CONTENTS

Econometrics as observation: the Lucas critique and the nature of econometric inference

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1 THE LUCAS CRITIQUE

Perhaps the principal challenge to the use of econometric models in economic analysis is the policy non-invariance argument, popularly known as the 'Lucas critique'. Robert Lucas (1976) attacks the use of econometric models as bases for the evaluation of policy on the grounds that the estimated equations of such models are unlikely to remain invariant to the very changes in policy that the economist seeks to evaluate. The argument is originally cast as an implication of rational expectations. Among the constraints people face are the policy rules of the government. If people are rational, then, when these rules change, and if the change is correctly perceived, they take proper account of the change in adjusting their behavior. The rational expectations hypothesis implies that changes in policy will in fact be correctly perceived up to a serially uncorrelated error.

The Lucas critique challenges macroeconometrics along two related paths. First, it suggests that existing models are useless for evaluating prospective changes in policy; second, it suggests that existing models are not accurate representations of even the current structure of the economy. I want to suggest that the first path can be thought of as a denying that econometric models typically capture the true causal structure of the economy, and that the second path can be thought of as denying that the models are identified in the econometrician's usual sense. These are closely related ideas, but they are not identical. Christopher Sims (1980: 1) faults large-scale econometric models for relying on 'incredible' identifying restrictions; yet Sims (1982; cf. Hoover 1988a: 197–202) asserts their usefulness, in a highly restricted sense, in evaluating alternative policies.

The concept of cause and which concepts are appropriate in which circumstances are hotly debated among some econometricians and economic methodologists (see, e.g., Granger 1980; Leamer 1985; Zellner 1979, and the supplement to the *Journal of Econometrics* 1988). The notion of cause as control, however, seems naturally appropriate in the debate over the Lucas critique. Roughly, a change in policy causes a

change in the economy if the change in policy can be used to control that aspect of the economy. I have argued elsewhere that a good rendering of this notion of causality is to be found in J.L. Mackie's (1980) conditional analysis of causality.[1]

Mackie defines a cause to be an Insufficient, Non-redundant member of a set of Unnecessary but Sufficient conditions for the effect. This is often called the INUS condition. Simply put it says that *a cause* is a critical part of one of the possibly numerous alternative combinations of circumstances that imply an effect. The details of Mackie's analysis are not important in the current discussion. What is important is to notice that causal relations are captured in this analysis by (contrary-to-fact) conditional propositions; that is, by statements of the form, 'if it were true that the economy was at full employment and the money supply increased by 10 percent, then it would be true that prices would rise by 10 percent'. Such a proposition sustains our belief that increases in the money supply cause increases in the price level. Yet its correctness is not challenged by our failure to observe the antecedents to be fulfilled.[2]

The conditional analysis of causality is related to both invariance and control. Consider a putatively correct conditional proposition whose antecedents are not fulfilled. If it happens that when the antecedents are in fact fulfilled, the conditional proposition is no longer correct, it was wrong to assert it in the first place. Any conditional proposition asserts the existence of a disposition. The properties of such a disposition cannot depend on whether or not it is in fact actualized. Thus, 'a diamond is hard enough to scratch glass' asserts a disposition. It is correct whether or not a diamond is ever used to scratch glass. But it would not be correct if hardness were attributed to the diamond only when no one attempted to scratch glass. Such dispositions necessarily presuppose invariance. And the conditional analysis of causality is partly the assertion that causal relations are invariant to attempts to use them to control the effects. Policy interventions are then connected to effects in the economy through invariant causal relations. The Lucas critique can thus be read as the claim that existing macroeconometric models do not isolate causal relations – i.e., they do not assert correct conditional propositions.

An INUS condition is not the complete cause of its effect. In general, we are interested in some INUS conditions but wish not to pay direct attention to others. Both Federal Reserve policy and the institutional structure of Wall Street may be INUS conditions of the term structure of interest rates; but a bond trader is directly interested in Fed policy and generally relegates institutional structure to what Mackie calls the 'causal field'. The causal field is simply the set of INUS conditions, which either do not change or which serve as boundary conditions for the problem at hand. To say that a causal relation is invariant to interventions of control leaves unstated the caveat, 'within a particular causal field'.

In a related paper (Hoover 1990), I demonstrate that one can link Mackie's conditional analysis of causality with a characterization of causality in systems of equations first developed by Herbert Simon (1953). Causality in a linear system of equations can be associated in Simon's analysis with block recursion between variables.[3] A variable ordered ahead of another in the recursion is said to cause the other. Since each equation can be thought of as a conditional proposition with its parameters and variables as antecedents, it is easy to surmise that some sort of mapping exists between Mackie's analysis and Simon's. Simon demonstrates that when a system of equations is causally ordered it is identified econometrically. The Lucas critique can therefore also be seen as the claim that existing macroeconometric models are in fact not identified.

2 IDENTIFICATION AND CAUSALITY

Models are formal representations that are meant to capture or mimic reality. Causality can be defined within the context of the model as Simon does or as a property of the world as Mackie does. The distinction between causal relations as they truly are and representations of them suggests that an important empirical problem is how we might infer from data what the underlying causal relations are; that is, how we might learn how our formal representations should be constructed or, given a tentative model, how we might check whether it has been constructed satisfactorily. The invariance property of causal relations might be exploited as a source of information. If models prove to be invariant to a wide class of interventions, such as changes in policy or in institutional structure, we have some grounds for tentatively accepting them as representations of the true causal structure – at least within the limits of the particular causal field justified by the range of the interventions. Equally, even if we do not postulate a causal model, we may by observing the behavior of statistical relations between observable variables under known interventions still be able to discern some of the causal links that any satisfactory model must represent.

These two approaches to learning about causal relations by means of the invariance property – that is, testing models and seeking restrictive criteria for any satisfactory model – are quite different, although not necessarily competitive. Both are strategies to secure invariance in economic models and, thus, to lend support to the attribution of verisimilitude to them. In section 3 below we will examine both approaches under the headings 'apriorism' and 'econometrics as observation'.

The Lucas critique and the new classical worldview generally suggest that a strong argument against the observational approach to assessing causal relations is that observed regularities are not autonomous but merely derivative. New classical economists argue that, since some of the important variables in the true causal ordering, such as expectational

variables, are intrinsically unobservable, observable statistical relations cannot be autonomous. Furthermore, even the true causal relations will be complicated, especially if, as is an article of faith among the new classicals, people use all the available information economically. Thus, they argue that it is the a priori approach which has the best hope of success, because it is only if one knows how to specify the linkages among observable variables, including those implied by their statistical relations to unobservable variables, that invariance can ever be observed.

To the econometrician, perhaps the most important obstacle to estimating models of economic processes is the so-called 'identification problem'. The root of the problem is this: economic theory uses variables to describe economic processes which are not observable; observable variables are the outcome of interactions among these unobservables; and without further information it is, in general, not possible to infer the behavior of the unobservables from the observables. The paradigm identification problem is the simple system in which desired or planned supply is an increasing function of price alone and desired or planned demand is a decreasing function of price alone. Together these functions determine an equilibrium observable outcome – the amount sold. The identification problem is that from observing this single price/quantity combination, one cannot infer what the underlying functions are. The problem is no easier in the presence of random shocks to the functions; the single observed point is then simply replaced by a scatter of points randomly distributed about the equilibrium. If we have enough additional information, say that the variance of the random shocks to the supply curve is much greater than that to the demand curve or that supply is also a function of, say, rainfall, then it may be possible to infer the shape of the underlying functions (subject to some random error), in this case because the movements of the supply curve from random shocks or variability in rainfall force the observed price/quantity combinations to trace out the demand curve.

The Lucas critique is a variation on the theme of the identification problem. Just as we find observed price/quantity patterns (a point, a scatter or a line, depending on the nature of the actual underlying relation) jumping about when underlying but unaccounted for factors, such as the level of rainfall, change, we notice that estimated (presumed) behavioral functions appear not to be stable in the face of policy changes not accounted for in the estimate. When seen in this light, the Lucas critique clearly did not originate with Lucas and deserves to bear his name only because he brought the invariance problem home to most economists more forcibly than any earlier author and because it serves as a convenient shorthand.

\Lucas (1976) himself claims no originality for the non-invariance argument, suggesting that it is implicit in the work of Frank Knight, Milton Friedman and John Muth. Lucas does not, however, notice the explicit

statement in Trygve Haavelmo's famous paper, 'The probability approach in econometrics' (1944). Haavelmo compares the estimation of simple econometric relations to working out the relation between the amount of throttle and the speed of a car on a flat track under uniform conditions. The relation may be precise; but change the surrounding conditions (e.g., take the car off the track or allow the engine to get out of tune), and the relation will almost certainly break down. Haavelmo (1944: 28) contrasts the lack of autonomy of such empirical regularities with such things as the laws of thermodynamics, friction and so forth, which are autonomous because they 'describe the functioning of some parts of the mechanism *irrespective* of what happens in some *other* parts'. What Haavelmo suggests, in the terminology of Zellner (1979), is that non-autonomous relations are not lawlike; they do not represent the underlying causal ordering. It will not do to overstate the case; so Haavelmo goes on to argue that autonomy is a matter of degree. Again this can be rephrased: a causal ordering is claimed to be invariant only with respect to a particular causal field, which may itself be of a broad or a narrow compass. In some cases, then, it may be useful to see the non-invariance of a relation (in the car example, for instance) as a change in the causal field. Haavelmo does not himself state the invariance problem in terms of representing causal relations, but Simon (1953: 25–7) puts it in exactly such terms and is, therefore, another precursor of Lucas. Simon (1953: 27) writes:

> causal ordering is a property of models that is invariant with respect to interventions within the model, and structural equations are equations that correspond to specified possibilities of intervention.

But it is precisely this same notion of intervention, and this same distinction between structural and nonstructural equations, that lies at the root of the identifiability concept.

Although Lucas was not the first to recognize the invariance problem explicitly, his own important contribution to it is to observe that one of the relations frequently omitted from putative causal representations is that of the formation of expectations. He notes, further, that the formation of expectations may depend upon people's understanding of the causal structure of the economy in general and of the process of policy formation in particular. This is why the rational expectations hypothesis is often linked with the Lucas critique. The important point about the analysis of non-invariance provided by Haavelmo and Simon is to remind us that rational expectations is simply one means by which causal relations may be linked; and, in general, it is the omission of *any* causal relation related to those remaining that produces non-invariance.[4] Lucas and his predecessors have, then, diagnosed a problem for economic analysis. We must now turn to proposed cures.

3 STRATEGIES FOR SECURING INVARIANCE

Apriorism

The invariance problem can be treated in two complementary ways: as a problem of representing causal relations or as a problem of identifiability. In discussing the problem of securing identification, econometric textbooks recommend that restrictions be imposed a priori on the basis of economic theory.[5] Thus, the second approach explains why the most frequently proposed strategy for securing invariance in econometric models subject to the Lucas critique is to structure such models around restrictions deduced a priori from economic theory. This *apriorism* is the received view of two generations of economists. Apriorism comes in two forms – strong and weak. Strong apriorism can be traced back to Tjalling Koopmans' attack, in his paper, 'Measurement without theory' (1947), on the atheoretical methods of Wesley Mitchell and his colleagues at the National Bureau of Economic Research. The strong apriorist view has been forcibly restated by Thomas F. Cooley and Stephen F. LeRoy (1985). They argue that work by the Cowles Commission in the 1940s – particularly by Simon and Koopmans – established the need to impose identifying restrictions on econometric equations if estimates of structural parameters are to be obtained. Furthermore, they argue that such restrictions are untestable. The only basis one can possibly have for imposing them, therefore, is that they are derived from a well-articulated theory acceptable a priori. By theory Cooley and LeRoy seem to mean a well-specified, consistent optimization problem. They go on to observe that the rational expectations hypothesis, which often generates a high degree of interdependence between variables in theoretical models, suggests that even economic theory cannot provide the restrictions needed to identify large-scale macroeconometric models. In this they are at one with Lucas and Sargent and other new classical economists, who argue that, only if economic theories are grounded in well-specified optimization problems, taking tastes and technology alone as given, will they be secure from the invariance problem (Lucas and Sargent 1979; Lucas 1981: Introduction).

The econometric analogue of the apriorist's belief in the dominance of economic theory is the familiar view that the *objective* significance of an econometric result varies with the means by which that result is obtained. On this view, good econometrics starts with a hypothesis derived from theory which dictates certain expected signs and significance levels of coefficients. It then estimates a regression and checks whether the result accords with the a priori expectation – if yes, fine; if no, back to the theoretical drawing board.[6] Bad econometrics tries out arbitrary (i.e., atheoretical) specifications until the results suit the investigator's prior beliefs. The strong apriorist's view of econometrics has the implausible,

counterintuitive result that, if I happen to estimate a confirming regression at the first go, my theory is supported; while, if you stumble on to the same estimate after 'data-mining', your (perhaps identical) theory should be neither supported nor rejected. Objectivity in any science should require that the identity of the investigator not affect the significance of an empirical observation. If the regression in question happened to be an exact replica of the process which generated the actual economic data, it would be ludicrous to accept it when it was arrived at by a first lucky guess and not when it was obtained by trial and error.

Apriorism is deeply ingrained in recent economic thinking. Yet many economists would not wish to adopt so strong a view as the new classicals seem to require. It is difficult to insist emphatically on the priority of economic theory when it is appreciated that there is no unanimity among economic theorists. Most, though not all, economists agree that economic theory should be grounded in a Walrasian general equilibrium approach founded on optimization by individual economic agents. There is less agreement on the use of the rational expectations hypothesis, although it is a common assumption. The consensus over what constitutes an adequate basis for economic theory does not rest on overwhelming empirical support.[7] Indeed, the strong apriorist denies that such support is forthcoming. Rather, the consensus rests upon the tacit agreement of theorists trained and working in a particular framework. Peirce (1957: chapter 5, esp. p. 196) calls such tacit agreement within a community the 'method of public opinion' for fixing belief. He goes on to observe, however, that beliefs fixed by public opinion within a community frequently come unstuck when there is contact with another community with conflicting beliefs. In economics, communities may be defined by their purposes or the circumstances in which they work. It is well known how the changes in the purposes of economic policy in the face of stagflation in the early 1970s and the apparent failure of macroeconometric model to predict the course of economic events broke the consensus in macroeconomics and spawned alternative approaches of which new classicism was the most prominent theoretically. The new classical strategy in the face of the Lucas critique and the events of the 1970s is not to give up the search for invariance, but to broaden the scope of the search, and to found it on strong a priori principles. Two principles are fundamental: that agent's knowledge is systemic – i.e., it is based on, at least, implicit understanding of the true structure of the economy (rational expectations); and that agents are continuous and successful (to a serially uncorrelated random deviation) optimizers (Lucas and Sargent 1979: 304–9). Imposing such principles results in models for which the consequences of policy changes can be derived. Such models can then be empirically tested.

The difficulty with this strategy is that it provides neither guidance on how to proceed nor leeway to adjust assumptions, if the data are widely at

variance with the model's predictions. The new classical organizing principles are uncontradictable.[8] Such principles are either not binding, because observationally identical results are just as well generated from other principles, or too constraining, because they rule out adjustments to the model which might reflect possible, but perhaps 'irrational' behavior. The problem with strong apriorism as a research strategy is principally that it does not naturally lead to a progressive development of models or knowledge. And, in fact, new classicism has not been able to offer a compelling alternative device for making economic predictions to the battered but stalwart community of macromodellers. For however great the influence of new classicism, and especially of the rational expectations hypothesis, on economic theory, it has not delivered any decisive empirical results.

As Cooley and LeRoy recognize, the thorough-going optimization implicit in new classicism suggests that everything depends on everything else, not just in theory, but in practice. Such complete interdependence suggests that theory does not provide sufficient restrictions to identify structural models. This shows up in the problem of observational equivalence: theoretical models which are antithetical nevertheless imply identical observational consequences (e.g., Sargent 1976). In the face of such a difficulty, Cooley and LeRoy retreat to theory, giving up hope of securing identification unless there is a theoretical breakthrough. Others have given up theory and asked, 'what can be learned from the data alone?'[9]

The weak apriorist goes to neither of these extremes. Instead he recognizes that belief and inference stand in a relationship of mutuality: inferences are founded partly on unexamined beliefs; but these inferences, in turn, may suggest the modification of those beliefs. Thus, theory presents us with some a priori (in the sense of not *currently* questioned) restrictions on empirical investigation; while the empirical results help generate beliefs (or new theories) which are prior to further investigations. Haavelmo expresses the essentials of weak apriorism clearly:

> How can we actually distinguish between the 'original' system and a derived system ...? That is not a problem of mathematical independence or the like; more generally, it is not a problem of pure logic, but a problem of actually *knowing something* about the real phenomena, and of making realistic assumptions about them. In trying to establish relations with a high degree of autonomy we take into consideration various *changes* in the economic structure which might upset our relations, we try to dig down to such relationships as actually might be expected to have a degree of invariance with respect to certain changes in structure that are 'reasonable'.
>
> (Haavelmo 144: 29)

The objection to the new classical strategy of strong apriorism is not that it involves non-empirical principles (beliefs) – all empirical research does that. Rather, it is that it is committed so strongly to these beliefs that it does not permit them to adjust in the interplay of theorizing with the testing of theories. The strength of weak apriorism is precisely that it recognizes the need for such interplay.

Econometrics as observation

The strong apriorist sets a formidable task for empirical economics. The title of Koopmans's attack on atheoretical economics suggests that econometrics, following a strict reading of its etymology, is concerned with the direct measurement of structures suggested by economic theory to be replicas of economic reality. But such a task is, as the weak apriorist suggests, not practicable unless we already have a good idea of what constraints reality places on the structures that we wish to measure. Measurement requires prior theory; equally, theory requires prior measurement.

That this circle seems vicious is the result of the apriorist failing to observe a critical distinction. Haavelmo distinguishes between *autonomous* relations, which are invariant to a wide range of interventions, and (adapting a term from Ragnar Frisch) *confluent* relations, which are the result of (complex) interactions of autonomous relations. Confluent relations may appear stable until subjected to interventions. Haavelmo's 'autonomous relations' are essentially the same as what the econometricians Hendry and Richard (1982) call the *data-generating process*: that is, the true, but unknown and unobservable description of how the data came to take on particular values.

Drawing this distinction forces us to recognize that only on the merest chance would we estimate a relation structurally identical to the data-generating process. Only on such a chance would we directly measure the underlying reality. And, what is more, since nothing in our estimate certifies its truthlikeness, we would never be completely sure that we had estimated the data-generating process. In general, we must assume that we estimate confluent relations.

Regressions and other econometric results are, first and foremost, calculations, summaries of observable data. Economists customarily speak of these calculations as 'good', 'bad', 'valid' and 'invalid'. Given the distinction between the data-generating process and confluent relations, it would be more to the point to think of these econometric calculations themselves as observations of the confluent relations. As such they may be illuminating or useful or neither, but not valid or invalid.

An analogy may make the point clearer. Astronomers use telescopes to observe the planets. The observations made with telescopes are not valid or

invalid, but in focus or out of focus and, therefore, useful or not useful. The standard by which the usefulness of an astronomical observation is to be judged varies with what one seeks to observe. Filters that allow ultraviolet light to be singled out may wreck the normal visual spectrum. Whether or not this is good depends upon whether one wants to see ultraviolet and not green or blue. Econometric calculations are the economist's telescope, and the restrictions implicit in the specification of a regression, for example, act like the ultraviolet filter. The observations made with econometric calculations are observations of confluent relations, the consequences of the (probably) unknown data-generating process. As such they are the grist for the mill of theory. They are what theory must explain. Theory may in turn suggest new restrictions on econometric calculations as likely to be more illuminating than the initial ones. The ideal theory, nevertheless, explains not only these new results, but *all* observations – that is, it encompasses them. The strong apriorist view that econometrics should measure the coefficients of the data-generating process is clearly untenable. It amounts to the same thing as suggesting that astronomers directly observe Newton's laws with their telescopes, rather than the complex consequences for the planets of those laws.

The view adopted here that econometrics is best thought of as the observation of confluent relations shades into weak apriorism. It differs mainly in that it distinguishes sharply, as the weak apriorist does not, between the unobservable, but ultimately constraining, data-generating process and the observed confluent relations. Haavelmo, for example, treats autonomy in econometric relations as a matter of degree. Similarly, Zellner identifies lawlikeness with invariance to a broad range of circumstances and boundary conditions.[10] This may be a good standard for *lawlikeness*, but on the present view an actual *law* is distinguished in kind from such empirical relations by being an element of the data-generating process itself.

The analogy between econometrics and observational sciences such as astronomy suggests that criteria are needed to determine when an econometric calculation will be useful. That is, rules for focusing the telescope are needed. Such rules themselves are derived from theory – for the most part theory that both is not currently under scrutiny and is supplemental to the main investigation. Thus, a crude rule for focusing a telescope might be that the edges of the object in view should be sharply defined. This follows from the theory of optics that light travels in straight lines and from the presupposition that the object in view is in fact solid. Were the more central astronomical theory to suggest that the object in view was a gaseous cloud with poorly defined edges, optical theory might in turn suggest maximizing the received light as a focusing criterion. Theory, therefore, may modify observational strategy. On the other hand, it may have been the impossibility of observing sharp edges which

suggested the modifications to the theory that generated such a change in strategy. The process is mutual. It would be absurd to give the central theory as dominant a role as strong apriorism demands. This would be equivalent to requiring Galileo to have had a theory of lunar geology before accepting that he had observed what we now know to be mountains on the moon. Theory governs our interpretation of what we observe; but its absence does not prevent us from observing something that needs interpretation and explanation.

Statistical theory provides the econometric equivalent of the focusing rule for the telescope. The basis for widely accepted criteria is the verisimilitude of an econometric specification or model with the data-generating process.[11] Not knowing the actual process, we can nevertheless say that a model cannot resemble it unless its errors are random – that is, unless the part that we cannot explain is, at least provisionally, unexplainable, the model cannot be called truthlike. Typical criteria for randomness are: estimated errors should be white noise (i.e., not correlated with their own past – equivalently, they should have no autocorrelation); errors should be innovations (i.e., not correlated with other variables omitted from the model); and errors should be homoscedastic (i.e., of constant variance). If errors do not possess these properties, then it should be possible to formulate a different model that is better in the sense of having a lower variance and encompassing the first model. (In this case, *encompassing* means providing a basis for calculating what the coefficients and variance of the other model would be without in fact estimating it.) In addition, on weak assumptions, statistical theory leads us to expect errors to be approximately normally distributed.

Just as in astronomy, theory may also guide econometric observations. At the crudest level, theory suggests potential variables. It also requires that models not imply values out of the range of possible observations. A consumption function, for instance, must not generate predictions of negative consumption. On a higher plane, we may impose restrictions from economic theory on econometric estimates and test these restrictions against more general models. If they are accepted, then theory aids in understanding the significance of the observation; if not, the observation may suggest what element of the theory is unsatisfactory.

Another requirement is stability of coefficient estimates. Like consistency with theory, this criterion is on a different plane from the need for random errors. The very concept of randomness – unexplainability – justifies it as a necessary condition of verisimilitude. There is no necessary connection between stability and the true data-generating process. Economic reality no doubt changes – perhaps, even so frequently that stability is not to be found. Nevertheless, econometric observations would be practically useless if they were completely unstable. We must, therefore, count on finding some stability and on supplementing econometric

observations with other information, say institutional facts, if we are to distinguish between real changes in structure and our own inability to focus our observations. The criterion of consistency with theory is subject to similar strictures. It is useful only in that it aids interpretation of observations. We must not join strong apriorism in affording it overarching status. Observations must give grounds for reconsidering theoretical commitments.

4 REALISM VERSUS NOMINALISM

In adopting the view that econometrics is an observational tool, we take what is best from weak apriorism, while avoiding the pitfalls of strong apriorism. Our research strategy is progressive, because our observations are always known to be provisional, subject to improvement on grounds of better statistical technique or better theoretical interpretability, and because our commitment to a particular theory is not so strong as to preclude modification in the face of observations.

In adopting econometrics as observation we also implicitly commit ourselves to metaphysical realism, abjuring the nominalism implicit in strong apriorism. By nominalism I mean the philosophical doctrine that only individuals are real and that general relations (e.g., causality) do not exist independently of the observer.[12] The desire to avoid metaphysics is strong among most economists, having been brought up on the philosophy of logical positivism. But metaphysics cannot be avoided; it can only be done well or badly (cf. Peirce 1957: 53, 292, 293). A thorough discussion of nominalism and realism would take us too far down a philosophical byway. The nominalism of apriorist econometricians nonetheless presents a practical difficulty for their analysis.

Simon (1953: 24–6) noticed that any set of data could be represented by many incompatible causal structures. Such non-uniqueness is not a property of Simon's representation alone. Indeed, Sims (1977) shows in a set-theoretic analysis that a wide class of formal representations suffers from a similar lack of uniqueness. The most common view among econometricians is that the problem of non-uniqueness can be avoided only by a priori commitments to certain restrictions on allowable representations.

This problem arises from nominalism, which fails to distinguish between the relation as it exists in the world and the representation of it or the operational means of inferring it. So long as causality is seen as a relation which exists only because it is imposed by the observer, few restrictions will be placed upon acceptable formalisms; and even these few will arise in a priori commitments, not from observed reality.

Realism is the contrary doctrine to nominalism: namely, that general relations exist independently of the observer 'in the objects'.[13] Realism is the foundation of the conditional analysis of causality. For without it,

counterfactuals do not make sense. The nominalist Mach writes: 'The universe is not twice given, with an earth at rest and an earth in motion; but only once with its relative motions, alone determinate. It is accordingly, not permitted us to say how things would be if the earth did not rotate' (Mach 1941: 284; cf. Simon 1952: 56). The realist objects that as we perfectly understand counterfactual claims, the nominalist is in no position to forbid them. The world *is*, in a sense, twice given. It is because we assume that certain causal relations exist in the objects that we are justified in making predictions. If we can predict on the basis of our understanding of what causal relations are, then we can equally well say what would have happened had antecedents been different. The world is twice given in the sense that our representation of the causal relations in it is not an arbitrary categorization, but a better or worse replica of the actual causal relations in the world. Hence, a counterfactual claim is sustained if it can be deduced from our representation (model or theory) on the assumption that antecedents are different from what they were in fact. The counterfactual claim, then, stands or falls with the satisfactoriness of our model or theory at truly representing the world. Even though such claims are simply formal deductions from our models or theories, they are nonetheless claims about the real world, not about our models.

Strong apriorism, either as an answer to the invariance problem or as a guide to good econometrics, adopts the nominalistic position that only particular facts are real and that general relations are not there to be found by the shrewd observer, but are imposed from without. It is the extra-ordinary view that theory is paramount and binds reality, rather than reality placing constraints upon what an acceptable theory would look like. On the strong a priori view, deductions from a theory may be disconfirmed, but such disconfirmation does not touch the theoretical core of the theory; it merely suggests that the optimization problem was not fully specified. The strong apriorist believes that economic observations are secure only if they are guided by a priori theory; but what is it that is supposed to make theory secure if it is not economic observation?

Econometrics as observation, in contrast, is grounded in realism. The econometrician does not impose restrictions but tries to learn in what way data constrain theories. The distinction between the model as a more or less good replica and the world is maintained. And the autonomy of the causal structure of the world is not questioned. The problem is not to decide how the world must be to agree with theoretical principles, but to discover how it is in fact.

The difficult problem of how to tell whether or not our theory is a good replica remains, but it is separate from the problem of whether causal relations are our own creations or are in the objects.

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NOTES

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1   Hoover (1988b). Mackie's analysis is also explored in Addison *et al.* (1980a,b) and Hammond (1986).
2   I say that the conditional proposition 'sustains' our inference, following Mackie, and that it is 'correct' rather than 'true' to avoid becoming directly embroiled in sharp debate between philosophers over how or whether truth values can be assigned to conditional propositions (see Hoover, 1988b: 6, esp. fn 7).
3   Generalizing beyond the linear even to very general mappings between variables is relatively easy; see Mesarovic (1969), Katzner (1983): chapter 6 and Hoover (1990): 232–4.
4   A point rediscovered by Buiter (1980), who observes that policy non-invariance requires only that agents take *some* account of policy rules, not that they have rational expectations.
5   For example, Johnston (1972): sections 12.2–12.4; the source for most text-book treatments of identifiability is Koopmans (1950).
6   A variation on this theme is the pre-test estimator, which penalizes the statistical significance levels according to the amount of search engaged in; see Judge *et al.* (1980): chapter 3 and Leamer (1978): chapter 5.
7   This proposition is amply documented for general equilibrium theory in Weintraub (1983).
8   This is admitted for the market-clearing assumption, see Lucas and Sargent (1979): 310–12.
9   E.g., Sims (1980). Despite the unsatisfactory nature of their own response, Cooley and LeRoy's criticism of the equivocations and invalid deductions drawn from vector autoregressions by Sims and others remains correct and important.
10 In keeping with its empiricist spirit, Engle *et al.* (1983): 285 identifies strong exogeneity (weak exogeneity plus invariance) with Zellner-causality (predictability according to law).
11 Fuller explanations and defence of these criteria are found in the writings of Hendry and his colleagues; see, e.g., Hendry and Richard (1982), Hendry (1983), Ericsson and Hendry (1984).
12 Goodman and Quine (1947) is a classic statement of the nominalist point of view; while Peirce (1957): chapters 1, 2, 4 and (1934): chapter 10, argues strongly against it, in favor of realism.
13 This phrase is Hume's (1888 [1739]: 88; see Mackie 1980: chapter 1). The philosophical doctrine of realism is expounded by Peirce (see n. 1, above) and presented as a basis for the philosophy of science by Newton-Smith (1981).

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