Observing Shocks

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1. The Rise of Shocks

Macroeconomists have observed business cycle fluctuations over time by constructing and manipulating models in which shocks have increasingly played a greater role. *Shock* is a relatively common English word, used by economists for a long time and to a large extent, much as other people use it to refer to external influences: small or large, frequent or infrequent, regularly transmissible or not. Over the past forty years or so, economists have broken ranks with ordinary language and both narrowed their preferred sense of *shock* and promoted it to a term of econometric art. A search of the economics journals archived in the JSTOR database shows that the use of the term *shock* has risen from about 3 percent of all articles in economics up to the 1960s to more than 23 percent in the first decade of the new millennium. If we restrict attention to macroeconomics, the proportion of articles that use *shock* rises to 44 percent. Year-by-year analysis of the 1960s and 1970s localizes the take-off point to 1973.

How can we account for the rise of the language of shocks? Our answer consists of a story about how the meaning of *shock* became

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sharpened and how shocks themselves became the objects of economic observation—both shocks as phenomena observed using economic theory to interpret data and shocks themselves as data that become the basis for observing phenomena that were not well articulated until shocks became observable.

What does it mean to be observable? We do not want to get sidetracked into the difficult issues in the philosophy of science, yet the philosophers James Bogen and James Woodward do provide a useful framework for discussing the developing epistemic status of shocks in (macro)economics that will enrich our historical account. They distinguish between *data* and *phenomena*:

> Data, which play the role of evidence for the existence of phenomena, for the most part can be straightforwardly observed. However, data typically cannot be predicted or systematically explained by theory. By contrast, well-developed scientific theories do predict and explain facts about phenomena. Phenomena are detected through the use of data, but in most cases are not observable in any interesting sense of that term. (Bogen and Woodward 1988, 305–6)

Cloud-chamber photographs are an example of data, which may provide evidence for the phenomena of weak neutral currents. Quantum mechanics predicts and explains weak neutral currents, but not cloud-chamber photographs.

We can see immediately that Bogen and Woodward's distinction is not unproblematic. Surely, individual price information gathered by the US Bureau of Labor Statistics is data, something identified independently of the observational apparatus. But is then the price index—a theoretically informed construction based on those data—a phenomenon, or is it, as macroeconomists typically treat it, itself data?[1] But, then, are those data "straightforwardly observed"? Does the quantity theory of money explain the phenomenon of the price level or the inflation rate, or are they the data it uses to explain the phenomenon of the proportionality of money and prices?

The ambiguity between data and phenomena, an ambiguity between the observable and the inferred, is recapitulated in the ambiguity in the

1. For further discussion on compromises made in data collection, see Stapleford, this volume; and Didier, this volume.

status of shocks, which shift from data to phenomena and back depending on the target of theoretical explanation. Our major goal is to explain how the changing epistemic status of shocks and the changing understanding of their observability account for the massive increase in their role in macroeconomics.

The roots of the rise of shocks can be found in two critical developments in the earliest days of modern macroeconomics and econometrics. The first is Ragnar Frisch's (1933) characterization of business cycle models as divided into impulse and propagation mechanisms: the shocks that perturbed the economic system away from equilibrium, and the structural properties of this system. The second is Trygve Haavelmo's (1944) division of econometric models into a deterministic component and a random component, which could be characterized by a well-behaved probability distribution. The random component could be seen as a mixture of "error" and Frisch's "impulses." The word *shock* frequently encompassed them both. Macroeconometric modeling used the terminology of shocks more frequently, characterizing them as phenomena (thus inferred and not observed): phenomena that were described only to be set aside as of secondary interest. Our story addresses the breakdown, beginning in the early 1970s, of the strategy of treating shocks as secondary phenomena.

2. Impulse and Error

Although the business cycle was a central target of economic analysis and observation in the early twentieth century, shocks first came to prominence in the late 1920s and early 1930s. Frisch's (1933) distinction between propagation and impulse problems became a key inspiration for later work on business cycles. Frisch represented the macroeconomy as a deterministic mathematical system of equations. *Propagation* referred to the time-series characteristics of this system, or "the structural properties of the swinging system" (171), which he characterized by a system of deterministic differential equations. Frisch argued that "a more or less regular fluctuation may be produced by a cause which operates irregularly" (171): he was principally interested in systems that displayed intrinsic cyclicality—that is, systems of differential equations with imaginary roots. He conjectured that damped cycles corresponded to economic reality.

Frisch drew on Knut Wicksell's (1907) metaphor of a rocking horse hit from time to time with a club: "The movement of the horse will be very different to that of the club" (quoted in Frisch 1933, 198). The role of the *impulse* is as "the source of energy" for the business cycle: an exterior shock (the club striking) pushes the system away from its steady state, and the size of the shock governs the "intensity" of the cycle (its amplitude), but the deterministic part (the rocking horse) determines the periodicity, length, and the tendency or not toward dampening of the cycle. Frisch referred to impulses as "shocks" and emphasized their erratic, irregular, and jerking character, which provide "the system the energy necessary to maintain the swings" (197).[2]

Frisch's own interest is principally in the propagation mechanism, and in 1933 he does not give a really distinct characterization of shocks. Frisch (1933, 198–99) credits Eugen Slutsky ([1927] 1937), among others, as a precursor in the "mathematical study [of] the mechanism by which . . . irregular fluctuations may be transformed into cycles." Where Frisch's focus was primarily on the deterministic component and not the shocks, Slutsky's was the other way round—focusing on the fact that cumulated shocks looked rather like business cycles without giving much explanation for the economic basis of the cumulation scheme or investigating the properties of its deterministic analogue.

Neither Frisch nor Slutsky was engaged in measuring the business cycle. The target of the analysis was not the impulse itself but the business cycle phenomenon. They sought to demonstrate in principle that, generically, systems of differential (or difference) equations subject to the stimulus of an otherwise unanalyzed and unidentified impulse would display behavior similar to business cycles. Shocks or other impulses were a source of "energy" driving the cycle, yet what was tracked were the measurable economic variables.

Shocks were not observed. But they could have been measured inferentially as the "'errors' in the rational behavior of individuals" (Frisch 1939, 639). A shock could then be defined as "any event which contradicts the assumptions of some pure economic theory and thus prevents the variables from following the exact course implied by that theory" (639). A practical implementation of that approach was available in the

2. Impulse is not a synonym for shock in Frisch's view. Impulses also include Schumpeterian innovations that are modeled as "ideas [that] accumulate in a more or less continuous fashion, but are put into practical application on a larger scale only during certain phases of the cycle" (Frisch 1933, 203). However, he leaves to further research the task of putting "the functioning of this whole instrument [i.e., innovations] into equations" (205).

form of the residual error terms from regression equations in structural equation models (640).[3] Frisch understood that the error terms of regression equations were not pure measures but a mixture of "stimuli" (the true impulse, the analogue of the club) and "aberrations" (Frisch [1938] 1948; see Qin and Gilbert 2001, 428–30).

Frisch's student Trygve Haavelmo (1940, 319) observed that the impulse component of error terms could be neglected in step-ahead conditional forecasts, as it was likely to be small. Over time, however, the impulses were critical to the ability of the model to generate cycles. Whereas measurement errors tend to cancel out when averaged, impulses tend to cumulate. Haavelmo (1940, esp. figs. 1 and 2) constructed a dynamic model that mimicked the business cycle in a manner similar to Frisch's (1933) simulation but which, unlike Frisch's model, contained no intrinsic cycle in the deterministic part—the cycle arising strictly from the cumulation of random impulses in the manner of Slutsky ([1927] 1937). Because of their essential role in generating cycles, Haavelmo (1940, 313–14) argued that the error terms must be regarded as a fundamental part of the explanatory model on par with the deterministic components and not merely as a measure of the failure of the model to match reality.

While Haavelmo and Frisch emphasized the causal role of shocks and the need to distinguish them from errors of measurement, their focus was not on the shocks themselves. Frisch's approach to statistics and estimation was skeptical of probability theory (see Louçã 2007, chap. 8; Hendry and Morgan 1995, 40–41). In contrast, Haavelmo's dissertation, "The Probability Approach in Econometrics" (1944), was a milestone in the history of econometrics (see Morgan 1990, chap. 8). Haavelmo argued that economic data could be conceived as governed by a probability distribution characterized by a deterministic, structural, dynamic element and an unexplained random element (cf. Haavelmo 1940, 312). His innovation was the idea that, if the dynamic element were sufficiently accurately described—a job that he assigned to a priori economic theory—the error term would conform to a tractable probability distribution. Shocks, rather than treated as unmeasured data, were now treated as phenomena. Theory focuses not on their individual values (data) but on their probability distributions (phenomena). Although shocks were now phenomena, they were

3. The idea of measuring an economically important, but otherwise unobservable, quantity as the residual after accounting for causes is an old one in economics—see Hoover and Dowell 2002. Frisch (1939, 639) attributes the idea of measuring the impulse to a business cycle as the deviation from rational behavior to François Divisia.

essentially *secondary* phenomena—characterized mainly to justify their being ignored.

While Frisch and Haavelmo were principally concerned with methodological issues, Jan Tinbergen was taking the first steps toward practical macroeconometrics with, for the time, relatively large-scale structural models of the Dutch and the US economies (see Morgan 1990, chap. 4). Tinbergen's practical concerns and Haavelmo's probabilistic approach were effectively wedded in the Cowles Commission's program, guided initially by Jacob Marschak and later by Tjalling Koopmans (1950; Hood and Koopmans 1953). Although residual errors in systems of equations were characterized as phenomena obeying a probability law, they were not the phenomena that interested the Cowles Commission. Haavelmo (1940, 320–21; 1944, 54–55) had stressed the need to decompose data into "explained" structure and "unexplained" error to get the structure right, and he had pointed out the risk of getting it wrong if the standard of judgment were merely the *ex post* ability of an equation to mimic the time path of an observed variable. Taking that lesson on board, the Cowles Commission emphasized the conditions under which a priori knowledge would justify the *identification* of underlying economic structures from the data. Their focus was then on estimating the parameters of the structural model and on the information needed to lend credibility to the claim that they captured the true structure.

Shocks, as quantities of independent interest, were shunted aside. Though Haavelmo had added some precision to one concept of shocks, various meanings continued in common usage, even among econometricians. Tinbergen (1939, 193), for instance, referred to *exogenous shocks* "amongst which certain measures of policy are to be counted." The focus of macroeconometric modeling in the hands not only of Tinbergen but of Lawrence Klein and others from the 1940s through the 1960s was not on shocks but on estimating the structural parameters of the deterministic components of the models. Models were generally evaluated through their ability to track endogenous variables conditional on exogenous variables (cf. Klein and Burmeister 1976). Consistent with such a standard of assessment, the practical goal of macroeconometric modeling was counterfactual analysis in which the models provided forecasts of the paths of variables of interest conditional on exogenous policy actions. Having relegated shocks to the status of secondary phenomena, economists largely forgot about shocks as the causal drivers of business cycle phenomena.

But not completely. In a 1959 article, Irma and Frank Adelman simulated the Klein-Goldberger macroeconomic model of the United States to determine whether it could generate business cycle phenomena. They first showed that the deterministic part of the model would not generate cycles. They then showed that by drawing artificial errors from random distributions that matched those of the estimated error processes, the models did generate series that looked like business cycles identified according to the techniques of the National Bureau of Economic Research (a Turing test; see Boumans 2005, 93). While the Adelmans' test returned shocks to a central causal role in the business cycle, they focused not on individual shocks but instead on their probability distribution.

3. The New Classical Macroeconomics and the Rediscovery of Shocks

Although shock continues to be used with a wide range of meanings, after 1973 the idea of shocks as pure transients or random impulses conforming to a probability distribution or the same random impulses conforming to a time-series model independent of any further economic explanation became dominant. Why? Our thesis is that it was, first, the inexorable result of the rise of the new classical macroeconomics and one of its key features, the rational expectations hypothesis, originally due to John Muth (1961) but most notably promoted in the early macroeconomic work of Robert Lucas (e.g., Lucas 1972) and Thomas Sargent (1972); and second, that it was promoted by the increased role of shocks as a result of Christopher Sims's vector autoregression (VAR) econometric approach.

While rational expectations has been given various glosses (e.g., people use all the information available or people know the true model of the economy), the most relevant one is probably Muth's (1961, 315, 316) original statement: "[Rational] expectations . . . are essentially the same as the predictions of the relevant economic theory." Rational expectations on this view are essentially equilibrium or consistent expectations. A standard formulation of rational price expectations (e.g., in Hoover 1988, 187) is that $p_t^e = E(p_t|\mathbf{\Omega}_{t-1})$, where p is the price level, $\mathbf{\Omega}$ is *all* the information available in the model, t indicates the time period, e indicates an expectation, and E is the mathematical conditional expectations operator. The expected p_t^e can differ from the actual price p_t, but only by a mean-zero, independent, serially uncorrelated random error. The feature that makes the expectation an equilibrium value analogous to a market clearing price is that the content of the information set $\mathbf{\Omega}_{t-1}$ includes the model itself, so that an expected price would not be consistent with the information if it differed from the best conditional forecast using the structure of the model, as well as the values of any exogenous variables known at time $t-1$.

The mathematical expectations operator reminds us that "to discuss rational expectations formation at all, some explicit stochastic description is clearly required" (Lucas 1973, 328–29 n. 5). Yet the need for a regular, stochastic characterization of the impulses to the economy places a premium on shocks with straightforward time-series representations, and this meaning of shock increasingly became the dominant one. The same pressure that led to the characterization of shocks as the products of regular, stochastic processes also suggested that government policy be characterized similarly—that is, by a policy rule with possibly random deviations. The economic, behavioral rationale was first, that policymakers, like other agents in the economy, do not take arbitrary actions but systematically pursue goals, and second, that other agents in the economy anticipate the actions of policymakers.

Sargent (1982, 383) relates the analysis of policy as rules under rational expectations to general equilibrium: "Since in general one agent's decision rule is another agent's constraint, a logical force is established toward the analysis of dynamic general equilibrium systems." Of course, this is a model-relative notion of general equilibrium (i.e., it is general only to the degree that the range of the conditioning of the expectations operator, $E(\cdot|\cdot)$, is unrestricted relative to the information set, Ω_{t-1}). Lucas took matters a step further in taking the new technology as an opportunity to integrate macroeconomics with a version of the more expansive Arrow-Debreu general equilibrium model. He noticed the equivalence between the intertemporal version of that model with contingent claims and one with rational expectations. In the version with rational expectations, it was relatively straightforward to characterize the shocks in a manner that reflected imperfect information—in contrast to the usual perfect-information framework of the Arrow-Debreu model—and generated more typically macroeconomic outcomes. Shocks were a centerpiece of his strategy:

> viewing a commodity as a function of stochastically determined shocks . . . in situations in which information differs in various ways among traders . . . permits one to use economic theory to make precise what one means by information, and to determine how it is valued economically. (Lucas 1980, 707)

His shock-oriented approach to general equilibrium models of business cycles was increasingly applied to different areas of macroeconomics.

Rational expectations, the focus on market-clearing, general equilibrium models, and the characterization of government policy as the execution of stable rules came together in Lucas's (1976) famous policy noninvariance argument (the "Lucas critique"): if macroeconometric models characterize the time-series behavior of variables without explicitly accounting for the underlying decision problems of the individual agents who make up the economy, then when the situations in which those agents find themselves change, their optimal decisions will change, as will the time-series behavior of the aggregate variables. The general lesson was that a macroeconometric model fit to aggregate data would not remain stable in the face of a shift in the policy rule and could not, therefore, be used to evaluate policy counterfactually.

In one sense, Lucas merely recapitulated and emphasized a worry that Haavelmo (1940) had already raised—namely, that a time-series characterization of macroeconomic behavior need not map onto a structural interpretation. But Haavelmo's (1944, chap. 2, sec. 8) notion of structure was more relativized than the one that Lucas appeared to advocate. Lucas (1980, esp. 702, 707) declared himself the enemy of "free parameters" and took the goal to be to articulate a complete general equilibrium model grounded in parameters governing "tastes and technology" and in exogenous stochastic shocks. Lucas's concept of structure leads naturally to the notion that what macroeconometrics requires is microfoundations—a grounding of macroeconomic relationships in microeconomic decision problems of individual agents (see Hoover 2012). The argument for microfoundations was barely articulated before Lucas (1980, 711) confronts its impracticality—analyzing the supposedly individual decision problems not in detail but through the instrument of "'representative' households and firms."

The Lucas critique stood at a crossroads in the history of empirical macroeconomics. Each macroeconometric methodology after the mid-1970s has been forced to confront the central issue that it raises. Within the new classical camp, there were essentially two initial responses to the Lucas critique—each in some measure recapitulating approaches from the 1930s through the 1950s.

Lars Hansen and Sargent's (1980) work on maximum-likelihood estimation of rational expectations models and subsequently Hansen's work on generalized method-of-moments estimators initiated (and exemplified) the first response (Hansen 1982; Hansen and Singleton 1982). Hansen and Sargent attempted to maintain the basic framework of the Cowles Commission's program of econometric identification (inspired by Haavelmo 1944) in which theory provided the deterministic structure that allowed the error to be characterized by manageable probability distributions and thus set aside. The target of explanation remained—as it had been for Frisch, Tinbergen, Klein, and the large-scale macroeconometric modelers—the conditional paths of aggregate variables. The structure was assumed to be known a priori, and measurement was directed to estimating parameters, now assumed to be "deep"—at least relative to the underlying representative agent model.

Finn Kydland and Edward Prescott, starting with their seminal real business cycle model in 1982, responded with a radical alternative to Hansen and Sargent. Instead of embracing the soundness of Haavelmo's division of labor between economic and statistical theories, they rejected it (see Kydland and Prescott 1990, 1991, esp. 164–67; Prescott 1986; Hoover 1995, 28–32).

Though neither Haavelmo nor his followers in the Cowles Commission clearly articulated either the fundamental nature of the a priori economic theory that was invoked to do so much work in supporting econometric identification or the ultimate sources of its credibility, Haavelmo's decomposition became the centerpiece of econometrics (being an unassailable dogma in some quarters).

Kydland and Prescott took the message from the Lucas critique that a workable model must be grounded in microeconomic optimization (or in as near to it as the representative agent model would allow). And they accepted Lucas's call for a macroeconomic theory based in general equilibrium with rational expectations. Though they held these theoretical presuppositions dogmatically—propositions that were stronger and more clearly articulated than any account of theory offered by Haavelmo or the Cowles Commission—they also held that models were at best workable approximations and not detailed, "realistic" recapitulations of the world. Thus they rejected the Cowles Commission's notion that the economy could be so finely recapitulated in a model that the errors could conform to a tractable probability law and that its true parameters could be the objects of observation or direct measurement.

Having rejected Haavelmo's "probability approach," Kydland and Prescott embraced Lucas's conception of models as simulacra:

> A "theory" is . . . an explicit set of instructions for building a parallel or analogue system—a mechanical, imitation economy. A "good" model, from this point of view, will not be exactly more "real" than a poor one, but will provide better imitations.
>
> Our task . . . is to write a FORTRAN program that will accept specific economic policy rules as "input" and will generate as "output" statistics describing the operating characteristics of time series we care about, which are predicted to result from these policies. (Lucas 1980, 697, 709–10)

On Lucas's view, a model needed to be realistic only to the degree that it captured some set of key elements of the problem to be analyzed and successfully mimicked economic behavior on those limited dimensions. Given the preference for general equilibrium models with few free parameters, shocks in Lucas's (1980, 697) framework became the essential driver and the basis on which models could be assessed: "We need to test [models] as useful imitations of reality by subjecting them to shocks for which we are fairly certain how actual economies, or parts of economies, would react."

Kydland and Prescott, starting with their first real business cycle model (1982), adopted Lucas's framework. Real (technology) shocks were treated as the main driver of their model, and its ability to mimic business cycle phenomena when shocked became the principal criterion for the empirical success (Prescott 1986; Kydland and Prescott 1990, 1991; and Kehoe and Prescott 1995). Shocks in Lucas's and Kydland and Prescott's framework assumed a new and now central crucial task: they became the instrument through which the real business cycle modeler would select the appropriate artificial economy to assess policy prescriptions. For this, it is necessary to identify correctly substantive shocks—that is, the ones the effect of which on the actual economy could be mapped with some degree of confidence. Kydland and Prescott's translation of Lucas's conception of modeling into the real business cycle model generated a large literature.

Both Kydland and Prescott's earliest business cycle models as well as their successors, the so-called dynamic stochastic general equilibrium (DSGE) models, were developed explicitly within Lucas's conceptual framework, though they subsequently were adopted by economists with quite different methodological orientations. Kydland and Prescott (1982) presented a tightly specified, representative-agent, general equilibrium model in which the parameters were calibrated. They rejected statistical estimation because it penalized models for not matching reality on dimensions that in fact were unrelated to "the operating characteristics of time series we care about." *Calibration* involves drawing parameter values from general economic considerations: both long-run unconditional moments of the data and facts about national-income accounting, as well as evidence from independent sources, such as microeconomic studies (Kydland and Prescott 1996, 74).

To evaluate their model, Kydland and Prescott (1982) adopt the "test of the Adelmans": would a business cycle analyst be unable to distinguish the artificial output of a model from the data on the actual economy (Kydland and Prescott 1990, 6; see also Lucas 1977, 219, 234)?[4] Kydland and Prescott's main criterion is how well the unconditional second moments of the simulated data matched the same moments in the real-world data. To generate the simulation, they simply drew shocks from a probability distribution whose parameters were chosen to ensure that the variance of output produced in the model matched exactly the corresponding value for the actual US economy (Kydland and Prescott 1982, 1362). This, of course, was a violation of Lucas's maxim: do not rely on free parameters. Given that shocks were not, like other variables, supplied in government statistics, their solution in later work was to take the "Solow residual" as the measure of technology shocks. In effect, they used the production function as an instrument to measure technology shocks (Prescott 1986, 14–16).

Kydland and Prescott treated the technology shocks measured by the Solow residual as data in Bogen and Woodward's sense. As with price indices, certain theoretical commitments were involved. Prescott (1986, 16–17) discussed various ways in which the Solow residual may fail to measure true technology shocks accurately, but concluded that, for the purpose at hand, that they would serve adequately. The key point at this stage is that—in keeping with Bogen and Woodward's distinction—Kydland and Prescott were not interested in the shocks per se but in what might be termed "the technology-shock phenomenon." The Solow residual is serially correlated. Prescott (1986, 7n5) treated it as governed by a time-series process. He claimed that very similar simulations and measures of business cycle phenomena (i.e., of the cross-correlations of current GDP with various variables at different lags) would result whether the

4. The phrase "test of the Adelmans'" was coined by King and Plosser (1994) and refers to Adelman and Adelman 1959.

shocks were modeled as nonstationary or as stationary but highly persistent (see also Kydland and Prescott 1990).

Kydland and Prescott's analysis was based not on direct observation of technology shocks (i.e., on the Solow residual) but on the statistical characterization of those shocks (the technology-shock phenomenon). The earlier simulation studies of Adelman and Adelman (1959) had been concerned not with the shocks but with the time paths of variables: the shock phenomenon was thus *secondary*. But for Kydland and Prescott, who focused on the covariation of the variables rather than their time paths, technology-shock phenomenon was *primary*.

In contrast with the Adelmans, whose measures of shocks depended on the whole structure of the model, Kydland and Prescott's technology shocks were measured by just one element of the model, the Cobb-Douglas production function. Measured this way, technology shocks on Kydland and Prescott's view have a degree of model-independence and an integrity that allows them to be transferred between modeling contexts.

Although real business cycle modelers typically use technology shocks to characterize the shock process, the technology-shock phenomenon, they have from time to time treated them as direct inputs into their models (essentially as observed data). Hansen and Prescott (1993) fed technology shocks directly into a real business cycle model to simulate the time path of US GDP over the 1990–91 recession.[5]

4. The Identification of Shocks

Whereas Kydland and Prescott had attacked Haavelmo's and the Cowles Commission's assumption that models define a tractable probability distribution, Christopher Sims (1980, 1, 2, 14, 33) attacked the credibility of the a priori assumptions that they used to identify the models. Nonetheless, it is the positive contribution of Sims's approach that bears most strongly on our story. Sims asks—to quote the title of Sargent and Sims's (1977) earlier paper—what can be learned about business cycles "without pretending to have too much *a priori* economic theory"?

5. The major reason for the focus of the real business cycle (RBC) literature on comparing unconditional moments is the way it characterized cycles as recurrent fluctuations in economic activity, going back to Burns and Mitchell 1946 through Lucas's equilibrium approach (Cooley and Prescott 1995, 26; Kydland and Prescott 1982, 1359–60). Robert King, Charles Plosser, and Sergio Rebelo (1988) provide an early example of a calibrated RBC model looking at time paths, while Christiano (1988) develops an estimated RBC model that compares theoretical and observed time paths.

Sims (1980) took general equilibrium, in one sense, more seriously than did the Cowles Commission in that he treated all the independently measured economic variables as endogenous. Although, as with Haavelmo, Sims divided the model into a deterministic and an indeterministic part, he rejected the notion that the deterministic part was structural. He regarded his system of equations—the *vector autoregression* (VAR) model—as a reduced form in which the random residuals were now the *only* drivers of the dynamics of the model and hence considerably more important than they had been in the Cowles Commission's approach. Sims referred to these residuals as "innovations," which stressed the fact that they were independent random shocks without their own time-series dynamics. Since the deterministic part of the model was not structural, all time-series behavior could be impounded there, so the shocks are now pure transients.

Sims used his VAR model to characterize dynamic phenomena through variance decomposition analysis and impulse-response functions. Variance decomposition is an accounting exercise that determines the proportion of the variability of each variable that is ultimately attributable to the exogenous shocks to each variable. The impulse-response function traces the effect on the time series for a variable from a known shock to itself or to another variable. Particular shocks need not be measured or observed in order to conduct either of these exercises; nonetheless, they must be characterized. The dynamics of the data must be cleanly divided between the deterministic part and the independent random shocks. The difficulty, however, is that, in general, there is no reason that the residuals to an estimated VAR ought to have the characteristic of independent random shocks—in particular, they will generally be correlated among themselves.

To deal with the problem of intercorrelated residuals, Sims assumed that the variables in his VAR could be ordered recursively (e.g., in a *Wold causal chain* via Cholesky decompositions), in which a shock to a given variable immediately affects that same variable and all those lower in the system. The coefficients on the contemporaneous variables are selected so that the shocks are orthogonal to each other.

Sims (1980, 2) admitted that individual equations of the model are not structural, and he suggested that "nobody is disturbed by this situation of multiple possible normalizations." In fact, given N variables, there are $N!$ possible normalizations (e.g., for $N = 6$, there are 720 normalizations). And far from nobody being disturbed, critics immediately pointed out that first, the variance decompositions and the impulse-response functions were, in general, not robust to the choice of normalization, and second, policy analysis required not just one of the possible renormalizations but the right one. Sims (1982, 1986) rapidly conceded the point. The VAR approach did not eliminate the need for identifying assumptions. Yet Sims had nevertheless changed the game.

The Cowles Commission had sought to measure the values of structural parameters by imposing identifying assumptions strong enough to recover them all. Sims had shown that, if the focus of attention was on identifying the shocks themselves, then the necessary identifying assumptions were weaker: with a *structural VAR* (SVAR)—that is, a VAR with orthogonalized shocks—one needs to know only the recursive order or, more generally, the causal structure of the *contemporaneous* variables. The parameters of the lagged variables in the dynamic system need not be structural, so that the SVAR is a quasi-reduced form, and less is taken on faith than in the Cowles Commission's or calibrationist frameworks.

The SVAR put shocks front and center, not because shocks could not have been identified in the Cowles Commission's framework or because shocks are automatically interesting in themselves but because their time-series properties are essential to the identification strategy. Variance-decomposition exercises and impulse-response functions do not necessarily consider measured shocks, but rather ask a simple counterfactual question, "What would be the effect of a generic shock u of size v to variable x on variables x, y, and z?" The situation is essentially no different than that of technology shocks measured using the Solow residual. The SVAR, like a production function used to measure technology shocks, can be used as a measuring instrument to observe shocks to each variable in the VAR system. Just as the real business cycle modeler may be more interested in the generic business cycle phenomena, so the SVAR modeler may be more interested in generic dynamic phenomena. But equally the SVAR modeler can use the particular observed shocks to the whole system of equations to generate specific historical time paths for variables or to conduct counterfactual experiments (e.g., Sims 1999).

There are, however, key differences with the calibrationist approach. Calibrationists make very strong identifying assumptions with respect to structure. Essentially, they claim to know not only the mathematical form of the economic relationships but their parameterization as well. The cost is that they give up on the notion that residuals will conform to tractable probability distributions. In contrast, the SVAR modeler makes minimal structural assumptions and specifies nothing about the values of the parameters other than that they must deliver orthogonal shocks. Whereas typical real business cycles are driven by technology shocks only, SVAR models necessarily observe shocks for each variable in the system.

5. Coming Full Circle: Estimation by Impulse-Response Matching

Although starting from very different critical stances, both Sims's SVAR approach and the new classicals' calibrationist approach elevated shocks to a starring role. Shocks had become the targets of measurement; models or parts of models had become the measuring instruments. In short, shocks were observable data in Bogen and Woodward's sense. Still, economists were frequently more interested in the phenomena that shocks generated—how the economy reacted generically to a particular type of shock—rather than in the particular shock to a particular variable on a particular date (although sometimes they were interested in shocks as data). Yet the observability of shocks was sine qua non of identifying these phenomena in the first place.

Whether because of the similarity in their views of shocks or, perhaps, for the more mundane sociological reason that economists, like other scientists, cannot resist trying to make sense of each other's work and often seek out common ground, the 1990s witnessed a rapprochement between the DSGE and SVAR programs. Any DSGE model has a reduced-form representation, which can be seen as a special case of a more general VAR, and it also has a contemporaneous causal ordering of its variables that provides a basis for converting the VAR into an SVAR. A calibrated or estimated DSGE model, therefore, can generate variance decompositions and impulse-response functions, which may, in their turns, be compared directly with their counterparts generated from estimated SVARs in which DSGE models are nested. Such comparisons are methodologically equivalent to Kydland and Prescott's strategy of attempting to match the second moments of calibrated models to the equivalent statistics for actual data; they just use different target phenomena.

By the early 1990s the terms of the debate in macroeconomics had shifted from one between monetarists, such as Milton Friedman, and old Keynesians in the macroeconometric tradition, such as James Tobin and Lawrence Klein, or one between the old Keynesians and the new classicals, to one between the new Keynesians and the new classicals (Hoover 1988, 1992). The new Keynesians essentially adopted the technical paradigms of the new classicals, typically including the rational expectations hypothesis, but rejected the notion of perfect competition with continuous market clearing as a sound basis for macroeconomic models, which opened the door for activist policies to improve welfare. Sims (1989, 1992) regarded the debate between the new classicals—especially, the real business cycle modelers—and the new Keynesians as having reached an impasse. In his view, real business cycle modelers assessed their models with an impoverished information set (unconditional moments). Sims (1992, 980) argued that the debate between the monetarists and the old Keynesians had reached a similar impasse, which a focus on time-series information (mainly responses to innovations and Granger causality) had helped resolve by establishing that monetary policy has substantial effects on real output. Analogously, Sims (1992, 980) suggested that real business cycle modelers should consider the richer set of time-series information. He urged them to confront their models with "the documented impulse response facts about interactions of monetary and real variables" (980).

Sims wanted to reestablish the relevance of estimation methods in an area of research that had become dominated by calibration techniques, and he sought common ground in what amounted to adopting Lucas's views on modeling: to select a substantive shock and compare models by the implied dynamic responses to it; a good model is one in which the impulse-response function of the model matches the impulse-response function of the data, as determined through the instrumentality of the SVAR (see also Christiano 1988 and Singleton 1988). Once again, shocks (via impulse-response functions) were data used to characterize phenomena, and models were judged by their ability to reproduce those phenomena.

Sims's proposal must be distinguished from merely matching historical performance in the manner of Hansen and Prescott (1993). The interactions of the different elements are too complex to connect, for example, policy actions to particular outcomes (Leeper, Sims, and Zha 1996, 2). Lawrence Christiano, Martin Eichenbaum, and Charles Evans (1999, 68) argued that the comovements among aggregate variables cannot be interpreted as evidence for or against the neutrality of money, since a "given policy action and the economic events that follow it reflect the effects of *all* the shocks to the economy." Sims's proposal, following Lucas, amounted to a highly restricted counterfactual experiment in which the effects of an isolated shock can be traced out in the economy (i.e., in the SVAR) and compared with the analogous effects in a model. The goal was precisely analogous to experimental controls in a laboratory in which the effect of a single modification is sought against a stable background.

Much of the research in this vein focused on monetary shocks—that is, shocks to short-term interest rates. The short-term interest rate was regarded as the central bank's policy instrument and assumed in the theoretical models to be governed by a policy rule—the central bank's *reaction function* (usually a "Taylor rule"). Monetary policy was, of course, an intrinsically interesting and important area of research. It also held out the promise of clearer discrimination among theoretical models "because different models respond very differently to monetary policy shocks" (Christiano, Eichenbaum, and Evans 1999, 67).

A case that well illustrates Sims's strategy is the so-called price puzzle (see Eichenbaum's 1992 comments on Sims 1992). Simple textbook models suggest that tighter monetary policy should reduce the rate of inflation and the price level. One might expect, therefore, that an exogenous positive shock to the short-term interest rate would result in a declining impulse-response function for prices. In fact, Sims and most subsequent researchers found that the impulse-response function for prices in an SVAR tends to rise for some time before falling. The quest for a theoretical model that accounts for this robust pattern has generated a large literature (see Demiralp, Hoover, and Perez 2010).

Sims's (1992) call for macroeconomists to focus on time-series evidence was taken into consideration subsequently. Whereas in his 1992 article he reported several point-estimate impulse-response functions obtained from alternative VARs for data from different countries, Eric Leeper, Sims, and Tao Zha (1996) focused on the US data and used sophisticated VAR methods to characterize features of aggregate time-series data. Here, in contrast to Sims (1992), the authors present confidence intervals for the estimated impulse-response functions (cf. Christiano, Eichenbaum, and Evans 1996, 1999).

Parallel to characterizing dynamic responses to shocks in the data through VARs, there was the effort to build artificial economies, small-scale dynamic general equilibrium monetary models, to explain the business cycle phenomena and to derive policy implications of them. Sims himself joined this enterprise with Leeper (Leeper and Sims 1994; see also Christiano and Eichenbaum 1995, Yun 1996, and Christiano, Eichenbaum, and Evans 1997). Here the parameters either were estimated with methods such as maximum likelihood or general methods of moments, or were calibrated. Once the parameters were assigned numerical values, one can derive the theoretical impulse-response functions to a monetary shock. However, the closeness of the match between the model-based and the SVAR-based impulse-response functions is usually judged in a rough-and-ready fashion—the same ocular standard applied in matching unconditional moments in the real business cycle literature.

Rotemberg and Woodford 1997 and the literature that derived from this work took impulse-response matching one step farther. Setting aside some of the fine details, the essence of Rotemberg and Woodford's approach was to select the parameterization of the theoretical model to mimimize the distance between the impulse-response functions of the model and those of the SVAR, which became a standard approach in DSGE macroeconomics (only parameters that were identifiable were estimated, the others were calibrated). But their model failed to deliver the slow responses ("inertia") observed in impulse-response functions generated from SVARs. Other economists took on the task of building DSGE models and estimating them by impulse-response matching that captured the inertia of the impulse-response functions (Christiano, Eichenbaum, and Evans 2005; Smets and Wouters 2007; see Duarte 2011).

Rotemberg and Woodford's method, in effect, treated the impulse-response functions of the SVAR as data in their own right—data that could be used as an input to the estimator. Where previously the shock could be regarded as data and the impulse-response functions as phenomena, the shocks were now moved down a level. They stood in the same relationship to those functions as the raw prices of individual goods did to the price index. And the focus of the technique shifted from the isolation of shocks and mimicking of dynamic phenomena back, as it had in the post–Cowles Commission macroeconometric program, to the measurement of structural parameters.

6. Shocks, Macroeconometrics, and Observability

We have explored the question of how economists observe the business cycle phenomena by treating shocks sometimes as data and other times as phenomena. We have thus addressed three main questions in this essay. Two were explicit: What is the relationship of shocks to observation? Why did the uses of the language of shocks explode after the early 1970s? And one question was only implicit: What lessons does the history of shocks provide to philosophers of science or economic methodologists? The answers to these three questions are deeply entangled in our narrative.

In the earliest days of modern econometrics in the 1930s, estimated equations were conceived of as having unobservable error terms. Yet these systems of equations, which had their own deterministic dynamics, were also thought of as being perturbed by actual disturbances, so that the error terms were—to use Frisch's terminology—a mixture of stimuli and aberration. Business cycle theory was principally interested in the stimuli. Business cycle theory gave way after World War II to a theory of macroeconomic policy that aimed to avoid cycles in the first place. Attention thus shifted to the deterministic parts of structural models and, notwithstanding Haavelmo's characterization of shocks as well-behaved phenomena with a regular probabilistic structure, shocks became of secondary interest.

Shocks returned to center stage only when the introduction of the rational expectations hypothesis compelled economists to treat the stochastic specification of a model as a fundamental element rather than as a largely ignorable supplement, and economists began to notice that models could be treated as measuring instruments through which shocks became observable. Rational expectations compel at least a relative-to-modeled-information general equilibrium approach to modeling. Thoroughly done, such an approach—whether theoretically, as in a real business cycle model, or econometrically, as in an SVAR—endogenizes every variable except the shocks. Shocks are then elevated to be the sole drivers of economic dynamics, and their observability, if not their particular values, becomes the sine qua non of a properly specified model. It is, therefore, hardly surprising that a vast rise in the usage of shock occurs after 1973, since shocks are central to a fundamental reconceptualization of macroeconomic theory that, to be sure, began with Frisch forty years earlier, but did not sweep the boards until the rise of the new classical macroeconomics.

We have used Bogen and Woodward's distinction between observable data and inferred phenomena to provide an organizing framework for our discussion. Although it may prove useful as a rough-and-ready contrast, it appears not to draw a bedrock distinction: at some points shocks could be best regarded as phenomena, inferred from observable data, and at other points as data observed using models as measuring instruments, or as the raw material from which data were constructed and which were then used as an input to generate further phenomena or as the basis for higher-order inference. Economics, even in its deepest reaches, is about relationships. What the history of shocks shows is that when we give up the rather tenuous grounding of observability in human senses, then the distinctions between observable and inferrable and between data and phenomena are, at best, relative ones that depend on our principal interests and our targets of explanation, on our presuppositions, explicitly theoretical or merely implicit, and on the modeling tools we have at our disposal—which emphasizes the role of models as measuring instrument (Boumans 2005, esp. 16–17) that integrate a range of ingredients coming from disparate sources, and as autonomous agents that mediate theories and the real world (Morgan and Morrison 1999). Philosophers of science would do well to consider such cases.

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