. We will call this the problem of the ambiguity of logical refutations. We shall see that this is particularly a problem for model-builders who are using models to refute neoclassical theory.

The logical problem of testing theories using models

To expect to refute a theory by showing that it is false by means of empirical testing means that we must expect to show that all possible models of the theory are false! In other words, to get at the basic behavioral assumptions themselves we must consider all possible ways of specifying them (however simple or complex). But there will always be an infinite number of ways. Assuming that there are no logical errors, if *every* one of them, when conjoined with the behavioral assumptions, can be shown to lead to *at least one* false prediction, then we *know* that at least one behavioral assumption is necessarily false. And if that were not the case, that is, if all the assumptions are (non-tautologically) true, then it is *possible* to specify the behavioral assumptions such that no false predictions could or would ever happen. Obviously, the requirement that we must show that *all* possible models are false is impossible for the same reason that it is impossible to verify a strictly universal statement [Popper, 1934/59]. We must therefore conclude that on this basis the empirical falsificaton of neoclassical theory using *models of the theory* is impossible. We will return to this question below.

Now what about building *specific* models of a theory intending to show that the theory is true? Well, this is the old logical problem which logic textbooks call 'the fallacy of affirming the consequent' [Boland, 1979a, p. 505]. In effect, every model of a theory is a special case and a confirmation of one model is good only for one given set of phenomena. Even though one may confirm a neoclassical model's application to one market during one period of time, one still has not proven that the same model can be applied to any other market or any other period of time. To say that a behavioral theory is true is to say that it applies to every situation to which it purports to be relevant. That is, *if* a theory is true, then it is *possible* to build at least one model that will fit the data in *any* given situation? If the theory is not a tautology (i.e., an argument which for logical reasons cannot be false), then to prove it true we would have to provide a potentially infinite series of models. That is, no finite set of confirmed models will do, since there will always be the logical possibility of a situation which cannot be modeled or fitted. It is easy to see that this is merely the Problem of Induction restated at a slightly different level of discussion.

The point of formalizing our view of models is to show that building models of a theory in effect insulates the theory from empirical testing if our purpose in testing is either refutation or verification. We can also conclude that neoclassical economists who are not prone to making logical errors, but are nevertheless building models to apply or to test neoclassical economics, must have some other objective in mind – otherwise there would be more concern for these logical problems.

The Problem with Stochastic Models

Some may argue that the logical problems discussed here are irrelevant for the neoclassical economist who is wedded to Conventionalism, since these problems concern only those cases in which someone is attempting to provide a proof of the absolute truth or falsity of any given theory. Instead, we should be concerned only with the problems of building models which fit the data with acceptable degrees of approximation [Simon, 1979]. But in response we might argue, if models are never refutations or verifications, what constitutes a successful model? When would a model-builder ever be forced to admit failure?

Virtually every applied neoclassical model today is a stochastic model. The reason for this is simple. Stochastic models are the primary means of accommodating the dictates of Conventionalism and at the same time solving the Problem of Conventions externally by appealing to universally accepted statistical testing conventions. One does not have to build stochastic models to satisfy Conventionalism, but it certainly helps.

The problem with the concept 'stochastic', or more generally, with the doctrine of 'stochasticism' – the view that realistic models must be stochastic models [Boland, 1977c] – is that it takes too much for granted. Some economists are fond of claiming that the world is 'a stochastic environment' [e.g., Smith, 1969]; thus technically no model is ever refuted or verified, and hence there could not be any chance of our construing one as a refutation or a verification of a theory? This concept of the world can be very misleading and thus requires a critical examination.

Our purpose here is to show that stochasticism involves model-building, as it requires an explicit assumption which is possibly false, and thus stochasticism should not be taken for granted, and, further, to argue that the retreat to stochasticism does not succeed in avoiding the logical problems of using models to test neoclassical economics.

The nature of stochasticism

The word 'stochastic' is based on the idea of a target and in particular on the pattern of hits around a target. The greater the distance a given unit of target area is from the center of the target, the less frequent or dense will be the hits on that area. It can also be said that there are two 'worlds': The 'real world' of observation and the 'ideal world' of the theory or mathematical model. Thus, we might look at a model as a shot at the 'real world' target. When we say the theory (or model) is 'true' we mean that there is an *exact* correspondence between the real and the ideal worlds. There are many reasons why we might miss the target, but they fall into two rough categories: (1) ours was a 'bad' shot, i.e., our model was false or logically invalid, or (2) the target moved unexpectedly, i.e., there are random, *unexplained* variations in the objects we are attempting to explain or use in our explanation. Some may thus say that a stochastic model is one which allows for movements of the target. However, it could also be said that stochastic models follow from a methodological decision *not* to attempt to explain anything *completely*.

Many will argue that even with true theories the *correspondence* between these two worlds will not be exact for many obvious reasons (e.g., errors of measurement, irrational mistakes, etc.). For these reasons neoclassical models are usually stochastic models which explicitly accommodate the stochastic nature *of the correspondence*. For example, we can assume that the measurement errors, etc., leave the observations in a normal, random distribution about the values of the ideal world. This means that it is the correspondence which is the stochastic element of the model. Note, however, we are saying that it is the model (or theory) which is stochastic rather than the world or the 'environment'. Any test of a stochastic model is a test as much of the assumed correspondence as of the theory itself.

One can see the world as being necessarily stochastic *only* if one assumes beyond question that it is one's model (the shot at the real world target) which is true (and fixed) and that the variability of the correspondence is due entirely to the movements of the target (the real world). Thus stochasticism can be seen to put the truth of our theories beyond question. There is a serious element of potential intellectual dishonesty in asserting that the environment is stochastic. We *assume* that the assumptions of our theory or model are true because we cannot prove them true. Thus there is no reason for any assumption to be beyond question, as stochasticism seems to presume.

The logical problems of stochastic models

If it is granted that it is the models or theories which are stochastic and not necessarily the real world, then stochastic models are still subject to the logical problems discussed above (see p. 161). Does this mean that we must give up any hope of testing neoclassical theories? We would argue that it does not; it just makes things a bit more complicated. The logical problems involved in any test of neoclassical economics are not insurmountable if it is recognized that it is the model rather than the environment which is stochastic. That is, we can overcome the logical problems outlined above if we explicitly recognize the specific assumptions which make the model stochastic.

Unfortunately, when we build stochastic models, the logical problems are not as apparent. So let us review the discussion with respect to nonstochastic models. We cannot refute a theory by first building a model of that theory and then refuting the model because of the problem of the ambiguity of logical refutation. We cannot logically identify the source of the refutation – is it the behavioral assumptions of the theory or is it only the 'simplifying' assumptions that we have added? This problem is solely the result of our having to add *extra* assumptions in order to build the model. Although stochasticism requires additional assumptions and thus suffers from this problem, it also adds an entirely different logical problem which is not widely recognized.

For now let us forget the problem caused by adding extra assumptions. Let us restrict our concerns to testing a model, not bothering about whether one can logically infer anything about the underlying theory. The logic of refutation is based on the three propositions. We refute a logically valid model (1) by arguing *modus ponens*: whenever all the assumptions are *true* then every prediction which logically follows from their conjunction *must be true*; or equivalently (2) by arguing *modus tollens*: whenever any prediction turns out to be *false* then we know that the conjunction of all of the assumptions cannot be *true*. If we actually observe a false prediction, does that guarantee that at least one of the assumptions is false? We are able to argue in favor of such a guarantee only because we accept (3) the logical condition of *the excluded middle*: 'A statement which is not *true* must be *false*.'

This is not a trivial word game about 'true' and 'false'. For example, if we adopt the stochastic-Conventionalist view that identifies absolute truth with a probability of 1.00 and absolute falsity with 0.00, then to say some given statement is not absolutely true does *not* imply that it is absolutely false. A stochastic statement with a probability of 0.60 is not absolutely true, nor is it absolutely false! This same ambiguity occurs when positive economists substitute 'confirmed' for the term 'true', and 'disconfirmed' for the term 'false' in the above logical propositions. Generally, 'not confirmed' does not mean 'disconfirmed'. In other words, when 'confirmed' and 'disconfirmed' are used in place of 'true' and 'false', proposition (3) is discarded. But when *the excluded middle* is discarded we sacrifice the logical force of any test. That is, we cannot construct an 'approximate *modus ponens*' such as (1') 'Whenever all assumptions are "confirmed" then every prediction which logically follows from their conjunction will be "confirmed"' because it does not imply (2') 'Whenever there is a "disconfirmed" prediction then all of the assumptions cannot be "confirmed"'. It is quite possible for all of the assumptions to be confirmed and, with the same data, for one or more of the predictions to be disconfirmed too.

This is probably not the place to argue this, so we will leave the analytical proof or disproof up to the reader. But in simple terms, what we are saying is that the conjunction of several assumptions, each with a probability of 0.60, does *not* imply that all predictions will have a probability of 0.60. One example should be sufficient. Consider the following four statements.

(a) Urn A has 100 red balls and no green balls.
(b) Urn B has 100 green balls and no red balls.
(c) I have withdrawn one ball from A or B.
(d) The ball is red.

Together these statements, if absolutely true, imply that the following statement is absolutely true:

(e) I have drawn a ball from urn A.

Now, if statements (a) through (d) are true 60 per cent of the time (that is, they have a probability of 0.60 of being true), then what is the probability of statement (e) being true? Surely its probability need not be 0.60, since it compounds the probabilities of the other statements and it must be false whenever (c) is false regardless of the probabilities of the other statements. In other words, given a logically valid argument which works for absolute truth, the same argument need not work for any given degree of approximate truth. If our argument here is correct, it has serious implications for the generally accepted view of the methods of testing stochastic models.

Testing with stochastic models

We have argued above that stochastic models are models which contain assumptions that detail the stochastic correspondence between the exact model and the observable real world. For example, a stochastic model might contain an assumption that observational errors will be normally distributed about the mean corresponding to zero error. But for the purposes of logical inferences we must specify in what circumstances such an assumption would be considered 'false' and in what circumstances it would be considered 'true'. Usually this assumption will be some sort of parametric limit applied to the observed distribution of the actual errors. There will be a range of possible statistically estimated means and standard deviations. The criteria are designed either to avoid Type I errors (rejecting the model as false when it is actually true) or Type II errors (the reverse acceptance) but not both. Remember that unless we are discussing absolute truth we need two different criteria because we can no longer rely on the proposition of the 'excluded middle'.

That statistical testing must choose between avoiding one or the other type of decision error is the key to the problem we wish to discuss now. If we build a model to test a theory by adding statistical decision criteria to the model (to specify when it applies to the available data) and then we deduce a test prediction (e.g., an equation to be estimated by linear regression), the results must be assessed by the same criteria. If the criteria specified minimum conditions for the assumptions of the models to be accepted as 'true' for the purposes of the logical deduction of the prediction, then it is logically consistent for us to apply the same criteria to assess the 'truth' of the prediction. For example, as above we could say if we accept the assumptions as 'true' when the fitted equation has a probability of at least 0.95, then we can accept the predictions as 'true' when they have a probability of at least 0.95. We still have not avoided the problems discussed above (pp. 165-8), but at least we can be logically consistent in our decision process. However, remember that this consistency is only for the purposes of deducing the confirming predictions. If all of the predictions pass the test, we can say without inconsistency that the theory is so far confirmed.

What can we say if a prediction fails according to the decision criteria? When we said that we would accept a statement (an assumption or a prediction) which has a probability of 0.95, we did not say that failure to have at least a 0.95 probability implied that the statement was false or 'disconfirmed'. On the contrary, a criterion of acceptance of a statement's falsity might be a probability of less than 0.05. Should our prediction fail the confirmation criterion by having a probability of say 0.80, it would still be a long way from being logically considered false. There is then a fundamental asymmetry between the criteria of confirmation and disconfirmation.

Since most stochastic model-building in positive economics is concerned with deducing stochastic predictions, the usual choice made is to use 'confirmation' criteria rather than 'disconfirmation' criteria for the purposes of defining a valid deduction [Friedman, 1953]. Such models cannot automatically be useful when we wish to test the model except for the purpose of finding confirmations. In order to test a theory by building stochastic models we must do much more.

We are arguing not just that whenever both criteria are employed there is a very large range of undecidable cases (e.g., where the probabilities are between 0.05 and 0.95 along the lines we have just illustrated) but also that even if one criterion is used, the results are often contradictory, leading to the conclusion that most statistical testing done in the neoclassical literature is more inconclusive than the reporting might indicate. Before we can show this we must consider what it would take statistically to refute a theory using a stochastic model. Remember that with exact models we can refute a model by showing that one of its predictions is false (*modus tollens*). In effect, a false prediction is a counter-example; that is, it is a statement which would be denied by the truth of the exact model. This is a clue for our design of a logically adequate test of any theory. Let us illustrate this with the exact model concerning the selection of red or green balls from two urns. Whenever we can show that statement (e) is false and that the statement

(f) The ball was drawn from Urn B.

is true, at least one of the statements (a) to (d) must be false. In other words, (f) is a counter-example to the conjunction of (a) to (d). If we really wished to test the conjunction, then the statistical question would have to concern how to decide when the counter-example is *confirmed*.

We know of only one case in which this form of statistical testing has been applied [Bennett, 1981]. In that one pioneering case the results were dramatic. It was shown that if one were to take some of the well-known reports of tests of models of post-Keynesian theories and extend them by performing a similar test of models of corresponding counter-examples, the results would show that *both* the theories and their counter-examples were confirmed *using the same statistical test criteria*! What this demonstrates is that testing models using confirmation criteria (i.e., a statement is considered true if its probability is at least 0.95) can lead to contradictory results and that thus the usual published tests are often very misleading. But it should also be noted that Bennett's demonstration shows that it is possible to have decisive tests subject to the acceptence of specific stochastic decision criteria. For example, relative to given confirmation criteria, a refutation is successful only if the predictions fail the confirmation test *and* the counter-example passes the same test. Not many reported 'disconfirmations' satisfy these requirements.

Positive Success or Positive Failure?

This now brings us back to the question we keep asking: what constitutes a successful model in positive neoclassical economics? And, more generally, to decide what constitutes success we need to ask: what is the objective of neoclassical model-building?

Let us now consider the available facts before we answer these questions. First, there are all the logical problems we have been discussing. Second, all the standard statistical parametric criteria have been designed or used to identify confirming predictions, even though some investigators have attempted to use them to establish 'disconfirmations'. Since there has been very little recognition of the logical problems, we can only assume that the positive economic model-builders are not attempting to deal with them. So it is the secondary evidence of the prevailing confirmation criteria and the recognition of the necessity to choose between Type I and Type II error avoidance that we must take into consideration.

We now argue that if the usual published positive neoclassical articles such as those noted at the beginning of this chapter are actually considered contributions to 'scientific knowledge', then it can only be the case that the hidden objective of such positive economics is 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. Our reasons 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. We 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.

8

Analytical Theory as Defeatist Conventionalism

> In recent years, mathematical tools of a more basic character have been introduced into economics, which permit us to perceive with greater clarity and express in simpler terms the logical structure of important parts of economic theory....
>
> It may facilitate reference if we set out the basic assumptions of the model to be discussed in a number of postulates. This may be looked upon as a device for separating the reasoning within the model from the discussion of its relation to reality. The postulates set up a universe of logical discourse in which the only criterion of validity is that of implication by the postulates....Only the logical contents of the postulates matter....
>
> Tjalling Koopmans [1957]

> In all formal procedures involving statistical testing or estimation, there are explicitly stated but untested hypotheses.... In ... econometric studies ... the 'premises' [e.g., profit maximization, maximization of satisfaction] ... play that role. More in general, any statement resulting from such studies retains the form of an 'if...then...' statement....
>
> The 'if ... then ...' statements are similar to those in the formal sciences. They read like logical or mathematical reasoning in the case of economic theory, and like applications of statistical methods in the case of econometric estimations or testing. The heart of substantive economics is what can be learned about the validity of the 'ifs' themselves, including the 'premises' discussed above. 'Thens' contradicted by observation call, as time goes on, for modification of the list of 'ifs' used. Absence of the contradiction gradually conveys survivor status to the 'ifs' in question. So, I do think a certain record of noncontradiction gradually becomes one of tentative confirmation. But the process of confirmation is slow and diffuse.
>
> Tjalling Koopmans [1979, p. 11]

Propositions and Proofs

In this chapter we shall examine the nature of the other mainstream research program in neoclassical economics which also conforms to a specific format but one unlike that of 'positive economics'. Again we shall describe the nature of the format and the problems involved in its application and then explain the hidden agenda implied by its widespread use. But first we must see why anyone might think there is a need for an alternative research program in neoclassical economics.

The problem of 'positive economics'

Those neoclassical economists who are pessimistic about the possibility of ever constructing an inductive proof for neoclassical theory based on observed 'facts' have slowly developed a research program which on the surface appears to depart significantly from that employed in 'positive economics'. They might argue either that induction is impossible or that inductive proofs are never final, as 'all facts are theory-laden' [Hanson, 1965; Samuelson and Scott, 1975]. But if one doubts 'facts', what is left? Is economic theory an arbitrary game? If there are no final inductive proofs, does this mean that all theories are circular or infinite regressions? Is there no solid foundation for a scientific economics? Such questions are seldom asked any more simply because economic theorists avoid making broad claims for economic theories. It might be interesting to consider why such questions are avoided. We think their avoidance is likely for the same reasons as those identified in earlier chapters for similar omissions – such questions do not need to be asked, since the answers are considered obvious.

We shall argue here that the reason why these questions need not be asked is that economic 'theorists' have found what may be considered a superior alternative to solid empirical 'facts'. The problem with empirical 'facts' or, more properly, with *reports* of observations is that they can easily be questioned. That is, they cannot be claimed to be absolutely true. For many mathematical logicians [e.g., see Hughes, 1981] *that* is the problem with induction. To begin any successful inductive argument what is needed are unquestionably true statements. It turns out that the only unquestionably true statements are those that are logically true.

Let us consider what constitutes a logically true statement [see also Quine, 1965]. Generally, many logicians argue that a statement is true if it cannot be false (e.g., truth tables). Thus a statement is true only if it is logically true. Logically true statements are to be distinguished from empirical truths, which are contingent truths – that is, an empirical statement is true only if it *logically follows* from other *true* empirical statements. The 'if' can be left unsatisfied by a failure to 'follow logically' or by the use of false supporting statements.

Now the importance of all this is not to argue that empirical theory cannot be true or that theories are empty tautologies. Such is simply not the case. The point is that empirical theories are concerned with *contingent truths*, that is, statements whose claimed truth depends on the truth of other statements whose truth in turn may be unproven.

Our argument is that today the research program of neoclassical economic 'theory' is one of seeking logical truths instead of empirical 'facts' so as to push on with an *ersatz* inductive science. That is, everything must be directed to establishing logically true facts – just as everyone once thought science established empirically true facts. However, there is a limit to all this, since we do not wish to end up with only pure logical truths (i.e., tautologies).

The format of 'economic theory'

The paraphernalia of the pursuit of logical truths include the following 'buzz-words': 'proposition', 'theorem', 'lemma', 'proof', 'corollary', 'hypothesis', 'condition', and 'definition'. These words play a prominent role in the format of recent theory articles. Usually they are printed in capital letters to highlight the format.

The topics of typical theory articles cover a wide range but most are concerned with the theoretical problems we discussed in Chapters 3 and 4 above. The standard format seems to yield an article with several numbered propositions or theorems, each followed by a proof. The standard format follows quite closely the format of Koopmans' first essay [1957], which in turn merely copied the format of many mathematics textbooks of its day. Procedurally, the standard theory article begins by setting up a 'universe of logical discourse' or a 'model', as it is sometimes called. The rules of the game do not permit any new terms to be introduced after this step, as the object of the game is to show that some particular given theorem or situation of concern can be handled using only the stated model.

Unlike 'positive' analysis, which attempts to show that a particular theoretical proposition is logically supported by available data, the 'theory' article attempts to show that a particular theoretical proposition is logically supported by available mathematical theorems. Where 'positive' economics seeks objectivity in repeatable or observable data, 'theoretical' or, more properly, 'analytical' economics seeks objectivity in the autonomy of the discipline of mathematics. And this, we shall argue, is the problem with this neoclassical research program. While it may be easy to dispute empirical 'facts', surely it is not supposed to be easy to dispute the veracity of the mathematics profession. But we shall ask a more fundamental question: what is the cost of our reliance on these given mathematical theorems?

Analytical Model-building

Acceptable givens

In order to assess the methodology of economic 'theory' we need only begin with an examination of what are considered acceptable givens. That is, if one is going to prove some given proposition, one still needs some assumptions, some premises, which are beyond question. One is successful at proving one's chosen proposition when one shows that the proposition logically follows from the conjunction of one or more acceptable premises. Years ago, there was a small set of mathematical theorems which would be invoked in almost every book devoted to the mathematical structure of neoclassical economics. The most frequently used theorems had names such as Kakutani, Lyapunov, Brouwer, and Frobenius. For a while, until quite recently, this game had been transformed into one of referring to theorems named after economists, such as Arrow's possibility theorem, Sheppard's Lemma, Stolper-Samuelson theorem, etc. Today, it is somewhat curious that theorists refer to very few named theorems. So what is the set of acceptable givens now?

It would appear that one item on the portion of the hidden agenda devoted to the objectives of economic 'theory' is that we must appear to be self-reliant – that is, we must no longer appear to be dependent on the mathematics profession for our fundamental theorems. Nevertheless, the proofs do depend on established principles of algebra or set theory. But since students of algebra or set theory are required to duplicate the proofs of established principles, all major principles are in the 'public domain' by demonstration. Thus the current fashion in economic 'theory' methodology is to incorporate all givens in the 'universe of discourse' and provide a proof for anything else that is introduced. This means that apart from the terms introduced in the 'universe of discourse' the only things we are allowed to take for granted are the rules of logic, since everything else will be proven by the economic 'theorist' within the 'universe of discourse'.

One of the consequences of this admirable show of self-reliance is that many of the stated theorems and propositions for which proofs are published yield trivial results. Usually they are nothing but some familiar theorem from standard neoclassical theory. The contribution provided by the given article is a 'new' proof or an 'alternative' proof demonstrating that the theorem or proposition can be proven using only the specified 'universe of discourse'. Anything novel or informative will have to be

provided in the 'universe of discourse'. What we are saying here is simply that economic 'theory' today is nothing but exercises in puzzle-solving – along the lines described by Thomas Kuhn [1962/70].

Avoiding pure analytical results: tautologies

If the only givens allowed, beyond the definition of the terms to be included in the model, are the rules of logic, what constitutes successful model-building? As we noted above, unless a reference is made to some contingent proposition, the only outcome can be a tautology. This is because, for the purposes of logic, to prove a statement true means to prove that it is always true in the given circumstances (i.e., the given 'universe of discourse'). If no contingent propositions are introduced, then the only possible true statement is a tautology. (To reiterate, a tautology is any statement which is true by virtue of its logical form alone.) Since a tautology is true regardless of our 'interpretations' of its terms, then the 'interpretations' are irrelevant to the truth of the proven proposition. Critics of neoclassical 'theory' are free to argue in this case that there is nothing empirical or 'scientific' about such neoclassical model-building.

Unfortunately, the critics are often a bit confused about the nature of tautologies. They tend to think that any argument involving definitions and logic must be purely analytical, resulting only in tautologies. Although by their nature tautologies make the meaning of non-logical terms irrelevant, tautologies are not just a matter of definitions [see Boland, 1981b]. To illustrate let us take an example from elementary neoclassical theory. We might say that every demand curve is downward-sloping, and if it is not downward-sloping, it cannot be a genuine demand curve. Such a statement is in effect a tautology, since all possibilities are covered – by the implied definition of a 'genuine' demand curve. As the previous example shows, not all tautologies involve peculiar definitions (apart from the accepted definitions of fundamental logical terms such as 'and' or 'or'). But considering how complex a theory can be, it is quite easy inadvertently to construct a tautology by defining the terms in a manner which indirectly covers all cases and thereby leaves no conceivable counter-example.

We are not facing up to a fundamental question: why not seek tautologies, since they are always true statements? In other words, why are tautologies unacceptable as explanations? This is a delicate question and it is more difficult to discuss than might be expected. Consider, for example, a common explanation offered by neoclassical demand theory. When we offer any explanation, we put the truth of our assumptions at stake. In this case, when we explain someone's consumption choice as a consequence of the maximization of his or her utility, we put our assumption of utility maximization at stake. If it matters whether our explanations are true, it is because we want our theories to be true while at the same time allowing the possibility that our theories might be false. If they cannot be false (for purely logical reasons), not much will ever be at stake and thus nothing much can be gained.

All this may seem perverse, but it is really rather simple. An explanation is interesting because, while it is claimed to be true, it could be at the same time false (hence, it is not a tautology). If someone offers us an explanation which is true purely as a matter of logical form alone (i.e., all cases have been covered and thus all possible counter-examples are rendered inconceivable), we are not going to be very impressed, except perhaps with his or her cleverness. What makes the theory that all consumers are utility maximizers interesting is merely that someone might think there is a possibility for consumers being otherwise motivated.

We thus have to be careful to distinguish between the logical impossibility of counter-examples to our theory (due to the logical form of our theory) and the empirical impossiblity of the existence of counter-examples (because our theory happens to be true). This distinction is difficult to see when we use only elementary examples. So let us consider a different example, one which is a bit more complex.

Many years ago, economic theorists accepted as true what they called the Law of Demand. This allegedly true statement considers the question (identified above) of whether demand curves are always downward-sloping. Immediately, given the above considerations, we might suspect that such an allegedly true statement may only be a tautology, but let us suspend our judgement for a while.

Empirically it may be true that all demand curves are downward-sloping, but it may also be true that a good with an upward sloping demand curve is still a possibility. For instance, consider the allegation that a good with an upward sloping demand curve was observed many years ago by the statistician named Giffen. Such an observation is not logically ruled out by maximizing behavior [Samuelson, 1953]. The good demanded may have been an inferior good. For a good to have an upward-sloping demand curve the good must be an inferior good (a good for which the demand falls when income rises). Even inferior goods may still have downward-sloping demand curves as long as they are not too inferior (that is, their positive income effect does not overwhelm the negative substitution effect of increasing their price). However, if one restricts consumer theory to the question of the demand for non-inferior (i.e., 'normal') goods, then as a matter of logic it is possible to show that all such goods will have downward-sloping demand curves whenever the only reason for demanding them is to maximize utility.

In a 'universe of discourse' consisting only of non-inferior (i.e., 'normal') goods and utility-maximizing consumers, upwardly-sloping demand curves are logically impossible. In such a hypothetical world, Giffen's observations would not be empirically possible, since they are logically impossible. But this question of possibility depends on the special characteristics of our invented hypothetical world. There is no reason why the real world has to correspond to this restricted hypothetical world. In other conceivable worlds it is quite possible for there to be upward-sloping demand curves (i.e., Giffen goods).

The point of all this complexity and perversity is that a statement which some might consider to be a tautology may only be a statement for which the hypothetical world has been designed logically to rule out all counter-examples. In fact, in economics there are very few pure tautologies (statements which are true regardless of definitions). But there are many theories and models which invent hypothetical worlds that provide what we might call 'pseudo-tautologies'. What is important at this stage is the recognition that when we want to provide a true explanation or theory for something, we do not want our explanation or theory to be true merely because it is a tautology. A tautology is a true statement but its truth is, in a sense, too easy.

A Critique of 'Pure' Theory

The methodology of tautology avoidance

Although it is not widely recognized, it is interesting to note that Paul Samuelson's monumental Ph.D. thesis [1947/65] was, among other things, concerned specifically with methodology. Its subtitle was 'The Operational Significance of Economic Theory'. One of his stated purposes for writing the book was to derive 'operationally meaningful theorems' from economic theory. By 'operationally meaningful theorems' he meant hypotheses 'about empirical data which could conceivably be refuted, if only under ideal conditions' [p. 4]. As far as we are aware, Samuelson nowhere tells us why one would ever want to derive 'operationally meaningful theorems' or why anyone would ever think economics hypotheses should be falsifiable. But everyone knows why. If a statement or theory is falsifiable, it cannot be a tautology [cf. Boland, 1977a, 1977b].

To a certain extent requiring falsifiability is *ad hoc*, since falsifiability is not necessary for the avoidance of tautologies. All that is necessary for the avoidance of a tautology is that the statement in question be conceivably false. Some statements which are conceivably false are not falsifiable. For example, a 'strictly existential' statement such as 'There will be a revolution after 1984' can be false but we could never refute it.

Now the reason why Samuelson found it necessary to invoke the *ad hoc* requirement of falsifiability is that he wished to promote analytical models of neoclassical economics. Specifically, he 'wanted to find the common, core properties of diverse parts of economic theory' [1947/65, p. ix]. In short, he attempted to show that the foundations of economic analysis are nothing more than the analytics of maximization (or minimization). Not only did he show the logical equivalence of the theories of consumer behavior and of costs and production but he also demonstrated that they are equivalent to the theory of equilibrium stability. That is, they can all be reduced to the analytical properties of a maximizing system in which 'analytical properties' are merely provable theorems.

Samuelson's methodological contribution was to recognize that in order to avoid tautologies we must be concerned with the correspondence of the analytical model of an equilibrium to a dynamic process. That is, not only must our equilibrium explanation imply the existence of a potential balancing of demand and supply but we must also provide an explanation for *why* a market price or quantity converges to that balance point. He called this the *correspondence principle*. Unfortunately, it is too easy to transform his correspondence principle into another analytical issue and thus to defeat the effort to make economics refutable [see Boland, 1977b]. Specifically, this is the problem of explaining away disequilibrium which we discussed in Chapter 3.

Whenever someone attempts to satisfy the correspondence principle by adding a mathematically appropriate difference or differential equation for the rate of change of the price relative to the extent of disequilibrium (see pp. 138-9 above), the question concerning the testability of the original model of the nature of market clearing prices goes begging. That is to say, if one refuted the augmented model (which added a rate of change equation), one would not know whether the source of the failure was the added equation or the original model. This is merely the same problem of the ambiguity of logical refutations which we discussed concerning model building in positive economics in Chapter 7! This means that Samuelson's method for avoiding tautologies – requiring testability through a correspondence principle – can, in effect, make the original model untestable and thus is a self-defeating methodology.

Is falsifiability really necessary?

As our example above showed, if all we wish to accomplish is an avoidance of tautologies, then falsifiability is sufficient, but not necessary, since strictly existential statements can be false (hence not tautological), although they need not be falsifiable. An alternative way of

avoiding tautologies is to consider the terms of the 'universe of discourse' to be contingent statements about the nature of the real world. That is, instead of the analytical model being defined by such statements as 'Suppose there are *N* goods, *M* people, constant returns, a competitive equilibrium....', some of those statements could be considered empirical statements about the nature of the real world (along the lines we suggested in Chapter 6). If this is allowed, then there is no necessary problem about the possibility of the model being conceivably false. Can the problem of tautologies be so easlily solved?

The logical problem of analytical models

The question of the falsifiability or testability of economics is rather stale today. And, as we have just indicated, falsifiability is not really essential. Does this mean that analytical economics or 'pure' theory need not worry about the potential shortcomings of relying only on analytical proofs of (desirable) propositions? We hope to show that there is yet a more fundamental problem.

In order to discuss this new problem we will need to review some technical issues of formal logic. Our major concern will be the logical concept called the 'material conditional' – a concept which remains a skeleton in the closet of analytical philosophers who have fostered the format and methodology of 'pure' theory [cf. Hollis and Nell, 1975]. What we have to say here may not satisfy the tastes of fastidious analytical and linguistic philosophers but they will have to clean out their own closets.

Let us state our 'universe of discourse'. First, suppose that only statements can be true (or false). A theory is true or false only by virtue of its being a compound statement such as a conjunction of all its premises (or assumptions, as we would call them). Second, suppose that logical arguments (e.g., proofs) consist of one or more statements. An argument is sufficient only if it is logically valid – which only means that whenever all of its premises are true its conclusions (or predictions, to use our terms) are also true without exception. Third, suppose that there is no universal or general means of proving suffiency. We have only minimum conditions for sufficiency. And fourth, suppose that an argument in favor of the truth of any particular proposition or statement has two essential parts. One asserts the *validity* of the argument connecting the truth of the assumptions to the truth of the proposition in question, and the other asserts the *truth* of all of the assumptions which form the conjunction representing the argument.

Since 'pure' economic theory takes formal logic as a given for the purpose of providing proofs of propositions, the only question of concern here is what constitutes a minimally acceptable statement to be included in the logical argument. This is a question which Aristotle addressed. He stated what amounted to three minimum conditions: (1) the *axiom of identity* – in the process of forming or stating an argument the definition of any term which appears in more than one statement cannot vary; (2) the *axiom of the excluded middle* (which we have already discussed) – admissible statements can be true or false (i.e., 'maybe' is not allowed), and, more important, if a statement is not-false, the only other possible status it may have is that it is true; (3) the *axiom of non-contradiction* – no admissible statement can be both true and false simultaneously in the same argument.

Most existential or universal statements would be admissible. For example, 'All consumers are utility maximizers', 'There is one equilibrium price', etc. are unambiguous candidates because we know what it means for them to be true or false, although we may not know how to prove their truth status. Now we ask the key critical question. Are conditionals, that is, statements of the form 'if ... then ....', always admissible? We offer the following argument for why they may not always be admissible and thus why the basis of analytical economic theory is not as secure as we are led to believe.

Consider the standard form of a conditional or 'if ... then' statement: 'If *P* then *Q*', where *P* and *Q* represent admissible statements. (Note that we are discussing conditionals and not necessarily 'implications' [Quine, 1965].) Some logic textbooks would have us believe in the material conditional, namely, that such a statement is false only when *P* is true while at the same time *Q* is false. In all other cases, we are supposed to accept the 'if ... then' statement as true because of the excluded middle. Now, we ask, why must we accept the material conditional?

There are two alternative answers to this question. Some logicians might say that the given 'if ... then' statement is logically equivalent to the statement 'It is not true that "*P* is true" and "*Q* is false".' In these terms the 'if ... then' statement appears equivalent to a conjunction and is thus admissible. As a conjunction, it is false whenever one or more of its constituent parts is false. But this argument might lead to circularity if we question what is meant by 'logically equivalent'.

We prefer a different explanation. We argue that the only reason for accepting the material conditional is that analytical philosophers want *all* compound statements which are not self-contradictory to be admissible into logical arguments. Specifically, let us consider the given statement 'If *P*, then *Q*' and grant that whenever *P* is false the statement 'If *P*, then *Q*' is *not false*.

Now we argue that whenever *P* is false the statement 'If *P*, then *Q*' can also be considered *not true*. Thus we argue that in these circumstances the statement 'If *P*, then *Q*' does not always satisfy the

axiom of the excluded middle (since it is neither true nor false), hence it is not always admissible into a logically valid argument! The textbook argument accepts the material conditional, we conjecture, on the following basis. They claim that to say the statement 'If *P*, then *Q*' is not false means, on the basis of the excluded middle, that the statement is true. But we would claim that the invocation of the excluded middle presupposes that the statement is admissible – which is the moot point. That is, only if one presumes that the given statement is admissible can one infer that it satisfies the axiom of the excluded middle. If the question of its admissibility is still open, then we cannot infer that when it is not false it must be true.

If our argument here against the presumptions of the material conditional is accepted, then it deals a serious blow to the presumed universality of analytical proofs and propositions. It means that the 'if ... then' propositions that abound in analytical economics are actually much more limited in their logical force than is presumed. Specifically, the truth status of the compound statement 'If *P*, then *Q*' is decisive only in one of the four possible combinations of the states of *P* and *Q*. Whenever *P* is false we cannot determine what the truth status of 'If *P*, then *Q*' is. In particular, the statement is logically decisive only when it is false. Saying that the compound statement is not always logically decisive in no way questions the truth status of its parts.

Analytical Success or Analytical Failure?

We claim that either one or the other of the following propositions is true:

PROPOSITION 1: We are wrong about the problems of the universal applicability of 'if ... then' statements; thus analytical economics is a successful program to establish logical facts. Furthermore, the ultimate objective of this program is the 'generalization' of neoclassical economics – that is, an inductive proof of its universal truth.

PROPOSITION 2: We are correct and thus analytical economics cannot provide proofs of universal propositions. It can only provide analytical refutations of contingent propositions. A successful generalization of neoclassical economics is thus an impossibility for the same reason that inductive proofs of universal statements are an impossibility.

We will not try to prove either proposition, as that would be contrary to our stated argument. But analytically they cannot both be true. With regard to the first proposition, the second part follows from the conjunction of our previous argument that (dealing with) the Problem of Induction is a primary item on the neoclassical hidden agenda and our argument earlier in this chapter that analytical economics rejects 'positive economics' as an impossible means of establishing indisputable 'facts'. Instead only a valid logical argument could ever provide proof of a generalization, that is, could ever demonstrate the impossibility of counter-examples.

The basis of the second proposition was argued on pp. 184-8. Without the material conditional, analytical economics cannot establish any non-contingent or logical facts (i.e., proven propositions). Without universal propositions each proposition must be proven in each real-world case by proving that the givens are true. Without a logical proof any claimed generalization is always open to dispute since exceptions cannot be logically precluded.

9

Instrumentalism as a Rejection of Conventionalism

> The subject matter of economics is regarded by almost everyone as vitally important to himself and within the range of his own experience and competence; it is the source of continuous and extensive controversy and the occasion for frequent legislation. Self-proclaimed 'experts' speak with many voices and can hardly all be regarded as disinterested; in any event, on questions that matter so much, 'expert' opinion could hardly be accepted solely on faith even if the 'experts' were nearly unanimous and clearly disinterested....
>
> Milton Friedman [1953, pp. 3-4]

In the previous two chapters we have examined the revealed methodologies of the two leading currents in mainstream neoclassical economics. Both are Conventionalist research programs and thus are based on the presumed need to solve the Problem of Induction. 'Positive' economics is directly concerned, optimistically, with establishing empirical 'facts'. Although no claim is made for the absolute truth of the facts, it is presumed that they do make a positive contribution towards the ultimate verification of neoclassical theory. Analytical or 'theoretical' economics is more concerned with things that seem possible, the establishment of absolute logical facts. There is not much left for those who reject the concerns of Conventionalist methodology.

Popular Alternatives to Conventionalism

There is little new under the methodological sun. As we explained in Chapter 1, most methodological prescriptions can be traced to nineteenth-century reactions to Hume's recognition of the impossibility of providing a foolproof empirical basis for (scientific) knowledge. The most widely accepted prescription is the one suggested by John Neville Keynes: Thou shall not base positive economics on normative judgements. J. N. Keynes was attempting to devise methodological rules to implement an Inductivist philosophy of science in economics. His only problem was that he was a hundred years too late. Inductivism was on the way out as a result of Hume's arguments, and an alternative viewpoint was already being developed by Duhem, Poincare, Eddington and others with respect to the philosophy of physics. Their view is what we have been calling Conventionalism. At about the same time another alternative was being developed by Dewey, Mach and others. This latter alternative is sometimes called Pragmatism and at other times called Instrumentalism, even though these two views are not equivalent. Where Conventionalism and Pragmatism are direct competitors, Conventionalism and Instrumentalism are not. This may seem confusing but it is the reason why there is much confusion about the differences between Conventionalism and Friedman's methodology which is merely a straightforward version of Instrumentalism [Boland, 1979a]. They both reject Pragmatism. Futhermore, if one gives up interest in the Problem of Induction, none of the popular alternatives seems worthwhile.

Conventionalism and Pragmatism

Modern Pragmatism, like Conventionalism, has its roots in our inability to solve the classic Problem of Induction, the alleged problem of providing a factual proof for every scientific statement. The old 'scientific method' – namely, systematic proof by induction – is no longer considered effective. Very many philosophers (but not all) think that the Problem of Induction still needs to be solved or dealt with in some other way; they think an alternative *must* be found.

Conventionalism and Pragmatism are the most common alternative ways of dealing with the Problem of Induction. They are both concerned with proofs of the truth of our scientific (or other) knowledge. Pragmatism, in effect, accepts practical success as a sufficient criterion of the truth of any theory. In short, if the theory works, it must be true, since that is all we ever want of a theory. Conventionalism takes a very different tack. It says that it is a mistake to think that scientific theories are true. Instead, any given theory is only 'better' or 'worse' than some other competing theory. In short, no theory is true, or provable by reference to facts. For adherents of Conventionalism, a theory is a convenient description of, or filing system for, the existing facts. Some filing systems are better than others. According to Samuelson's version of Conventionalism, 'explanation' is merely the name we give to a 'better description' [1965].

In order to distinguish Pragmatism let us restate the nature of Conventionalism. Conventionalism is designed to deal with the classic Problem of Induction but it does so by redefining the problem by changing it into a problem that can be solved. Conventionalism is designed to solve the revised problem of choosing the 'best' theory among several competitors. The 'best' is always relative (i.e. subject to conditions). That is, there is no claim that the 'best' theory is necessarily the one 'true' theory; this is the quintessence of Conventionalism. There are many different versions of Conventionalism which differ only to the extent that there are different criteria to be used to choose the 'best' theory.

All versions of Conventionalism require generous amounts of handwaving and clever philosophical analyses to be convincing. Pragmatism is much more straightforward. Whatever 'works' is true. If a theory does not work, it cannot be true. If it is true, it will work. If it is false, then eventually we will find that there is something for which it does not work.

The important point we wish to stress here is that both Conventionalism and Pragmatism are based on the acceptance of the necessity of dealing with the Problem of Induction. The former deals with the problem by denying its original objective, which was to establish the truth of scientific theories. The latter deals with the problem by accepting a weak criterion of truth, namely, 'usefulness'. Friedman's 1953 essay is often mistaken for a version of Pragmatism. Some followers of Conventionalist methodology unfortunately think they are opponents of Friedman's methodology because the latter often invokes usefulness as its primary objective. They miss the point, however. Friedman's methodology also rejects Pragmatism!

Instrumentalism and the usefulness of logic

It is nevertheless true that once one recognizes 'usefulness' as a criterion of truth one is immediately reminded of the many methodological prescriptions emanating from the so-called Chicago School. The source is allegedly Friedman's 1953 essay which presents his version of Instrumentalism, the view that theories are only useful tools or instruments and they are not intended to be true. Many of the followers of Friedman's essay on methodology claim that the only criterion to use when it comes to assessing a given theory is the theory's usefulness. The question we should ask is whether by 'usefulness' they mean the same thing as do orthodox Pragmatists. To answer this we must look at what Friedman's essay contributes to the discussion, so let us now turn to a discussion of the philosophical basis of Friedman's methodology, drawing upon some of our previous examinations [Boland, 1979a, 1980, and 1981a].

For the purposes of discussing Friedman's methodology, one can consider any theory to be an argument in favor of certain given propositions or specific predictions. As such, a theory can be considered to consist only of a conjunction of assumption statements, each of which is *assumed* (or asserted) to be true. In order for the argument to be sufficient it must be a deductive argument, which means that at least some of the assumptions must be in the form of general statements [Popper, 1934/56]. But, without an inductive logic, this latter requirement seems to raise all the methodological problems we discussed in Chapters 1, 7 and 8. When can one assume a theory is true? It is such difficulties that Friedman's essay attempts to overcome.

As long as a theory does its intended job, there is no apparent need to argue in its favor, or in favor of any of its constituent parts. For some policy-oriented economists, the intended job is the generation of 'true' or successful predictions. In this case a theory's predictive success is always a sufficient argument in its favor. This view of the *role* of theories is called 'Instrumentalism'. It says that theories are convenient and useful ways of (logically) generating what have turned out to be true (or successful) predictions or conclusions. Instrumentalism is the primary methodological point of view expressed in Friedman's famous essay.

For those economists who see the object of science as finding the *one* true theory of the economy, the task cannot be simple. However, if the object of building or choosing theories (or models of theories) is only to have a theory or model that provides true predictions or conclusions, *a priori* truth of the assumptions is not required *if* it is already known that the conclusions are true or acceptable by some Conventionalist criterion. Thus, theories do not have to be considered true statements about the nature of the world, but only convenient ways of systematically generating the already known 'true' conclusions.

In this manner Instrumentalism offers an alternative to the Conventionalist response to the Problem of Induction. Instrumentalism considers the truth status of theories, hypotheses, or assumptions to be *irrelevant* to any practical purposes, as long as the conclusions logically derived from them are successful. Although Conventionalism may deny the possibility of determining the truth status of theories, Instrumentalism simply ignores the issue. Some followers of Instrumentalism may personally care about truth or falsity, or even believe in the powers of induction, but such concern or belief is considered to be separate from the Instrumentalist view of the role of theories in science.

There are only two useful ways of employing formal logic which we discussed in Chapter 7. There is *modus ponens*, which is valid only for arguments from the truth of one's assumptions to the truth of one's conclusions; and there is *modus tollens*, which is valid only for arguments from the falsity of one's conclusions to the falsity of one's assumptions. By the adherents of Instrumentalism, who think they have solved the Problem of Induction by ignoring truth, *modus ponens* will necessarily be seen to be irrelevant. This is because they do begin their analysis with a search not for the true assumptions but rather for true or useful (i.e., successful) conclusions. (Note that 'analytical theorists' start in the same way but they seek only logically valid conclusions!) *Modus tollens* is likewise irrelevant because its use can only begin with false conclusions.

Pragmatism vs Instrumentalism

The point we wish to stress is that the criterion of 'usefulness' is not being applied to the same problem in each case. What Pragmatism desires is a truth substitute in order to provide what the old 'scientific method' was supposed to provide, a solution to the Problem of Induction. Instrumentalism, such as the view presented in Friedman's essay, does not seek a truth substitute. Instead, the Problem of Induction is dismissed. In fact, all such philosophical problems (and solutions such as Pragmatism) are dismissed. The only question at issue concerns which method is appropriate for success in choosing theories as guides for practical policies.

If followers of Instrumentalism reject Pragmatism, how do they assure the truth of the theories they wish to use? The answer is that they do not require such an assurance. Truth is not essential for practical success. When we take our television set to the repair man, we do not usually think it is necessary to quiz the repair man about his understanding of electromagnetics or quantum physics. For our purposes, it can be quite adequate for him to believe that, for example, there are little green men in those tubes or transistors and that the only problem is that one of the little green men is dead. As long as the tube or transistor with the dead little green man is replaced and our television set subsequently works, all is well.

This is the essence of Instrumentalism. If emphasis is being placed on success and there are no doubts about one's success – for example, the television set does, in fact, now function properly – there is no immediate need for a philosophical substitute for inductive science. However, it is also clear that since truth is not necessary, there is no need to confuse success with truth. Thus we see, while success-in-use is a criterion of truth for Pragmatism, for Instrumentalism it is not. Unlike Pragmatism or Conventionalism, which both offer a way to resolve the Problem of Induction, Instrumentalism does not attempt to deal with that philosophical problem. That is, Instrumentalism does not attempt to establish the truth of scientific theories, since truth is not necessary for practical success.

The Methodological Differences

This brings us to the alleged differences between Conventionalism and Friedman's Instrumentalism. Our argument here is simply that, contrary to popular opinion, the followers of Instrumentalism and Conventionalism do not necessarily disagree. Their differences are at cross-purposes. Conventionalism and Instrumentalism agree that there is no direct solution to the Problem of Induction; and that the Pragmatist solution may be rejected. They only disagree about what we should do about the Problem. While Conventionalism looks for some criterion to provide a truth substitute, Instrumentalism looks for short-run criteria which promise immediate success. There is no claim that Instrumentalist criteria are adequate truth substitutes. The classic dispute is between 'generality' and 'simplicity'. The former criterion is typical of Conventionalist objectives; the latter is typical of Instrumentalist objectives.

Conventionalist 'simplicity'

If one were to consider Friedman's methodology as a solution to the Problem of Induction (which would be an error), then one might see his methodological prescriptions as direct competitors with orthodox Conventionalist prescriptions, since all versions of Conventionalism seek criteria to use in the allegedly necessary task of choosing between competing theories. In this sense, analytical economists see Friedman's advocacy of simplicity as a rejection of generality.

Let us consider how simplicity might be desirable from a Conventionalist's standpoint. Simplicity is advocated by those Conventionalists who believe that Nature is essentially simple. Historically, simplicity was invoked because many philosophers would invent complexities in order to overcome the failure of their explanations. The historical details do not matter here, so let us illustrate this with a modern example. Let us say that someone might see the demand curve as a mathematical function relating the price to the quantity demanded. Supposedly, if we know the price, then we can calculate the quantity demanded. The demand function says that any time the price changes, the quantity changes in a predictable way. In some sense, then, the price is used to predict the demand. This would be the simplest possible explanation of the quantity demanded, as there is a minimum number of variables involved – two: the price and the quantity demanded. Now if it were observed that the price changed but the quantity demanded did not, how would we explain this? The only way is to introduce a third variable, say, income. Thus, it might be argued that although the price changed, so did the consumer's income, so that the effects of the price change alone were cancelled out by the income

change. The obvious instance would be that whenever prices double and incomes double, the demand will not usually change.

This illustration is not intended as a criticism of demand theory. Rather, we are suggesting that no matter how many variables are involved or introduced, we can always explain away any insufficiency in our original theory by introducing a new explanatory variable. But is the introduction of additional variables an acceptable way of dealing with failures to explain? Surely such a method of dealing with explanatory failures could get out of hand. We could have so many variables that there would be one variable for every possible change. With so many variables things could get very complex.

Sometimes we have to admit that our explanations are wrong. But if we are allowed to invent new variables to explain away our failures, such admissions can be postponed for ever. This, historically, is the type of situation that fostered the desire for simplicity. The methodological prescription used to be that whenever facing the choice of two competing theories, always choose the one with fewer variables or conditions. This prescription would reduce the chance of opting for a complex theory which merely covers up an inherently false theory. Note that this prescription of simplicity can be misleading, since the true theory may actually be very complex!

Not all followers of Conventionalism advocate simplicity; some like Lucas and Samuelson advocate generality. Generality is the criterion invoked by those followers of Conventionalism who wish to explain much by little. The Conventionalist view that a theory is but a filing cabinet for systematically storing and describing available facts leads to the view that the more that can be stored, the better. This is the essence of the criterion of generality. The more the situations that can be described, the more general is the theory. In terms of the theory of demand, the ability to deal with various types of goods (e.g. normal, inferior, and Giffen, as well as complements and substitutes) is a definite plus for the generalized form of demand theory which Samuelson presented in his *Foundations* [1947/65].

This, then, would appear to be the difference between generality and simplicity. But is the difference so (sorry...) simple? Even when the number of variables is low, the relationship between them could be very complex. What one is looking for, given the Conventionalist penchant for choice-criteria, is a theory which is both simple and general. Thus, on purely Conventionalist grounds, there is no necessary choice between simplicity and generality, as it may only be a question of personal tastes.

Instrumental simplicity

Adherents of Instrumentalism do not usually advocate generality and they desire simplicity for entirely different reasons. For Instrumentalism the only criterion to be considered is the practical success of a theory; otherwise anything goes. General theories are all right if they work. The reason why Instrumentalism values simplicity is that simple theories are easier to implement. They require less information. If there are few relevant variables, then there are few calculations to be made in the predictions. There is not much more to say than that. The only caution is to note that a small number of variables does not always imply simplicity. Two variables could be related in a linear fashion, as with a straight-line demand function. On the other hand, two variables could be related by means of a very complex polynomial of a very high degree. Thus it is possible for the relationship between three variables to be less complex than the relationship between two variables.

From the perspective of Instrumentalism, there is no need to impose arbitrary criteria such as simplicity or generality. The only relevant criterion is whatever works. Simplicity arises only because it is related to the practical question of the amount of information needed to implement any given theory. But the difficulty of collecting information may not always be a problem. In such cases, it is possible for the more general theory to be more useful than the less complex. So be it. From the stand point of Instrumentalism, the only prescription is to choose the theory which is most useful.

Critiques of Friedman's Essay

Friedman's essay elicited a long series of critiques. The most popular of these were by Koopmans [1957], Eugene Rotwein [1959], Samuelson [1963] and, to some extent, Herbert Simon [1963]. All of these critiques fail because they misunderstand that Friedman is merely stating his version of Instrumentalism.

Most of the misunderstandings are the result of Friedman's 'Introduction', where he seems to be saying that he is about to make another contribution to the traditional discussion about the methodology of Inductivism and Conventionalism. Such a discussion would usually be about issues, such as the verifiability or refutability of truely scientific theories. What Friedman actually gives is an alternative to that type of discussion. Unfortunately, most critics miss this point. Consequently, the critiques are quite predictable.

Koopmans takes Friedman to task for dismissing the problem of clarifying the truth of the premises – the problem that Koopmans wishes to solve. The source of the disagreement is Koopmans' confusion of 'explanatory' with 'positive' [see 1957, p. 134]. Koopmans, adhering to Inductivism, would define *successful* explanation as being logically based on observably true premises, that is, ones that are in turn

(inductively) based on observation. Friedman does not consider assumptions or theories to be the embodiment of truth but only instruments for the generation of useful (because successful) predictions. Thus, for Friedman 'positive' is not equivalent to 'explanatory' because he does not use *modus ponens*. Explanation in Koopmans' sense is irrelevant to Friedman's Instrumentalism. Followers of Friedman's methodology can easily escape from Koopmans' critique.

Rotwein merely asserts that everyone should adhere to optimistic Conventionalism, which he calls 'empiricism'. Specifically, empiricism prescribes that everyone must justify every claim they make for the truth of their conclusions or predictions. Amazingly, Rotwein as a follower of empiricism recognizes that Hume showed that 'there was no reasoning that could justify (inductively) expectations that past regularities would be repeated in the future' [1980, p. 1554]. But rather than drop the presumed need to justify one's empirical claims, Rotwein says: 'Hume, however, held that such expectations were to be accepted because, given the kinds of creatures we are, or the manner in which we form our beliefs, we had no alternative to their acceptance; and this view has been central to the empirical tradition ever since his time' [1980, p. 1555]. Somehow, in everyone's head there is supposedly a perfectly functioning inductive logic which does what we cannot do outside our heads. How do the empiricists who follow Hume 'know' that there is such a functioning induction? This form of empiricism is silly and Friedman is quite free to dismiss it as such.

Simon's critique of Friedman's essay is based on the acceptance of a surrogate inductive learning function which Simon calls 'the principle of continuity of approximation'. Simon says that 'it asserts [that] if the conditions of the real world approximate sufficiently well the assumptions of an ideal type, the derivations from these assumptions will be approximately correct' [1963, p. 230]. This principle is nothing more than a sophisticated version of the inductive principle often used by mathematicians to avoid the intractable complications caused by the absence of an inductive logic [see Boland, 1979a, pp. 506-7]. Formally, Simon's principle would appear to be a restatement of *modus ponens*, but, as we explained in Chapter 7, there is no valid *approximate modus ponens* [see also Haavelmo, 1944, p. 56]. It is to Friedman's credit that he did not opt for this sophisticated subterfuge which smuggles successful induction in through the approximate back door.

Samuelson's critique is easily the most popular. Many critics of Friedman's economics are eager to believe that here is a critique which works. And since Samuelson's is so obscure, it is easy to accept it as an adequate critique because it is not well understood. Samuelson tries to criticize Friedman's methodology by attempting to argue that it is self-contradictory. Specifically, he offers a false theory of the motivation for Friedman's methodology and applies the false theory to explain the behavior of Friedman's followers. By implication we are supposed to conclude from the alleged successful explanation that there is some merit in his deliberately false assumptions. This implication is supposed to be a criticism of Friedman's use of the 'as if' principle, but it is a misuse of that principle.

Perhaps Samuelson is correct in attributing a pattern of behavior to the followers of Friedman and in positing that such a pattern can be shown to follow logically from his assumption concerning their motivations, but the 'as if' principle still does not warrant the empirical claim that his assumption about Friedman's (or Friedman's followers') motivation is true. More important, the 'as if' principle is validly used only when explaining true conclusions [Boland, 1979a, pp. 512-13]. That is, one cannot validly use such an 'as if' argument as a *critical* device similar to *modus tollens*. If the *implications* of using Samuelson's false assumption are undesirable, then one cannot pass the undesirableness back to the assumption. Furthermore, there are infinitely many false arguments that can imply any given (true) conclusion. The question is whether Samuelson's assumption is *necessary* for his conclusion. Of course, it is not, and that is because Samuelson is imitating Friedman's mode of argument, which relies on sufficient conditions for success.

The irony of Samuelson's critique is that his followers accept it *as if* it were successful. Logically, there is no way Samuelson's criticism can be considered successful, since such a line of argument requires logically necessary assumptions. But worse than this, most critics of Friedman's essay object to its dismissal of the necessity of 'realistic' assumptions, yet Samuelson's criticism is based on deliberately 'unrealistic' assumptions! These critics are caught violating their own requirement in order to criticize Friedman's essay. In effect they employ 'as if' arguments while criticizing their use. By their own rules they should reject their own critiques.

Conventionalist Critiques of Instrumentalism

There have been many Conventionalist critiques of Instrumentalism [cf. Caldwell, 1980]. All of them have viewed Instrumentalism as just another alleged solution to the Problem of Induction. What is surprising about this is that Instrumentalism is a rejection of the philosophical questions addressed by Conventionalism.

In a previously published defense of Friedman's essay against what we considered to be unfair critiques [Boland, 1979a] we stressed the importance of distinguishing Friedman's Instrumentalism from the Conventionalist philosopher's alternatives that are more concerned with methods of establishing the universal truth (or probable truth) of scientific theories. The key issue is the separation of purposes, that is, the separation of immediate practical problems from long-term philosophical questions. Although Instrumentalism may be appropriate only for the former, the view that Conventionalism is the superior alternative is at least open to question. It is time to examine critically the logic of Conventionalism and its relationship to Instrumentalism.

Realism of assumptions

The success of Instrumentalism is based on the following proposition: in the short run or for most practical problems, one's theories do not have to be true to be successful. Our story of the television repair man clearly illustrates this. As we argued [Boland, 1979a, pp. 512-13], logically the truth (or probable truth) of one's assumptions is not necessary. To say that it is necessary is the 'Fallacy of Affirming the Consequent'.

Instrumentalism through Conventionalist eyes

The common error of seeing the necessary superiority of Conventionalism over Instrumentalism is the result of falsely assuming that one's own objectives are shared by everyone. If Friedman's Instrumentalism were intended to be an all-encompassing philosophy of science, any modern philosopher could easily be dissatisfied. But we argued [Boland, 1979a, p. 510] that although Friedman gives an appropriate bow to J. N. Keynes, Friedman's approach is to drop the traditional problem posed by Keynes because its solution would require an inductive logic. Friedman's method of dealing with the question of a 'positive science' is to limit the domain of the question in the case of economics to only that which is appropriate for a practical policy science. Limiting the domain of applicability for any method or techinque is a rather obvious Instrumentalist ploy – one which can easily be justified in Instrumentalist terms.

Philosophical comparisons of Instrumentalism with Conventionalism are not uncommon; but we think they can be misleading if presented only in Conventionalist terms. The late Imre Lakatos was noted for considering Instrumentalism to be 'a degenerate version of [Conventionalism], based on a mere philosophical muddle caused by a lack of elementary logical competence' [1971, p. 95]. But his judgement is based on whether Instrumentalism is a means of achieving the objectives of most Conventionalist philosophers of science, and not on whether it is a useful guide for dealing with practical problems. In terms of Instrumentalist objectives, any advocate of Instrumentalism could argue that Conventionalist philosophy of science is obviously useless.

Moreover, as we have shown [Boland, 1979a], Lakatos is wrong; Instrumentalism on its own terms is devoid of the alleged elementary logical errors.

Some Words of Caution

Now before one jumps to the conclusion that the real choice is between Instrumentalism (i.e., Friedman's methodology) and Conventionalism (i.e., the methodology of Samuelson or Lucas) and, worse, that if one rejects Conventionalism, one must then embrace Instrumentalism for all of economics, let us add some further advice. Instrumentalism is always limited to short-run practical problems. If one is looking for a more universal, lasting understanding of the workings of the economy – that is, a true theory of economics – then Instrumentalism will never do, since it ignores the truth of theories. Of course, Conventionalism fails here too, since it denies any truth status to theories. If a true theory of the economy is our objective, then we will just have to look beyond the dispute over methodology between Friedman's Instrumentalism and the Conventionalism of Samuelson or Lucas.

PART IV

THE FOLLY OF AN ALL-PURPOSE METHODOLOGY