

Response to Gordon and Ando

by Thomas J. Sargent

The paper by Sims and me is clearly not intended as a categorical indictment of building "structural" econometric models. We would both agree that in certain contexts structural estimation is possible and desirable. However, in the context of building large economy-wide models, we do mean to question whether existing economic theory really provides the reliable identifying restrictions needed to carry out structural estimation. It is our opinion that the identifying restrictions used in macroeconomic models are sufficiently questionable to make the exploration of alternative modeling strategies worthwhile.

Albert Ando and Robert Gordon each assert that macroeconomic theory is capable of supplying zero-one restrictions that are reliable enough to make unnecessary and undesirable our exploration of alternative devices for restricting the dimensionality of models, devices that don't build in the same macroeconomic theory that Ando and Gordon feel to be adequate. Backing up that assertion would, at a minimum, require delivering some examples of exclusion restrictions and restrictions on the orders of disturbance processes that have been deduced from explicitly stated economic theories. Only Gordon has even attempted to offer any examples. It is to Gordon's credit that he poses the issue concretely by offering up a specimen of a model embodying approximately adequate exclusion restrictions. To my mind, however, Gordon's sample model, which incorporates what he claims are the profession's consensus exclusion restrictions, really illustrates how shaky is his claim that we know enough to implement the classic Cowles commission strategy. For one thing, his model does not include any random disturbances; but, surely, they ought to be included, and it is restrictions on the processes governing those disturbances that are required in order to satisfy the identifiability conditions.[†] Gordon doesn't tell us what body of economic theory delivers those restrictions on disturbance processes. Further, is it really

[†]See Hatanaka [53].

true that the profession agrees on the exclusion restrictions in Gordon's equations (1) through (6)? Gordon excludes expectations about future prices and interest rates from the aggregate demand schedule, though microeconomic theories of consumption and investment assert that they belong in that schedule. Gordon excludes expected future income and also all rates of return except one from the demand schedule for money. Do explicitly stated theories of the demand for money deliver such exclusion restrictions? Rarely.[†] The principle governing expectations formation is not explicitly stated by Gordon, although if by E is meant the mathematical expectation (or, weaker, the orthogonal projection) operator and if the information set I_{t-1} is explicitly defined, then (6) becomes the hypothesis of rational expectations, which is a useful identifying restriction. But builders of macroeconomic models have not endorsed that hypothesis; they have never even advanced an economic hypothesis about expectations formation from which their versions of (6) could be derived.

Gordon suggests that there is wide agreement about the nature of the investment schedule. That claim is contradicted by comparing, say, the random completion model of Jorgenson [68], on the one hand, with the costly adjustment model of Lucas [83] and Eisner and Strotz,[‡] on the other hand. In addition, Tobin and Griliches, in [70], have gone on record as disagreeing with Jorgenson's [70] claim to have derived a Keynesian investment schedule in a setup with perfectly competitive factor and output markets. In such a perfectly competitive environment, it is generally agreed (although Jorgenson disagrees [70]) that firms don't possess demand schedules for a flow of additions to their capital stock.[§] And if it were not enough that we have no agreement about the determination of capital accumulation, our knowledge of the effects of capital accumulation on subsequent aggregate supply is not firmly rooted in theory. Measures of the capital stock are usually "doctored" or adjusted for capacity utilization in attempts to make the coefficient on capital positive and of the correct magnitude. No theoretical rationale has been offered for this procedure. Further, the procyclical behavior of output per hour, which seemingly contradicts the classical theory of production, is a phenomenon that has fostered a literature on short-run productivity — a literature containing *several* distinct theories about the phenomenon, each with prominent adherents.^{||} The dynamics of macroeconomic

[†]See Miller [94], for one example.

[‡]See R. Eisner and R. H. Strotz, "Determinants of Business Investment," Research Study 2, in *Impacts of Monetary Policy*, (Englewood Cliffs, N.J.: Prentice-Hall, 1963).

[§]Tobin's "Dynamic Aggregative Model" is one where firms have no investment demand schedule, one that behaves very differently than does the standard Keynesian model, but one that is just as theoretically respectable as the Keynesian model.

^{||}For examples, see Lucas [84], Okun [111], and the references on the subject cited by Okun [111, pp. 211-13].

models are determined in important ways by investment schedules and production functions, especially the dynamic responses to tax changes. These dynamics are not based on well-understood or widely-agreed-upon theoretical foundations.

Macroeconometric models have come under increasing attack from theorists precisely because they do not embody the results of coherent general equilibrium theorizing. Macro models are constructed in an *ad hoc*, piecemeal fashion without any concern for the consistency relations (for example, market clearing conditions) that general equilibrium analysis imposes.[†] Separate and even mutually inconsistent assumptions about the environment typically underlie the different schedules in macro models. The theorists complain that we use large macroeconometric models to evaluate the effects of alternative monetary policies but that the models themselves contain no explicit, coherent explanation for why equilibria emerge in which individuals hold money.[‡] In the light of the criticisms of macroeconomic models that leading theorists have made — criticisms more damaging and incisive than those I have made above — I find the contention that those models embody identifying restrictions derived from widely accepted theories to be a very extravagant claim.

It is true that the enterprise of estimating vector autoregressive representations that Sims and I undertake is a relatively modest one, especially when compared with the vision held by macroeconometric model builders of what they are doing. As we emphasized in our paper, the vector autoregressions will not, in general, remain stable across interruptions in policy regimes, so that the models cannot be used to simulate the effects of alternative policy regimes. Macroeconometric models do pretend to offer such capabilities, but there are strong theoretical reasons for expecting the existing macroeconometric models to break down across such breaks in regime — theoretical reasons backed up by the history of such models having repeatedly failed to hold up across regime changes.[§] Indeed, standard macroeconometric simulations typically incompletely specify the experiments performed, since it is rarely possible to deduce the optimization problems that agents are being assumed to solve from the description of the experiments themselves. Are the time paths of the exogenous variables for which simulations are performed to be viewed as drawings from some stochastic process? Or are they to be viewed as deterministic and hence perfectly known by agents in advance? The form of agents' optimal decision rules obviously depends on the answer to this question. Yet macroeconometric model simulators never

[†]For example, is there anything to guarantee that the distribution of yields that was assumed to confront the individual in order to derive a portfolio balance schedule, (2) in Gordon's comment, will be the same distribution that the complete model will imply? The answer is clearly no, given the way macroeconomic models are constructed.

[‡]For examples, see Hahn [49] and the papers he cites.

[§]See Lucas [92], and Muench, Rolnick, Wallace, and Weiler [99].

face the question, pretending instead that their simulations are valid for any *arbitrary* inputs of exogenous variables. Dynamic programs for agents simply can't be calculated for arbitrary inputs.

Even if one disagrees with my remarks about the tenuous theoretical content of the restrictions used to identify existing macroeconomic models, ignoring the empirical results turned up by the techniques that Sims and I have experimented with is not justified. As we have remarked, the intersection of observable and unobservable index models with standard econometric models is small. To the extent that low-order index models describe the data adequately, a challenge is set down for the standard models to meet. Further, this work can be rationalized as part of the process of turning up empirical regularities, regularities that theorizing should attempt to explain.

On Causality

Ando voices his suspicion that Granger's definition of causality is defective, by which I take it that he means Granger's definition of causality does not coincide with the way Ando and many of the rest of us use the word "cause."[†] It is true that Granger's definition of a causal relation does *not*, in general, coincide with the economist's usual definition of one: namely, a relation that is invariant with respect to interventions in the form of imposed changes in the processes governing the causing variables.[‡] Sims' test cannot in and of itself be relied upon to uncover causal relations in this usual sense. However, this observation in no way diminishes the value of Sims' test as a device for testing the adequacy of prior specifications about econometric exogeneity. The important thing that Sims showed is the coincidence of the condition "y doesn't Granger-cause x" with the condition "there exists a representation (a model) in which x is strictly econometrically exogenous with respect to y." Multivariate extensions of this coincidence also hold,[§] with the practical consequence that the validity of assumptions of strict econometric exogeneity, assumptions that are necessary to deliver good properties to the statistical estimators applied to simultaneous equations models, become testable. Use of Sims' test for two-sidedness is clearly not necessarily tied to bivariate systems but is fruitfully applied in the context of multivariate relations.^{||} In view of the wide use of distributed lags in

[†]Or else that Sims' test is not a valid one, a possibility that must be dismissed unless someone can show there to be an error in Sims' Theorem 2.

[‡]Sims suggests to me that it is not really so clear that economists' use of the word cause typically coincides with "invariance under an intervention" rather than "a one-sided relation with a strictly exogenous variable on the right-hand side." Certainly in the mathematics and engineering literature the concept of a causal relation coincides with the latter one. For example, see Naylor and Sell [100].

[§]This can be seen easily by interpreting the variates in Sims' appendix [136] as vectors.

^{||}In fact, two-sidedness of lag distributions is a symptom of more than just failure of

macroeconomic models, Sims' test is potentially an extremely important diagnostic device for checking on the quality of assumptions about strict econometric exogeneity, a device that rather greatly expands the set of instances in which one can test for exogeneity. From this point of view, it is irrelevant whether or not Granger's definition of causality agrees with the way we ordinarily like to use the word cause.

Ando dismisses Sims' results on money and income as a "mildly curious fact" and claims that we know that the true relation between these two variables is a more complex one about which Sims' regression tells us nothing. However, it is easy to show that if variables from the true structure have been omitted, as Ando claims Sims has done, it will generally lead to rejecting exogeneity assumptions that would be valid in larger systems, rather than to accepting the exogeneity of money as Sims was able to do. This is precisely what makes Sims' results remarkable. What Ando has not done, and what will be very difficult to do, is to construct a concrete example of a complicated system with the interactions and the behavior of the monetary authority that he alludes to, but in which results like Sims' will spuriously emerge in large samples.

exogeneity assumptions, as usually thought of. It is also symptomatic of measurement error, inappropriate aggregation over time, and inappropriate, asymmetrical seasonal adjustment. Each of these possible misspecifications ought to be tested for.