OBSERVING SHOCKS

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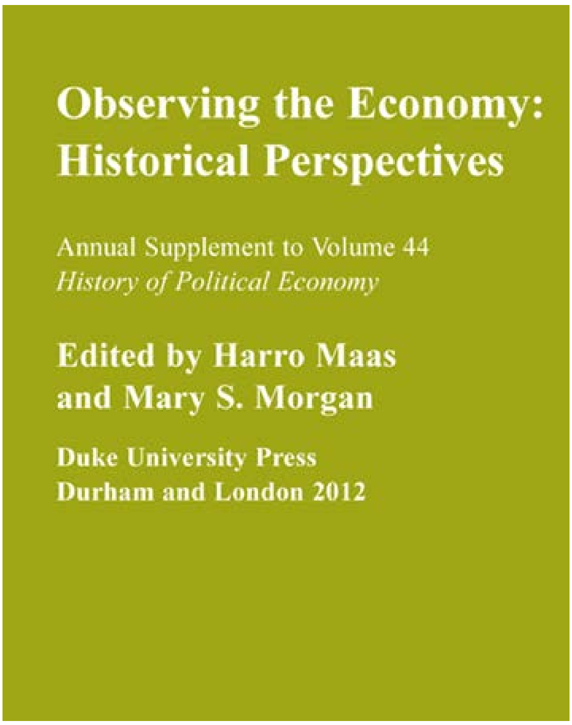

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Abstract

of

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Shock is a term of art that pervades modern economics appearing in nearly a quarter of all journal articles in economics and in nearly half in macroeconomics. Surprisingly, its rise as an essential element in the vocabulary of economists can be dated only to the early 1970s. The paper traces the history of shocks in macroeconomics from Frisch and Slutzky in the 1920s and 1930s through real-business-cycle and DSGE models and to the use of shocks as generators of impulse-response functions, which are in turn used as data in matching estimators. The history is organized around the observability of shocks. As well as documenting a critical conceptual development in economics, the history of shocks provides a case study that illustrates, but also suggests the limitations of, the distinction drawn by the philosophers of science James Bogen and James Woodward between data and phenomena. The history of shocks shows that this distinction must be substantially relativized if it is to be at all plausible.

Keywords: shock, new classical macroeconomics, real-business-cycle model, Ragnar Frisch, Jan Tinbergen, Robert Lucas, data, phenomena, James Bogen, James Woodward, DSGE model, impulse-response function

JEL Codes: B22, B23, B41

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Observing Shocks

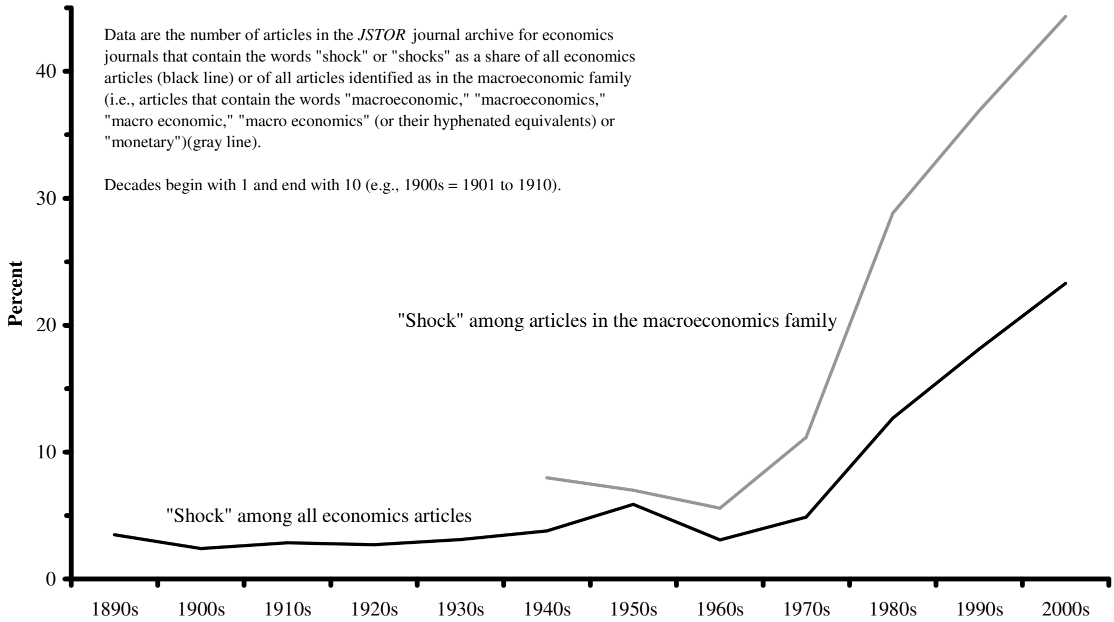
I. The Rise of Shocks

Shock is a relatively common English word – used by economists for a long time and to a large extent much as other people used it. 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. The black line in Figure 1 shows the fraction of all articles using the term "shock" (or "shocks") as a percentage of all articles in the economics journals archived in the *JSTOR* database from the last decade of the 19th century to the present. The striking feature of the figure is that the use of "shock" varies between 2½ percent and 5 percent up to the 1960s, and then accelerates steeply, so that by the first decade of the new millennium "shock" appears in more than 23 percent of all articles in economics. Year-by-year analysis of the 1960s and 1970s localizes the take-off point to 1973. The gray line, which presents the share of all articles that mention a family of terms identifying macroeconomic articles and also "shock" or "shocks," is even more striking.[1] It lies somewhat above the black line until the 1960s. It takes off at the same point but at a faster rate, so that, by the first decade of the millennium, "shock" appears in more than 44 percent of macroeconomics articles.

Since the 1970s the macroeconomic debate has been centered to some extent on shocks: the divide between real business cycle theorists and new Keynesian macroeconomists evolved around the importance of real versus nominal shocks for business cycle fluctuations. More important, shocks became a central element in observing the macroeconomic phenomena. Then, one question to be addressed in this

[1] The macroeconomics family includes "macroeconomic," "macroeconomics," "macro economic," "macro economics," and "monetary." Because the search tool in *JSTOR* ignores hyphens, this catches the common variant spellings, including hyphenated spellings.

Figure 1
Usage for "Shock"



paper is, how can we account for that terminological transformation? Our answer consists of a story about how the meaning of "shock" became sharpened and how shocks themselves became the objects of economic observation – both shocks as phenomena that are observed using economic theory to interpret data and shocks themselves data that become the basis for observing phenomena, which were not well articulated until shocks became observable. Here we are particularly interested in the debates carried out in the macroeconomic literature of the business cycle.

Among economists "shock" has long been used in a variety of ways. The earliest example in *JSTOR* underscores a notion of frequent but irregular blows to the economy: "an unending succession of slight shocks of earthquake to the terrestrial structure of business, varying of course in practical effect in different places and times …" (Horton 1886, 47). Near the same time, we also find the idea that shocks have some sort of propagation mechanism:

> Different industrial classes have in very different degrees the quality of economic elasticity; that is, the power of reacting upon and transmitting the various forms of economic shock and pressure. [Giddings (1887), 371]

Francis Walker (1887, 279) refers to "shocks to credit, disturbances of production, and fluctuations of prices" as a cause of suffering, especially among the working classes. Frank Taussig (1892, 83) worries about a "shock to confidence" as a causal factor in the price mechanism. Charles W. Mixter (1902, 411) considers the transmission of the "shock of invention" to wages. Among these early economists, the metaphor of shocks may refer to something small or large, frequent or infrequent, regularly transmissible or not. And while these varieties of usages continue to the present day, increasingly shocks are regarded as transient features of economic time series subject to well-defined probability distributions, transmissible through regular deterministic processes. Over time, shocks have come to be regarded as the objects of economic analysis and, we suggest, as *observable*.

What does it mean to be observable? The answer is often merely implicit – not only among economists, but among other scientists. Critics of scientific realism typically take middle-sized entities (for example, tables and chairs) as unproblematically observable. They object to the claims of scientific realists for the existence of very tiny entities (e.g., an electron) or very large entities (e.g., the dark matter of the universe) in part on the grounds that they are not observable, taking them instead to be theoretical constructions that may or may not really exist. Realist philosophers of science also accept everyday observation as unproblematic, but may respond to the critics by arguing that instruments such as microscopes, telescopes, and cloud chambers are extensions of our ordinary observational apparatus and that their targets are, in fact, directly observed (and are not artifacts of these apparatuses). The critics point out that substantial theoretical commitments are involved in "seeing" inaccessible entities with such instruments, and what we see would change if we rethought those commitments – hardly the mark of something *real* in the sense of independent of ourselves and our own thinking.

In a contribution to this debate that will form a useful foil in our historical account, the philosophers James Bogen and James Woodward argue that we ought to draw a distinction 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), 305-306].

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.

Qualifiers such as "typically," "in most cases," and "in any interesting sense" leave Bogen and Woodward with considerable wiggle room. But we are not engaged in a philosophical investigation *per se*. Their distinction between data and phenomena provides us with a useful framework for discussing the developing epistemic status of shocks in (macro)economics.

We can see immediately that economics may challenge the basic distinction at a fairly low level. The U.S. Bureau of Labor Statistics collects information on the prices of various goods in order to construct the consumer price index (CPI) and the producer price index (PPI).[2] Although various decisions have to be made about how to collect the information – for example, what counts as the same good from survey to survey or how to construct the sampling, issues that may be informed by theoretical considerations – it is fair to consider the root information to be data in Bogen and Woodward's sense. These data are transformed into the price indices. The construction of index numbers for prices has been the target of considerable theoretical discussion – some purely statistical, some drawing on economics explicitly. Are the price indices, then, phenomena – not observed, but explained by theory? The theory used in their construction is not of an explanatory or predictive type; rather it is a theory of measurement – a theory of how to organize information in a manner that could be the target of an explanatory theory or that may be used for some other purpose. We might, then, wish to regard – as economists almost always do – the price indices as data. But can we say that such data are "straightforwardly observed"?

The raw information from which the price indices are constructed also fits Bogen and Woodward's notion that the object of theory is not to explain the data. For example, the quantity theory of money aims to explain the price level or the rate of inflation, but not the individual prices of gasoline or oatmeal that form part of the raw material from which the price level is fabricated. While that suggests that the price level is a phenomenon, here again we might question whether the object of explanation is truly the price level (say a specific value for the CPI at a specific time) or whether it is rather the fact of the proportionality of money and prices, so that the observations of the price level are data, and the proportionality of prices to money is the phenomenon.

[2] See Boumans (2005, ch. 6) and Stapleford (2009) for discussions of the history and construction of index numbers.

The ambiguity between data and phenomena, an ambiguity between the observable and the inferred, is recapitulated in the ambiguity in the status of shocks, which shift from data to phenomena and back – from observed to inferred – 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 accounts for the massive increase in their role in economics as documented in Figure 1. Shocks moved from secondary factors to the center of economic attention after 1973. That story tells us something about economic observation. And even though we are telling an historical tale, and not a methodological or philosophical one, the history does, we believe, call any simple reading of Bogen and Woodward's distinction between data and phenomena into question and may, therefore, be of some interest to philosophers as well as historians.

II. Impulse and Error

The business cycle was a central target of economic analysis and observation in the early 20th century. Mitchell (1913) and later Mitchell and Burns (1946) developed the so-called National Bureau of Economic Research (NBER) methods for characterizing business cycles based on repeated patterns of movements in, and comovements among, economic variables. These patterns (phases of the cycle, periodicity, frequency, distance between peaks and throughs, comovements among aggregate variables, etc.) can be seen as phenomena in Bogen and Woodward's sense and as the objects of theoretical explanation.

Frisch (1933) in drawing a distinction between propagation and impulse problems provides a key inspiration for later work on business cycles. Frisch represented the macroeconomy as a deterministic mathematical system of equations.[3] *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 (or difference) 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. To display the cyclical properties of an illustrative, three-variable economic system, he chose starting values for the variables away from their stationary equilibrium and traced their fluctuating convergence back to their stationary state values (Frisch 1933, Figures 3-5, 193-195).

The role of *impulses* was to explain why the system ever deviated from steady-state. Frisch drew on Knut Wicksell's 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" (Wicksell 1907 as quoted by 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 towards 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), and thus,

[3] "Macroeconomy" is an appropriate term here, not only because Frisch is addressing business cycles, but also because he is the first to explicitly conceive of them as belonging to this distinct realm of analysis, having originated both the distinction between macroeconomics and microeconomics and the related distinction between macrodynamics and microdynamics (see Hoover 2010).

together with the propagation mechanism explain the "more or less regular cyclical movements" we observe in reality. In a later article, Frisch (1936, 102) defines shock more carefully: he denotes by disturbance "any … new element which was not contained in the determinate theory as originally conceived" and shocks are those disturbances that "takes the form of a discontinuous (or nearly discontinuous) change in initial conditions [i.e., the values of all variables in the model in the preceding time periods that would, in the absence of this shock, determine their future values] (as these were determined by the previous evolution of the system), or in the form of the structural equations."

Frisch's own interest is principally in the propagation mechanism, and, though he sees shocks as a distinguishable category, he does not in 1933 give a really distinct characterization of them. This is seen in the way in which he illustrates an economic system energized by successive shocks. Rather than treating shocks as a random *variable*, he sees a shock as restarting the propagation process from a new initial value, while at the same time allowing the original propagation process to continue without disturbance (Frisch 1933, 201).[4] The actual path of a variable in an economic system subject to shocks is the summation at each time of the values of these overlapping propagation processes. The resulting "changing harmonic" curve looks considerably more like a business cycle than the graphs of the damped oscillations of equations started away from their steady-states but not subsequently shocked (Frisch 1933, 202, esp. Figure 6).

Frisch (1933, 198-199) credits Eugen Slutzky (1927/1937), G. Udny Yule (1927), and Harold Hotelling (1927) as precursors in the "mathematical study [of] the

[4] Cf. Tinbergen (1935, 242): "Frequently, the impulses present themselves as given initial conditions of the variables – comparable with the shock generating a movement of a pendulum – or as given changes in the data entering the equation."

mechanism by which … irregular fluctuations may be transformed into cycles." And he connects that work to more general work in time-series analysis due to Norbert Wiener (1930). 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 nor investigating the properties of its deterministic analogue.[5]

Neither Frisch nor Slutzky was engaged in measurement of the business cycle. The target of the analysis was not the impulse itself but the business cycle phenomena. 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 form of

[5] 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). Innovations, Frisch believed might produce an "auto-maintained oscillation." However, while he presents Schumpeterian innovation as "another source of energy operating in a more continuous fashion [than shocks] and being more intimately connected with the permanent evolution in human societies" (203), he leave to further research the task of putting "the functioning of this whole instrument into equations" (205).

the residual error terms from regression equations in structural-equation models (640)[6]. Frisch understood that the error terms of regression equations were not pure measures, but were a mixture of "stimuli" (the true impulse, the analogue of the club) and "aberrations" (Frisch 1938; see Qin and Gilbert 2001, 428-430).

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. On the other hand, the very cyclicality of the targeted variables may depend on cumulation of the impulse component – measurement errors canceling rather than cumulating. Haavelmo (1940, esp. Figures 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. Because the ability of a model to generate cycles appears to depend on true shocks, Haavelmo (1940, 313-314) argued in favor of treating the error terms as a fundamental part of the explanatory model against an approach that views the fundamental model as a deterministic one in which the error merely represents a measure of the failure of the model to match reality.

While Haavelmo and Frisch emphasize 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, ch. 8; Hendry and Morgan 1995: 40-41). In contrast, Haavelmo's dissertation, "The Probability Approach in Econometrics" (1944) , was a

[6] 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.

milestone in the history of econometrics (see Morgan 1990, ch. 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 was 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, are 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 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 towards practical macroeconometrics with, for the time, relatively large scale structural models of the Dutch and the U.S. economies (see Morgan 1990, ch. 4). Tinbergen's practical concerns and Haavelmo's probabilistic approach were effectively wedded in the Cowles-Commission program, guided initially by Jacob Marschak and later by Tjalling Koopmans (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, 320-321; 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 – what Frisch had referred to as the "inversion problem" (Frisch 1939, 638; Louçã 2007, 11, 95, *passim*). Their focus was then on the estimation of 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, a variety of 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 …"[7] 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[8]. Models were generally evaluated through their ability to track endogenous variables conditional on exogenous variables. 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, shocks as the causal drivers of business cycle phenomena were largely forgotten.

But not completely. In a famous article in 1959, Irma and Frank Adelman

[7] Tinbergen here spells the adjective "exogenous," whereas in 1935 (257) he had used both this (257) and the less common spelling, "exogeneous" (262). Some dictionaries treat the latter as a mispelling of the former; others draw a distinction in meaning; Tinbergen and probably most economists treat them as synonyms.

[8] See the manner in which models are evaluated and compared in Duesenberry *et. al.* (1965), Klein and Burmeister (1976) and Klein (1991).

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. The main contribution of the paper, however, was to propose a kind of Turing test for these shock-driven fluctuations (Boumans 2005, 93). If NBER techniques of business-cycle measurement in the manner of Burns and Mitchell were applied to the artificially generated variables, would they display characteristics that could not be easily distinguished from those of the actual variables. The test turned out favorably. While the Adelmans' test returned shocks to a central causal role in the business cycle, they did not turn the focus toward individual shocks but merely to their probability distribution.

III. The New Classical Macroeconomics and the Rediscovery of Shocks

In the age of econometrics, "shock" continues to be used with meanings similar in variety to those in use in the 19th century. Among these, "shock" is used, first in the sense of a dramatic exogenous event (for example, calling the massive increases in oil prices in 1973 and 1980, "oil shocks"); second, in the sense of irregular, but permanent, changes to underlying structure (for example, some uses of "supply shocks" or "demand shocks"); third, in the sense of one-time shift in an exogenous variable; and, finally, in the sense of a generic synonym for any exogenous influence on the economy. What has been added since 1933 is the notion that a shock is an exogenous stochastic disturbance, indeterministic but conformable to a probability distribution. Frisch already began to suggest this sense of "shock" with his distinction between impulse and propagation mechanisms and with his definition (cited earlier) of shock as an event not determined by the (deterministic part of) an economic theory. But it is Haavelmo's treatment of the residual terms of systems of stochastic difference equations as subject to well-defined probability distributions that in the final analysis distinguishes this sense of "shock" from earlier usage.

In the simplest case, shocks in the new sense are transients – independently distributed random variables. But they need not be. As early as 1944, econometricians "discussed the possibility of relaxing assumptions customarily made (e.g., randomness of shocks) so as to achieve a higher degree of realism in the models" (Hurwicz 1945, 79). Purely, random shocks were mathematically convenient, but the fact that models were lower dimensional than reality meant that some variables were autocorrelated with "'composite' disturbances with properties of moving averages" (79). The issue is not a question of the way the economy is organized but of the way in which modeling choices interact with the way the economy is organized. Christopher Sims (1980, 16) draws the contrast between one extreme form of a model "in which the nature of the cyclical variation is determined by the parameters of economic behavioral relations" and another extreme form "in which unexplained serially correlated shocks … account for cyclical variation." Sims, like Haavelmo before him, holds an economic account to be more desirable than a pure time-series account, but recognizes that the line is likely to be drawn between the extremes.

While the full range of meanings of "shock" remains, 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 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).

While rational expectations has been given various glosses (for example, people use all the information available or people know the true model of the economy), the most relevant one is probably Muth's original statement: "[rational] expectations . . . are essentially the same as the predictions of the relevant economic theory" (Muth 1961, 315, 316). 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 $p_t^e = E(p_t \mid \Omega_{t-1})$, where p is the price level, Ω 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 Ω_{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 $t-1$. This is analogous to the solution to a supply and demand system, which would not be an equilibrium if the price and quantity did not lie simultaneously on both the supply and demand curves.

Most of the characteristic and (to economists of the early 1970s) surprising implications of the rational expectations hypothesis arise from its general-equilibrium character. While a model may not be comprehensive relative to the economy as, say, a Walrasian or Arrow-Debreu model is meant (at least in concept) to be, rational expectations are formed comprehensively relative to whatever model is in place. And like its general-equilibrium counterparts, the rational expectations hypothesis ignores the processes by which equilibrium is achieved in favor of asserting that the best explanation is simply to determine what the equilibrium must be.

The mathematical expectations operator reminds us that "to discuss rational expectations formation at all, some explicit stochastic description is clearly required" (Lucas 1973, 328-329, fn. 5). Lucas points out that the convenient assumption of identically, independent "'shocks' to economy is not central to the theory . . ." Shocks could equally have a time-series characterization; but they cannot be *sui generis*; they must be subject to some sort of regular characterization. Lucas continues to use "shock" in some of its other meanings – for example, he characterizes a once-and-for-all increase in the money supply (an event to which he associates a zero probability) as a demand shock (Lucas 1975, p 1134). Yet, the need for a regular, stochastic characterization of the impulses to the economy places a premium on shocks with straightforward time-series representations (identically independent or dependent and serially correlated); and this meaning of shock has increasingly become the dominant one, at least in the technical macroeconomic modeling literature.

This is not the place to give a detailed account of the rise of the new classical macroeconomics. Still, some of its history is relevant to the history of shocks.[9] The same pressure that led to the characterization of shocks as the products of regular,

[9] For the early history of the new classical macroeconomics, see Hoover (1988, 1992).

stochastic processes also suggested that government policy be characterized similarly – that is, by a policy rule with possibly random deviations. Such a characterization supported the consistent application of the rational expectations hypothesis and the restriction of analytical attention to equilibria typical of the new classical school. 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 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" (1982, 383). Of course, this is a model-relative notion of general equilibrium (that is, 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 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, increasingly applied to different realms of macroeconomics, contributed vitally to the rise of shocks as an element of macroeconomic analysis from the mid-1970s on.

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. Widely referred to later as the "Lucas critique," the argument was straightforward: 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 basic outline of the argument was an old one (Marschak 1953; Tinbergen 1956, ch. 5; Haavelmo 1944, p. 28).

Lucas's particular contribution was to point to the role of rational expectations in connecting changes in the decisions of private agents with changes in government policy reflected in changes in the parameters of the policy rule. The general lesson was that a macroeconometric model fitted 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 notion of structure was more relativized than the one that Lucas appeared to advocate.[10] Lucas declared himself to be 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 (1980, esp. 702 and 707). 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 2010). The argument for microfoundations was barely articulated before Lucas confronts its impracticality – analyzing the supposedly individual decision problems not in detail but through the instrument of "'representative' households and firms" (Lucas 1980, 711).[11]

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 program of econometric identification in which theory provided the deterministic structure that allowed the error to be characterized by manageable

[10] See Haavelmo's (1944, ch. II, section 8) discussion of autonomy – also Aldrich 1989.
[11] See Hartley (1997) for the history of the representative-agent assumption. Duarte (2010a,b) discusses the centrality of the representative agent since the new classical macroeconomics and how modern macroeconomists trace this use back to the work of Frank Ramsey , who may have had in fact a notion of a representative agent similar to its modern version (Duarte 2009, 2010b).

probability distributions. The characterizations of shocks as a secondary phenomena here. 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 the estimation of 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's conservative approach. Haavelmo had argued for a decomposition of data into the deterministically explained and the stochastically unexplained and suggested relying on economic theory to structure the data in such a manner that the stochastic element could be characterized according to a probability law. 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 division of labor between economic theory and statistical theory became the centerpiece of econometrics. In some quarters, it was raised to an unassailable dogma. Koopmans, for instance, argued in the "measurement-without-theory" debate with Rutledge Vining, that without Haavelmo's decomposition, economic measurement was simply impossible.[12] Kydland and Prescott challenged the soundness of Haavelmo's decomposition (see Kydland and Prescott 1990, 1991, esp. 164-167; Prescott 1986; Hoover 1995, 28-32).

Kydland and Prescott took the message from the Lucas critique that a workable

[12] Koopmans (1947) fired the opening shot in the debate; Vining (1949a) answered; Koopmans (1949) replied; and Vining (1949b) rejoined. Hendry and Morgan (1995, ch. 43) provide an abridged version, and the debate is discussed in Morgan (1990, section 8.4).

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 which 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.[13] 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 "probablity approach," their alternative approach embraced Lucas's conception of models as simulacra:[14]

> . . . 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. [Lucas 1980, p 696-7]
>
> . . .
>
> 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, 709-710]

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

[13] "Dogmatically" is not too strong a word. Kydland and Prescott frequently refer to their use of "currently established theory" (1991, 174), a "well-tested theory" (1996, 70, 72), and "standard theory" (1996, 74-76), but they nowhere provide either a precise account of that theory, the nature of the evidence in support of it, or even an account of confirmation or refutation that would convince a neutral party of their credibility of their theory. They do suggest that the comparing predictions of theory with actual data provides a test, but they nowhere provide an account of what standards ought to apply to judged whether the inevitable failures to match the actual data are small enough to count as successes or large enough to count as failures (1996, 71).

[14] Lucas's view on models and its relationship to Kydland and Prescott's calibration program is addressed in Hoover (1994, 1995).

behavior on those limited dimensions (see Hoover 1995, section 3). Given the preference for general-equilibrium models with few free parameters, shocks in Lucas's framework became the essential driver and input to the model and generated 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" (Lucas 1980, 697). [15]

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 the model and the ability of the model to mimic business-cycle phenomena when shocked became the principal criterion for the empirical success.[16] Shocks in Lucas's and Kydland and Prescott's framework have 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 that actually drove the economy and 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 and contributed substantially to the widespread integration of the shock into a central conceptual category in macroeconomics, as record in Figure 1.

Both Kydland and Prescott's earliest business cycle model, the dynamics of which

[15] However, as Eichenbaum (1995, 1611) notes, "the problem is that Lucas doesn't specify what 'more' or 'mimics' mean or how we are supposed to figure out the way an actual economy responds to an actual shock. But in the absence of specificity, we are left wondering just how to build trust in the answers that particular models give us."

[16] See the various programmatic statements Prescott (1986), Kydland and Prescott (1990, 1991), and Kehoe and Prescott (1995).

were driven by technology (i.e., "real") shocks, as well as the so-called *dynamic stochastic general-equilibrium* (*DSGE*) *models* of which they were the precursors, were developed explicitly within Lucas's conceptual framework.[17] Kydland and Prescott (1982) presented a tightly specified, representative-agent, general-equilibrium model in which the parameters were calibrated rather than estimated statistically. They rejected statistical estimation – specifically rejecting the Cowles Commission's "systems-of-equations" approach – on the ground that Lucas's conception of modeling required matching reality only on specific dimensions and that statistical estimation penalized models for not matching it on dimensions that in fact were unrelated to "the operating characteristics of time series we care about." *Calibration*, as they define it similarly to graduating measuring instruments (Kydland and Prescott 1996, 74), involves drawing parameter values from general economic considerations: both long-run unconditional moments of the data (means, variances, covariances, cross-correlations) and facts about national-income accounting (like the average ratio among variables, shares of inputs on output and so on), as well as evidence from independent sources, such as microeconomic studies (elasticities, etc.).[18]

Kydland and Prescott's (1982) then judged their models by how well the unconditional second moments of the simulated data matched the same moments in the real-world data. They consciously adopted what they regarded as the standard of the Adelmans (and Lucas) in judging the success of their model (Kydland and Prescott 1990, 6; see also Lucas 1977, 219, 234; King and Plosser 1989). Yet, there were significant

[17] This is not to deny that DSGE models have become widespread among economists with different methodological orientations and have slipped the Lucasian traces in many cases, as discussed by Duarte (2010a).
[18] Calibration raises a large number of methodological questions that are analyzed by Hoover 1994, 1995; Hansen and Heckman 1996; Hartley et al. 1997, 1998; Boumans 2007.

differences: First, where the Adelman's applied exacting NBER methods to evaluate the cyclical characteristics of the Klein-Goldberger model, Kydland and Prescott applied an "occular" standard ("looks close to us"). The justification is, in part, that while they defended the spirit of NBER methods against the arguments originating in Koopman's contribution to the "measurement-without-theory" debate, they regarded the time-series statistics that they employed as a technical advance over Burns and Mitchell's approach (Kydland and Prescott 1990, 1-7).

Second, the parameters of the Klein-Goldberger model were estimated, and the means and standard deviations of the residual errors from those estimates provided the basis on which the Adelmans generated the shocks used to simulate the model. Without estimates, and having rejected the relevance of a probability model to the error process, Kydland and Prescott (1982) were left without a basis for simulating technology shocks. To generate the simulation, they simply drew shocks from a probability distribution the parameters of which were chosen to ensure that the variance of output produced in the model matched exactly the corresponding value for the actual U.S. 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 was to take the "Solow residual" – the measure of technological progress proposed by Robert Solow (1957) –as the measure of technology shocks. Prescott (1986, 14-16) defined the Solow residual as the variable z_t in the Cobb-Douglas production function $y_t = z_t k_t^{1-\theta} n_t^{\theta}$, where y is GDP, k is the capital stock, n is labor, and θ is labor's share in GDP. Since the variables y, k, and n are measured in government statistics, and the average value of θ can be calculated using the national accounts, the production function can be solved at each time (t) for the value of z – hence the term "residual." In effect, the production function was being used as a measuring instrument.[19]

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 accurately to measure true technology shocks, 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, 14 and 15, fn. 5) treated it as governed by a time series process $z_{t+1} = \rho z_t + \varepsilon_{t+1}$, where ε is an independent random error term and ρ is the autoregressive parameter governing the persistence of the technology shock. He argued that the variance of the technology shock was not sensitive to reasonable variations in the labor-share parameter (θ) that might capture variations in rates of capacity utilization. What is more, he claimed that whether the technology shock process was treated as a random walk (ρ =1) or as stationary, but highly persistent, series (ρ = 0.9), very similar simulations and measures of business-cycle phenomena (that is, of the cross-correlations of current GDP with a variety of variables at different lags) would result (see also Kydland and Prescott 1990).

Kydland and Prescott's simulations were not based on direct observation of

[19] See Boumans (2005, ch. 5) on models as measuring instruments, and Mata and Louçã (2009) for an historical analysis of the Solow residual.

technology shocks (that is, on the Solow residual), but on the statistical characterization of those shocks (the technology-shock phenomenon). Technology shocks, in their approach, were observed data in a way in which the residual error terms in the Klein-Goldberger model as used in the Adelmans' simulations were not. In part, the difference was a matter of intent. The Adelmans had no independent interest in the shocks. The statistical characterization of the error terms could be seen as a phenomenon; but, as we have noted previously, it was a secondary phenomenon to investigators interested principally in the time-paths of variables. Kydland and Prescott were also interested in the technology-shock phenomenon (that is, the time-series characterization of the shocks, especially their variability); but it is primary, not secondary, since their focus was on the covariation of the variables rather than their time paths.[20]

The difference with the Adelmans was also, in part, a matter of measurement. The shocks in the Klein-Goldberger model are fundamentally determined by every equation in the model and were measures of the lack perfect fit between the model under investigation and data – true residuals. Kydland and Prescott's technology shocks were somewhat similar, since the measurement equation, the Cobb-Douglas production function, was one element of the model. But the Solow residual is independent of the

[20] The real business cycle literature often focused on comparing unconditional moments from their models to those observed in the data. The major reason for this is the way they characterized cycles as recurrent fluctuations in economic activity, going back to Burns and Mitchel through Lucas's equilibrium approach and understanding that "the business cycle should be thought of as apparent deviations from a [long-term growth] trend in which variables move together. … The fluctuations are typically irregularly spaced and of varying amplitude and duration" but exhibit similar comovements between key aggregate variables (Cooley and Prescott 1995, 26; see also Kydland and Prescott 1982, 1359-1360). Besides this, examining time paths of variables implied by the model, compared to those observed in the data, requires the selection of appropriate initial conditions from which to simulate the model. Though their focus was on unconditional moments, there were a few RBC economists exploring the time paths of variables: see King, Plosser and Rebelo (1988), Plosser (1989) and Cooley (1995) for examples of calibrated RBC models that also track time paths of variables. Christiano (1988) is an early example of an estimated RBC model that compares theoretical time paths with those observed in the data. Novales (1990) uses a calibrated model to compare visually theoretical and estimated impulse response functions.

other equations of the model with, as Kydland and Prescott would argue, an independent validity. Shocks, on this measure, have a degree of model-independence and an integrity that allows the same measure of shocks to be used in different modeling contexts.

The dominant role of technology shocks in real-business cycle models has been as data used to characterize the shock process, the technology-shock phenomenon. From time-to-time, however, observed technology shocks have been treated as more direct inputs into real-business-cycle models. Hansen and Prescott (1993) fed technology shocks directly into a real-business-cycle model in order to model the time path of U.S. GDP over the 1990-91 recession. Although there are serious questions whether a good fit on such an exercise provides any independent confirmation of the model, the point that needs to be noted is that shocks were treated there in precisely the same manner as other observed data (see Hoover 1997; Hoover and Salyer 1998).

IV. The Identification of Shocks

The structural econometrics of the Cowles Commission was attacked from another direction. Whereas Kydland and Prescott had attacked Haavelmo's and the Cowles Commission's assumption that models define a tractable probability distribution, Sims (1980) attacked the credibility of the *a priori* assumptions used to identify the models.[21] Sims's methods were drawn from the time-series tradition in statistics rather than from the econometric tradition – in particular, Sims built on the work of Clive Granger (1969; see Sims 1972) – and developed a line of criticism that began with T.C. Liu (1960; cf. Chao and Huang 2011). Sims (1980, 1, 2, 14, 33) asserted that the identification assumptions of typical macroeconometric models are literally "incredible" and offered a

[21] Hoover (2006, 240-246) discusses Lucas's (and, by extension, Kydland and Prescott's) and Sim's attacks on the Cowles Commission's program as distinct, but interacting, critiques.

variety of arguments that sought to undermine the credibility of the usual identifying assumptions (6-11). Perhaps the most striking was one that saw the *a priori* assumptions needed to recover structural parameters related to expectational terms as "an order of magnitude more stringent in rational expectations models" (7). Where the first reaction to rational expectations – both among those who attempted to continue in the Cowles-Commission tradition and in those who rejected it – was to push all the more strongly for structural identification, Sims concluded that "rational expectations was more deeply subversive of identification than has yet been recognized" (7).

It is, however, the positive contribution of Sims's approach that bears most strongly on our story. The article explored the question of what can be learned about business cycles "without pretending to have too much *a priori* economic theory" (to quote the title of the earlier paper – Sargent and Sims (1977)).

Sims's (1980) took general-equilibrium, in one sense, more seriously than does the Cowles Commission in that he treated all the independently measured economic variables as endogenous. Like Haavelmo, Sims divided the model into a deterministic and an indeterministic part, but 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.[22] 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.[23] 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 (in a *Wold causal chain* via Cholesky decompositions, for example), 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 the individual equations of the model are not products of distinct exercises in economic theory" – that is, not structural in the Cowles Commission's sense. And he suggested that "[n]obody is disturbed by this situation of multiple possible normalizations." In fact, given *N* variables, there are *N*! possible

[22] Sims (1980) did not use the term "impulse-response function," which is first mentioned in economics journals catalogued in *JSTOR* in a review of a statistic book dealing with time-series models (*Econometrica* 1967). Sims's colleague, Thomas Sargent (1972) appears to have been among the first two economists to use "impulse-response function" and among the first four to use "impulse response" in an economics article catalogued in *JSTOR*. Sims did, however, provide graphs of "response of [a variable] to innovations in other variables" (e.g., 24), which is exactly what are commonly referred to as impulse-response functions today.

[23] Frisch's (1933) figures 3-5 (193-195) are essentially earlier impulse-response functions.

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, that policy-analysis, whether it was conceived of as changes in the parameters of policy rules or as shocks delivered by policymakers, required not just one of the possible renormalizations, but, in fact, the right one.[24] Sims (1982, 1986) rapidly conceded the point. His 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 through 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. To estimate a *structural VAR* (SVAR) – that is, a VAR with orthogonalized shocks – one needed 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, in which the problem of providing a basis for the credibility of the causal structure remains, though one might well imagine that less need be taken on faith than in the Cowles Commission's or calibrationist frameworks.

The important point for our story is that the SVAR put the shocks front and center – not because shocks could not have been identified in the Cowles Commission's framework nor because shocks are automatically interesting in themselves, but because the time-series properties of the shocks (that is, that they are independent and *mutually orthogonal* random variables) are essential to the identification strategy. Variance-

[24] See Demiralp and Hoover (2003) and Hoover (2005).

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. Kydland and Prescott treated the calibrated production function as a measuring instrument used to observe technology shocks. The SVAR can similarly be used as a measuring instrument to observed 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 just as Hansen and Prescott (1993) had actually used the particular observed technology shocks to generate specific paths for GDP, so might the SVAR modeler use the particular observed shocks to the whole system of equations to generate specific historical time paths for variables or to conduct counterfactual experiments.

To take one example, Sims (1999) conducts a counterfactual experiment in which he asks whether a monetary policy like that of the Volcker-Greenspan era would have improved economic performance in the Great Depression. This involved estimating a policy-rule and the actual shocks for the 1930s and combining them in a modified SVAR for the 1930s.[25] The key point, however, is to notice that the particular observed shocks are an essential part of generating the counterfactual, just as they had been in Hansen and Prescott's comparison of a the path of GDP generated by a real-business-cycle model to actual GDP.

The SVAR is another example of an economic model used as a measuring

[25] One difficulty with Sims's implementation is that it does not provide a valid response to the Lucas critique; see Hoover and Jordá (2001) and Hoover (forthcoming).

instrument. While in that respect similar to Kydland and Prescott's measurement of technology shocks, there are 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. Typical real-business-cycles are driven by technology shocks only, SVAR models necessarily observe shocks for each variable in the system.

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

The relationship of Sims's work on time-series econometrics and the new classical macroeconomics in the 1970s and 1980s was asymmetrical. Sargent, in particular, drew heavily on Sims's (1972) earlier work of Granger-causality and exogeneity, both as a means of determining suitable conditioning variables for the empirical implementation of rational expectations and as a way of testing the implications of competing models – e.g., natural and non-natural rate models (Sargent 1976; see also Hoover 1988, 182-185 and Sent 1998, ch. 3, esp. appendix 3B). Sims, as we have seen, mainly drew on the rational-expectations hypothesis to provide critical ammunition against the identifiability of macroeconometric models. Still, both approaches ultimately ended up with closely related positions relative to shocks: far from being relegated to the status of a secondary phenomena as in Haavelmo's and the Cowles Commission's methodologies, both the new-classical calibrationists and the SVAR modelers elevated them 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. Even then, economists were frequently interested more 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. Yet, the observability of shocks was *sine qua non* of identifying these phenomena in the first place. Both approaches, however, provided instances in which the particular values of shocks were treated as important in their own right.

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 to the variance decompositions and impulse-response functions generated from an estimated SVAR in which they 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 into one between the *new Keynesians* and the new classicals.[26] 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. They were Keynesian in the sense that, absent monetary or fiscal interventions, their models generated suboptimal outcomes that activist policy could ameliorate. 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 – the unconditional moments that Kydland and Prescott had suggested as characterizing business cycles. 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 set out to provide inviting real-business-cycle modelers to confront their models with "the documented impulse response facts about interactions of monetary and real variables" (980).

Sims (joining forces with Christiano (1988) and Singleton (1988)) wanted to reestablish the relevance of estimation methods in an area of research that had become

[26] Hoover (1988) gives treats the relationship of the monetarists to the new classicals. And Hoover (1992) gives a compressed account of the relationship of the new classicals to the new Keynesians; while Duarte (2010a) recounts how a new consensus developed in the 1990s between these competing research programs.

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. Once again, shocks 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). Christiano, Eichenbaum and Evans (1999, 68), for example, argued the covements 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 (that is, in the SVAR) and compared 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. While different in its implementation, this was precisely the interpretation that was offered by Haavelmo of ordinary econometric estimation of systems of equations – it was a method of introducing the analogue of experimental controls into situations in which only passive observation was possible.[27]

Much of the research in this vein focused on monetary shocks – that is, to shocks to short-term interest rates (in the U.S. context, typically the Federal funds rate). 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*. The "Taylor rule" was a typical reaction function (Taylor 1993). 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 illustrates very clearly 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 an 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 *et al.* 2010).

Sims's (1992) call for macroeconomists consider seriously time-series evidence was taken into consideration subsequently. While in his 1992 article he basically reported several poin-estimate impulse response functions obtained from alternative VARs for data from different countries, Leeper, Sims and Zha (1996) focused on the U.S. data and used sophisticated VAR methods to characterize features of aggregate time-

[27] Haavelmo (1944, esp. ch. II); see also Morgan (1990, ch. 8) and Boumans (2010)

series data. Here, in contrast to Sims (1992), the authors present confidence intervals for the estimated impulse response functions.[28]

Parallel to characterizing dynamic responses to shocks in the data through VARs there was the effort of building 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 Eric Leeper (Leeper and Sims 1994). Other significant papers in this vein are Christiano and Eichenbaum (1995), Yun (1996), and Christiano, Eichenbaum and Evans (1997). Here the parameters were either estimated with methods like 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 ususally judged in a rough-and-ready fashion – the same occular 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 further. Setting aside some of the fine details, the essence of their approach was to select the parameterization of the theoretical model in order 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 Rotemberg and Woodford's (1997) model failed to deliver the slow responses ("intertia") observed in impulse-response functions generated from SVARs. Other economists took

[28] Christiano, Eichenbaum and Evans (1996, 1999), among many others in a extensive literature, similarly explore the dynamic effects of monetary shocks in a VAR framework.

on the task of building DSGE models, estimated by impulse-response matching that captured the inertia of the impulse-response functions (Christiano, Eichenbaum and Evans 2005 and Smets and Wouters 2007 – see Duarte forthcoming).[29]

Rottemberg 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 phenomena, the shocks were now moved down a level. They stood in the same relationship to the new data as the raw prices of individual goods did the CPI. 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.

VI. Shocks, Macroeconometrics, and Observability

We have addressed three main questions in this paper. 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

[29] Thus, the observation of the business cycle phenomena through impulse response functions to a (monetary) shock is now the basis upon which these macroeconomists build their DSGE models that are taken to policymaking. The very model used to observe the cycle, tuned to replicate the data in some key dimensions, is used by policymakers to act upon the economy (which in turn feeds back to the dynamic responses that guide DSGE model building). This provides a not obvious example of performativity in economics (alternative to some issues analyzed in Mackenzie, Muniesa and Siu 2007).

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 – that is, modeling the cycle in its own right – 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.

It was 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 a largely ignorable supplement that shocks returned to center stage 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" and "shocks" 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. But if anything, the history of shocks calls into question whether any such distinction can be given a deep, principled defense, however useful it may prove as a rough-and-ready contrast. The problem as we saw was that at different times shocks could best be regarded as phenomena, inferred from observable data; or as data observed using models as measuring instruments; or as the raw material from which data were constructed (as, for example, with the Solow residual), which were then used as an input to generate further phenomena or which served as the basis for higher-order inference. The attraction of the distinction between observation and inference in physical sciences is the very human notion that there is something privileged about what we see with our own eyes or hear with our own ears. But economics, even in its deepest reaches, is not about things that we see, hear, touch, or feel, but about relationships. A government worker may see a price and record it, a price in itself is not the visible printed numerals pasted on a store’s shelf, but a three-way relationship among a buyer, a seller, and a good. If the notion of observation is to do any work in economics, it must account for the observability of such relationships and must not, therefore, ground them fundamentally in human sensory capacities. 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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