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# Karl Brunner and Monetarism

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# 12

## Karl Brunner's Philosophy of Science: Macroeconomics through the Lens of Logical Empiricism

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### 12.1 A Philosophical Economist

Karl Brunner recalled that on finishing his PhD in 1942, he joined the economics department of the Swiss National Bank, which at the time was something of a quiet backwater. One lesson from his time there stuck with him for the rest of his career: "Self-declared practical men of affairs frequently are, without seeming to realize it, ardent theorists" (Brunner 1991, 20).<sup>1</sup> The policy maker, like all men, is "a theorizing animal," and understanding how theory implicitly or explicitly shapes economic cognition and economic policy became Brunner's lifelong quest (Brunner 1980, 306). Brunner was by no means a professional philosopher, but he had a philosophical turn of mind—a preference for order and precision in thought and an ability to apply sophisticated philosophical ideas to economics. His encounter with a fellow émigré, the "lucid philosopher of science" Hans Reichenbach, at the University of California, Los Angeles (UCLA), in the early 1950s was an intellectual watershed (Brunner 1980, 304). He absorbed Reichenbach's *Prediction and Experience* (1938) and made a "substantial investment in logic and the philosophy of science" that, along with other influences, "dispersed the intellectual fog and gradually structured [his] thinking about economics and its role in our endeavor to understand the world" (Brunner 1991, 22; 1980, 304).

Reichenbach was one of the leading lights of the philosophical movement known as *logical empiricism* (Caldwell 1994, chap. 3). Brunner mentions other philosophers with a similar orientation as having influenced his thinking, including Rudolph Carnap, Ernst Nagel, and R. B. Braithwaite (Brunner 1984, 11). Logical empiricism was the successor to *logical positivism*, mainly associated with the Vienna Circle in the 1920s and early 1930s (see Sigmund 2017). The logical positivists adopted the extreme position that all meaningful statements were either statements

of logic and mathematics or reported verifiable empirical observations. Every statement that was neither a necessary truth of logic or mathematics nor a verifiable empirical observation was metaphysics and literally nonsense that, following Hume's dictum, we ought to "commit it . . . to the flames: for it can contain nothing but sophistry and illusion" (Hume 1777, sec. XII, pt. III; Ayer 1946; Caldwell 1994, chap. 2). Few philosophers stuck with the strict version of logical positivism. Among other problems was that a richer understanding of the actual practice of science convinced most that scientific theories involved indispensable terms that were neither logical truths nor *directly* empirically verifiable. For example, neither quarks nor permanent income is directly observable; yet quantum mechanics requires the one and macroeconomics the other. Philosophers were loath to call such successful science "nonsense." By the time that Brunner encountered Reichenbach, the principal problem for the logical empiricists was to understand the role of theory as somewhat independent from, yet actively tethered to, empirical observations.

Brunner cannot be considered a major contributor to the pure philosophy of science. Rather, he pursued a lifelong project in practical philosophy. Motivated by the problem that he had encountered at the Swiss National Bank, he sought to develop the lessons of the logical empiricism that he absorbed at UCLA and apply them to the problems of monetary economics and macroeconomics, and he did so with clarity and consistency that are rarely matched among economists.

Not unlike his position with respect to monetarism, Brunner's contributions to the philosophy and methodology of economics are overshadowed by Milton Friedman. If economists know any work of methodology at first hand, it is most likely Friedman's "Methodology of Positive Economics" (1953). Brunner found much to admire in Friedman but criticized his methodology—or at least its exposition—over several decades. Friedman, in Brunner's view, lacked semantic precision, employed inadequate formalizations, and relied far too much on metaphors and examples, rather than on a logical presentation, to make his points (Brunner 1957, 513; 1969, 504; 1980, 303, 317; 1984, 18; 1991, 26). Brunner's criticisms gained little traction. He, in fact, wrote few philosophical works, publishing only one in a philosophy-of-science journal (Brunner 1969).<sup>2</sup> It is a backhanded defense of Friedman—essentially supporting his conclusions while replacing his loose rhetoric with a rigorous logical analysis. Although Brunner took Friedman's rhetorical approach to be the weakness of his methodological essay, that approach may, in fact, account for the essay's accessibility and wide readership. Brunner's essay is rigorous and enlightening,

but it is neither easily accessible nor widely read or cited.<sup>3</sup> Its importance is less for itself than for its encapsulating the philosophical positions that are reflected throughout Brunner's economic writings. We must look to those writings to see just how consistently and thoroughly Brunner applied philosophical analysis to the practical problems of economics.

### 12.2 Logical Empiricism

Despite its lack of direct influence and even though it is specifically couched as a focused effort to add precision to Friedman's methodological argument, Brunner's "Assumptions" and the *Cognitive Quality of Theories* (1969) remains the best starting place for understanding his philosophy of science. The voluminous debate over Friedman's methodology essay is deeply muddled and, after more than six decades, remains unresolved (see Hirsch and De Marchi 1990; Caldwell 1994, chap. 8; Mäki 2009). Friedman was famous (or infamous) for his claim that "truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)" (1953, 14). Most of the ink has been spilled over the question of "realism": Is it dispensable? How could lacking it be helpful? What is "this sense"? ad nauseam. Brunner, in contrast, located the fundamental confusion in the ambiguities surrounding the meaning of assumptions. His paper is a tour de force of logical and semantic analysis, discerning seven distinct uses of "assumptions" in the debate. Although Brunner contributed a novel clarity to the debate, the background exposition of his logical empiricist framework in the article is of greater interest for our purposes. It is his only sustained and systematic explication of his philosophical framework. He articulates the essential cognitive role of theory in an empirical science and shows how that methodological understanding could guide economics to resolve debates and build knowledge.

Following the lead of his logical empiricist mentors, Brunner distinguishes between observational and theoretical statements (Brunner 1969, 506–507). He recognizes that the distinction is a pragmatic one, conceding that observations themselves may be theory laden to different degrees. Observations are singular facts, describable using first-order predicate logic.<sup>4</sup>

When we report an observation, we make an existential claim—for instance, "the unemployment rate is 5.7 percent." Theory, in contrast,

involves statements about observational statements and statements about other theoretical statements. Theory requires higher-order logic and mathematics. Theory is universal in the sense that it refers to any possible case that fulfills its conditions. Rather than involving existential quantification (e.g., claims such as "there is an electron at point A at time  $t$ " or "the Federal funds rate on 3 March 2020 was 0.27 percent"), theory involves universal quantification (e.g., "if anything is an electron, it carries a negative charge" or "all perfectly competitive economies are Pareto efficient"). Universal operators ("For all...") make claims that apply in general but do not assert the existence of objects, situations, or relations that actually exemplify those claims. "All unicorns have a single horn" is not a claim that there are, in fact, any unicorns. Existential operators ("...exists") do not assert general claims but do assert that specific objects, situations, or relations in fact exist ("There is a unicorn in my garden right now"). "Long bond yields are the geometric average of a sequence of short bond yields covering the same time horizon" is a universal, theoretical claim; in contrast, "the 10-year bond yield is 2.2 percent" is an existential, factual claim.

The absence of existential operators implies that theory can say nothing about the actual world unless it is joined to empirical premises—that is, to observational facts, which serve as initial conditions for deductive arguments that imply observations or potential observations that go beyond the initial premises themselves. There are two parts to tying theory to the actual world.

First, theoretical terms must be given semantic content that allows them to be directly or indirectly identified with observational facts. This is a weaker requirement than the logical positivist view that each meaningful statement must be individually empirically verifiable. Instead, some terms in the theory must correspond to the categories of observations so that deductions from the theory can be compared with observations. Brunner is clear that observations may be theory laden: theory itself may help shape observational categories. For example, the United States Congressional Budget Office publishes a time series for the natural rate of unemployment. These are not direct observations but are backed out of a specific model of the expectations-augmented Phillips curve.

Second, because deduction is truth preserving, if the premises of the argument (theoretical or observational) are true, then the conclusions (i.e., empirical predictions) are necessarily true. But how do we know that the premises are true? Initial conditions are either directly asserted based on observation (e.g., "the rate of growth of gross domestic product is 2.1 percent") or stipulated as counterfactual claims such as those one

finds in policy analysis (e.g., "suppose the Federal Reserve raises the Federal funds rate 2.5 basis points"). (Brunner, of course, does not rule out the possibility of observational errors.) But how are theoretical premises to be established?

Brunner sees two options:

- Option 1: they may be established a priori as *first principles*, without the need for empirical support.
- Option 2: their truth may be grounded in empirical observations.

Option 1 is advocated by *rationalists* (e.g., Descartes among philosophers or Ludwig von Mises among economists). Brunner summarily rejects option 1 while accepting option 2. This position defines him as an *empiricist*. Still, option 2 requires further defense.

Although Brunner does not explicitly mention David Hume, he is clearly fully aware of Hume's riddle of induction (Hume 1777, sec. IV). Induction is the form of inference that derives a general conclusion from particular instances. The riddle is, "On what basis is it legitimate to infer a universal statement from any finite number of singular facts?" Hume argued that although people found it psychologically compelling to generalize from particulars, there was no *logical* basis for such inferences. The hackneyed illustration is that after seeing 10,000 white swans and none of any other color, we have no logical reason to conclude that the 10,001st swan will be white or to assert that "all swans are white." Various philosophers have tried to resolve Hume's riddle in different ways. Karl Popper (1959, chap. 1; 1963, chap. 1) rejects all such solutions, accepting Hume's riddle as a demonstration that there is no logic of induction. The best that we can do is to accept that all theoretical knowledge is conjectural: universal claims cannot be verified but may be falsified. No number of white swans proves the claim that "all swans are white," whereas a single black swan falsifies it.

Without citing Popper, Brunner embraces his purely logical claim. But Brunner's position is more subtle and goes beyond a simple account of conjectures and refutations in three ways.

First, Brunner accepts that there is no induction by enumeration—for example, by counting and classifying swans (Brunner 1969, 508). Just the fact that theories are constructed using only universal quantifiers implies that much: in science, we are rarely interested in claims as straightforward as "all swans are white." But for Brunner, simple enumerative examples do not reveal much about actual scientific theories.

Second, falsification in Brunner's view is a necessary but not a sufficient condition for rejecting a theory (Brunner 1969, 507). Observations may

be inconsistent with a theory—that is, *falsify* it. They may also be consistent with a theory—that is, *confirm* it. Confirmation does not provide inductive justification; it merely indicates the compatibility of the theory with the evidence to date. Brunner's point is similar to the distinction drawn by Imré Lakatos, who wrote around the same time but was not cited by Brunner, between *refuting a theory* (i.e., falsification) and *rejecting it* (i.e., giving it up).<sup>5</sup> Like Lakatos, Brunner believed that no theory is perfect and that we do not reject theories in isolation but only relative to seriously offered alternative theories (Brunner 1969, 507–508). Various logical empiricists and other philosophers have attempted to develop logics of confirmation, although all of them are problematic. Brunner simply embraces the idea that the degrees of confirmation of different theories can be compared without elaborating on how the relative degree of confirmation or even the rank ordering of theories as better or worse confirmed is to be established.

Third, knowledge is acquired in Brunner's view through an evolutionary process in which the fitter theories survive a competition of relative confirmation and others are rejected (Brunner 1969, 501–502, 508). We start neither with first principles nor with completely articulated theoretical systems but with the observable world and our own mental capacities. A science grounded in first principles would be

untenable and conflict with the reality of our cognitive progress over history. Science rarely progresses by “working down from first principles”; it progresses and expands the other way. We begin with empirical regularities and go backward to more and more complicated hypotheses and theories. Adherence to the Cartesian principle would condemn science to stagnation. There are, moreover, as Karl Popper properly emphasized, no first principles. (Brunner 1984, 16; cf. 1989, 195)

In contrast to theories, observables are in the world; yet how we look at them is up to us. Our theories are part of the cognitive resources that we use to look at the world in ways that we hope will be fruitful. Although we can never know with certainty that our theories are true, without them we would be cognitively helpless. For Brunner, at best, we can know (a) that a contradiction exists between our theory and an observation, and (b) that one theory is better or worse confirmed relative to another theory. His scientific strategy, then, is (a) to maintain the best confirmed theory as our working hypothesis for practical use and (b) to try to develop a better theory that avoids the contradictions between our maintained theory and empirical observations.

Brunner steers a path between inductionism and dogmatic versions of Popperian falsificationism. In this, as in many other instances, Brunner

is a *radical moderate*, finding the middle ground between extremes but doing so for principled reasons and not simply from an irenic or tolerant impulse. To take another example, Brunner rejects the Walrasian vision of the perfectly articulated, complete model of the economy. He does not, however, reject either the importance of the idea of interdependence encapsulated in the notion of general equilibrium or the formal models as illustrations of the possibility of the emergence of order out of uncoordinated decision-making (Brunner 1987, 376–377). He tempers his admiration, nonetheless, with the recognition (in the mode of John Maynard Keynes and Friedrich Hayek) that these models typically misrepresent the degree of ignorance that we face when making real-world decisions.

With respect to the Walrasian vision, Brunner (1972b, 268) rejects the idea of a “Platonic supermodel” of the economy—what the philosopher Paul Teller (2001) has called the “perfect-model model” of knowledge—in favor of a model of the piecemeal acquisition of knowledge:

The history of our cognitive efforts progressed successfully with a systematic denial of the thesis that “everything depends more or less uniformly on everything else.”

...

Successful cognition selected comparatively small subsets of factors which effectively explain the major outline of a given phenomenon. (Brunner 1970, 7)

And yet, as a radical moderate, Brunner also rejects cognitive pluralism and the idea that we should have a suite of distinct approaches, each acceptable in its own domain (Brunner 1987, 378; Brunner and Mechling 1977, 79). Instead, he advocates *seeking* unified approaches to science—both within and across disciplines—while rejecting either unification or the notion of a priori first principles as starting places or *sine qua non* of science.

One element in his effort to achieve unification in science is his recognition of a hierarchy of theories (Brunner 1969, 506). A simple empirical generalization (e.g., “for all economies, inflation is correlated with the growth rate of the money stock”) is an elementary theoretical claim. It goes beyond observation but is testable. An observation of an economy in which such a correlation was not found would refute it (but would not necessarily lead us to reject it). Still, such an empirical generalization is analytically shallow. A higher-level theory of the economy (e.g., the IS-LM model, Brunner and Meltzer's own three-asset extension of that model, or the dynamic stochastic general-equilibrium [DSGE] model) from which such elementary empirical generalizations could be derived provides us with a richer cognitive articulation and a greater analytical capacity, which



are helpful, for example, in guiding policy and understanding its limitations. Our deep need for such cognitive resources goes a long way toward explaining Brunner's early observation that policy makers always have an implicit theory. His life goal was to make such theories explicit, richer, and more empirically successful.

Although more sophisticated higher-level theories can be valuable, Brunner rejects the notion, which appears to him to be implicit in the practice of many economists, that simply providing a derivation of a lower-level theory from a higher-level theory adds to the credibility or verisimilitude of the lower-level theory (Brunner 1971, 168; 1984, 11). Deduction is *truth preserving*: if its premises are true, then its conclusions must be true. It is not *truth establishing*: the truth of its premises can be warranted only on the basis of empirical observation. Similarly, observations support lower-level theoretical generalizations, which in turn support higher-level theoretical generalizations. As Brunner (1969, 509) puts it, "Both confirmation and assignment of meaning move actually in the direction opposite to the lines of logical deduction." *Truth transmits up, not down.*

### 12.3 Philosophy of Economics

Going beyond "Assumptions' and the Cognitive Quality of Theories" (1969), Brunner's other forays into philosophy deal mainly with political philosophy, which is beyond the scope of this chapter. Still, in two articles, Brunner (1972a; Brunner and Mechling 1977) contrasts different conceptions of man that define the essential difference between economics and sociology as well as other fields, such as political science and psychology, and offers his clearest articulation of the distinctive nature of economic science. Consistent with his logical empiricism, Brunner is a scientific unionist and rejects the idea that there are distinct scientific standards that differ between natural and social sciences or between economics and other social sciences. Economics is *in fact* different from sociology in its approach to social science. But this difference is one that Brunner wishes not to embrace but to resolve.

Brunner's master insight is that economics starts (or should start) with a notion of "REMM—Resourceful, Evaluating, Maximizing Man" (Brunner and Mechling 1977, 71; Brunner 1983, 233). Although he fails to acknowledge it, his notion is close to Lionel Robbins's (1935, 16) definition of economics as "the science which studies the relationship between ends and scarce means which have alternative uses" and to Popper's advocacy of

*situational logic* as the fundamental method of analysis in the social sciences (see Hoover 2016).

In advocating REMM as the starting place for economic analysis, Brunner is once again a radical moderate. On the one hand, Brunner rejects all visions of man that treat him as a cipher or inert element ("passive engineering particles" [Brunner 1980, 312]) shaped entirely by forces outside his control. On the other hand, he rejects the vision of man as "a brainy, but heartless calculating machine."<sup>6</sup> "Charitable behavior, love of family, compassion, can be consistently subsumed" within the REMM framework (Brunner and Mechling 1977, 72). Resourcefulness can be thought of as rationality, but not as rationality narrowly conceived as solutions to well-defined optimization problems. We are generally too ignorant of the relevant details (per Hayek) or too uncertain about future probabilities (per Keynes) to comprehensively model the overall situation. Brunner does not rule out formal optimization as a tool for narrowly defined problems (Brunner 1987, 372–373). More generally, however, he takes rational, evaluating, and maximizing as pointing toward coping strategies that suit human limitations. Many social institutions are the result of a resourceful adjustment to the limits of our knowledge and cognitive resources. Brunner frequently insists that the most important thing about money, for instance, is not that it is a means of transactions or a store of value—although it is both those things—but that it is an institution for dealing with our ignorance and lack of information in a resourceful way (Brunner 1971, sec. 3; Brunner and Meltzer 1971). Similarly, Brunner deprecates Herbert Simon's terminology "bounded rationality"—not rejecting Simon's idea but objecting to its suggestion that "satisficing" is a deviation from rationality (Brunner 1987, 378). It is rather exactly what rationality or resourcefulness calls for when we are ignorant, uncertain and possess limited computational abilities.

For Brunner, REMM is the only game in town for social sciences (Brunner 1980, 318–319). With this assertion, he does not raise it to the role of an a priori first principle. It is instead a higher-level theoretical premise that can be justified only on the basis of superior empirical performance relative to alternative conceptions of man. Brunner believes that economics, proceeding more or less on the basis of REMM, has in fact been more empirically successful than sociology. The problem with sociology in his view is that it lacks a developed theory (Brunner and Mechling 1977, 71). It is, he believes, stuck at the level of empirical generalization. Its so-called theory amounts to little more than description. These descriptions are useful knowledge—and grist for the mill of

economics—but do not provide the cognitive tools that would support a sophisticated, empirically relevant theoretical or policy analysis (Brunner 1987, 368). Lacking such developed theory, sociology can neither predict the effects of policy actions nor give principled accounts of the likely operation of institutions.

In keeping with the joint premises of a unified science and the supposed empirical superiority of REMM, Brunner is a supporter of *economics imperialism*—the notion frequently associated with the work of Gary Becker, which applies the tools of economic analysis within domains that have typically been reserved for sociology, anthropology, political science, and social psychology. For Brunner, economics imperialism is the inevitable prerequisite of actually making theoretical progress in the noneconomic social sciences.

#### 12.4 Econometrics

Because Brunner was an empiricist, sound econometrics was important to his understanding of economic science, and he used his logical empiricism as the basis for trenchant criticism of common econometric practices. Throughout most of his career, his principal target was the practices of the large-scale Keynesian macroeconomic modelers, such as Lawrence Klein, James Duesenberry, Franco Modigliani, Albert Ando, and their teams of colleagues and assistants, who developed the Brookings model, the Massachusetts Institute of Technology—University of Pennsylvania—Social Sciences Research Council (MPS) model, and similar efforts. The key point was that for Brunner, econometrics was not fundamentally about statistics and correlations; instead, it was about using statistics to test hypotheses and theories. With their focus on forecasting performance, the Keynesian macromodelers, in Brunner's view, let their modeling enterprise lose touch with economic theory, suffering from “confusions approaching logical illiteracy concerning the nature of evaluation” (Brunner 1973, 932; see also Brunner 1980, 318). As a matter of history, we need not necessarily agree with Brunner's criticisms of the Keynesian macroeconomic program—that is, whether they actually did commit the methodological sins that he attributes to them—to appreciate the fact that his criticisms were substantially grounded in his logical empiricism or that they illuminate just what that philosophy of science amounted to in his hands.

The logic that Brunner requires of macroeconomics is fundamentally the logic of his overall philosophy of science. A model must be formulated

that, when combined with observable initial conditions, allows the deduction of observable test statements that can be confirmed or disconfirmed with observable data. He taxes the Keynesian macroeconomic modelers with paying insufficient attention to the correspondence between available data and theoretical concepts, taking as an example the mistake of treating data that are only rank ordered as subject to meaningful addition or multiplication (Brunner 1972b, 273). Brunner also suggests that the macroeconomic modelers fail to account for the stochastic nature of their models. Stochastic models do not make point predictions but instead generate test statements that make probabilistic claims (Brunner 1973, 932). Any meaningful model must rule out some states of the world: “No cognitive claim can be seriously considered without some delimitation of a positive content, a nonvanishing range of observations highly unlikely to occur whenever the claim is true. An estimation procedure with unknown stochastic properties prohibits any possible delimitation of even vague scanning of the empirical content” (Brunner 1973, 930; see also Brunner 1968, 781). Here, Brunner anticipates the focus on “severe testing” advocated by Deborah Mayo and Aris Spanos (2006).<sup>7</sup> A test is severe only if we compare it with an alternative hypothesis that gives it every chance to fail and yet it succeeds. Only severe tests have probative value.

In Brunner's view, the Keynesian macroeconomic modeling project is a failure, and the root cause of its failure is found in its focus on forecasting performance (Brunner 1973, 932). Brunner does not deny the pragmatic value of forecasting but argues that the macroeconomic modelers have mistaken pragmatically valuable forecasts for meaningful tests of the cognitive value of their models (926). Forecasts that offer singular predictions are not appropriate tests of stochastic theories. What needs to be confirmed is not the closeness of a forecasted value to an outcome but the closeness of a frequency distribution of predicted values to the probability distribution deduced from the model. Clients may, for a variety of purposes, wish to have a point forecast and value it when it comes close to the actual outcome; yet that does not bear on the truth of the model because stochastic models generally do not rule out singular predictions as impossible but rather predict their relative frequencies (930).

In an effort to satisfy the pragmatic needs of clients, the macroeconomic modelers constantly adjust their models by building in factors that account for the systematic drifting of new data away from the supposed structural estimates or by allowing subjective judgments to override the actual forecasts deduced from the model (Brunner 1972b, 770–771). Brunner makes it clear that he does not object to updating estimates with

new data that render them more precise. Rather, he objects to the way in which these procedures sever the link between the theoretical model and the observable outcomes in a way that vitiates any possibility of the observations being logically salient tests of the theory (Brunner 1980, 317–318). Subjective model adjustments encourage the economist to tweak the model until it suits his or her prior beliefs, and focusing on the maximization of forecast fit by any means necessary reduces the cognitive role of the theoretical model in generating forecasts (Brunner 1973, 930). After sufficient tweaking, Brunner suggests that the equations of the model are dominated by *implicit* dummy variables that guarantee fit but explain nothing (938n4).

Brunner also notes that the preferred estimation procedures for large macroeconomic models typically rely on instrumental variables that are chosen not on the basis of a careful theoretical and empirical investigation of their appropriateness but on the basis of informal, casual justifications and a priori beliefs. Combined with the absence of attention to stochastic structure, estimates using such instrumental variables involve a large number of unarticulated, implicit hypotheses that render the connection between test statistics and the explicit hypotheses being tested meaningless (Brunner 1973, 931; 1991, 27).

Brunner faults not only the Keynesian macroeconomic modelers but also econometricians in general for failing to engage in rigorous model comparisons that would allow them to confirm or disconfirm the implications of competing theories. At the most basic level, he excoriates the practice of focusing on zero null hypotheses (Brunner 1991, 27). To find a test statistic to be significantly different from a null of zero would only rarely constitute a severe test and typically would not be very informative within a methodological framework that stresses the comparison between competing, seriously maintained hypotheses.

More broadly, Brunner argues that the macromodelers failed to use their large econometric models as an adequate tool for discrimination among theories (for example, between Keynesian theory and Brunner's own monetarist theory) or even to formulate them in a way that they could be adequate for that purpose. He believed—and regarded it as a methodological scandal—that the large-scale macroeconomic modelers made no serious effort to evaluate the more than twenty competing macroeconomic models extant in 1972 (Brunner 1972b, 279).<sup>8</sup>

Never one to mince words, Brunner (1973, 930) accuses Keynesian macroeconomic modelers of having abandoned “empirical science for

a numerology similar to astrology” and of failing to live up to the strictures of a logical empiricist methodology (Brunner 1973, 930).

In the debate between monetarists and Keynesians in the 1960s and 1970s, Brunner again relied on logical/philosophical analysis to bolster the monetarist side. Keynesians preferred “structural” models, following the tradition of the Cowles Commission that sought to isolate fundamental connections among variables by providing coefficient estimates within systems of equations that were supposed to capture detailed channels of causal influence (Koopmans 1950; Hood and Koopmans 1953; Morgan 1990, chap. 8). Typically, these estimates were obtained by the method of estimating *reduced forms*—that is, statistical regression equations in which endogenous variables were regressed only on exogenous variables. To recover the structural coefficient estimates from the reduced forms, the Cowles Commission's methodology required prior knowledge of the underlying functional structure. The main challenge was that distinct structures could be solved to deliver the same reduced forms. Econometric models were considered to be “identified” if, and only if, the restrictions embedded in the prior structure permitted the recovery of unique estimates of the structural parameters from the reduced form.

Brunner does not challenge the logic of identification. Instead, he objects, first, to what he believes to be an illogical regard on the part of structural modelers for the derivability of their structural models from higher-level models or supposedly deeper theoretical principles. In one case, he criticizes James Tobin's insistence that his structural models of financial markets gain credibility from the derivability of their functional forms from the Markowitz risk-return portfolio theory (Brunner 1971, 168–169). This reverses the logic of confirmation in Brunner's view: the evidence of truth transmits upward from observations to theory at increasingly higher levels and not downward from higher-level theory to lower-level theory.

Second, Brunner characterizes the Keynesian critics of monetarism as dismissing the evidence for monetarism—in particular, in Leonall Andersen and Jerry Jordan's (1969) work at the Federal Reserve Bank of St. Louis—because it was derived from reduced-form estimates that did not articulate structural parameters.<sup>9</sup> Brunner denied that the differences between the monetarist and Keynesian views could be reduced to the differences in parameter values within a common model, such as the IS-LM model or one of its Keynesian elaborations (Brunner 1970, 4, 23; 1980, 309). The dispute, he believed, was about more fundamental theoretical



differences and could not be resolved on the basis of particular coefficient estimates within such a Keynesian model. The attack on reduced-form modeling was, in Brunner's view, illogical because models in the Keynesian class, even ones with different structural details, implied distinct reduced forms relative to models in the monetarist class. The evidence for or against each of these reduced forms amounted to evidence for or against the entire class (Brunner 1984, 11). Although it may be useful at some later stage to discriminate among models within the same theoretical class, no insistence on structural articulation would change the import of the evidence for each class as a whole.

### 12.5 Macroeconomic Model Building and Aggregation

Yet another example of Brunner's radical moderation is the preference that he and his frequent collaborator Allan Meltzer showed for middle-sized formal macroeconomic models—in particular, their three-asset elaboration on the IS-LM model—adopting neither the extreme in which Keynesian macroeconomists developed models with hundreds of equations nor Friedman's practice of almost eschewing formal modeling altogether (Brunner and Meltzer 1976).

Friedman was a monetarist ally; yet in Brunner's view, Friedman was too content with low-level causal hypotheses, and Brunner urged him to articulate the monetarist theory with greater precision (Brunner and Meltzer 1974). Although Friedman (1974) succumbed and attempted to shoehorn his monetarism into an IS-LM framework, Brunner and Meltzer found the effort unsuccessful, conceding too much to the view common among Keynesians that the two schools essentially shared a common framework and that their differences were resolvable through the estimation of particular parameters of a common model.

Unsurprisingly, Brunner's objections to the Keynesian modelers are elaborated in more detail. Klein, for instance, favored the most complicated, feasible models. As he put it late in his career:

In contrast with the parsimonious view of natural simplicity, I believe that economic life is enormously complicated and that the successful model will try to build in as much of the complicated interrelationships as possible. That is why I want to work with large econometric models and a great deal of computer power. Instead of the rule of parsimony, I prefer the following rule: the largest possible system that can be managed and that can explain the main economic magnitudes as well as the parsimonious system is the better system to develop and use. (Klein 1992, 184)

For Brunner, the impulse behind both Klein's commitment to complicated macroeconomic models and Tobin's similar commitment to complicated models of financial markets was their "rationalist heritage" with its commitment to first principles (Brunner 1971, 168). In both cases, the first principle was a commitment to the Walrasian model, in which the economy is explained in principle through accounting for the decision problems of each and every separate agent.

Brunner's argument is not that the Keynesian macromodelers ignore data but that they practice "a remarkable combination of Schmoller and Walras" (Brunner 1971, 168).<sup>10</sup> Brunner suggests that in their urge to disaggregate and fulfill the Walrasian vision of a complete model of the economy grounded in individuals, the Keynesian macromodelers lost their grip on the cognitive function of a behavioral economic theory and the possibility of an explanation of economic phenomena that might go beyond the description and categorization of the data.

A commitment to microfoundations, the idea that macroeconomics must somehow be grounded in the microeconomic behavior of individuals, is widespread among economists (for a history, see Hoover 2012). Even Keynes, who is widely, if not quite correctly, credited with inventing macroeconomics, explained the behavior of aggregate data largely with reference to the economic choices of individuals.<sup>11</sup> Klein (1947, 57), however, sharply criticized Keynes for failing to provide a formal account of the connection between microeconomic decision problems and the aggregate level; he made it his life's work to develop a fully disaggregated model.

Brunner's position is much closer to that of Keynes than to that of the "Keynesians." His commitment to REMM as the best starting point for economic analysis naturally makes him sympathetic to Klein's microfoundational impulse; however, whatever the fundamental motivators of human behavior may be, he sees no reason to credit the *a priori* claim that the completely disaggregated model will prove to have the greatest cognitive or practical utility or, especially, that the derivability of aggregative models from microeconomic first principles guarantees their verisimilitude. Although Brunner accepts that formal derivations in tractable models will be useful for some problems, the narrow scope of rationality in such models, which falls short of the full potential of human resourcefulness, makes it unlikely that a comprehensive model of the economy could be built along those lines. "The [Keynesian macroeconomic modelers'] combination of Schmoller and Walras . . . disregards the middle range where analysis emerges in the form of explicitly constructed

empirical hypotheses with a definitely assessable form" (Brunner 1971, 169). As always, Brunner seeks a principled middle ground.

Ultimately, however, as an empiricist, Brunner argues that the level of aggregation must be judged empirically: truth transmits up. Brunner sees the desire for disaggregation as also proceeding in part on the respectable empirical hunch that accounting for distributional or, as he puts it, *allocative* processes will render the models more empirically stable (Brunner 1970, sec. 4). This empirical hunch is respectable, but Brunner argues that it is probably false. He suggests that the nature of human interactions and institutions is such that there is a rough separation between aggregative trends and allocative details. He explicates this idea with the suggestion that the forces shifting the means of the probability distributions that describe the economy are approximately independent of those that describe the realizations relative to the mean of the individuals governed by those distributions (Brunner 1970, 15). The aggregate demand for electricity, for example, may be reliably predicted from a small number of aggregate variables, whereas any individual's demand for electricity may be too idiosyncratic to be accurately predicted with even a large number of variables. Similarly, and more to the macroeconomic point, aggregate GDP and the aggregate price index may be reliably modeled using a few aggregate variables (e.g., the growth rate of money), but the place of an individual in the distribution of income is likely to be less accurately predictable. Brunner (1980, 311) goes out of his way to point out that the claim that a few aggregate forces dominate the movements of macroeconomic variables does not rule out second-order effects of allocative processes on the macroeconomic variables. Nonetheless, for most policy purposes, second-order effects can be neglected, and the dominance of aggregate over allocative effects justifies Brunner in his preference for small aggregative macro models.

Brunner's premise of the dominance of aggregate forces is ultimately an empirical premise and not by any means supported by his logical empiricism on its own. His appeal to logical empiricism on this issue is mainly to defang the argument that microfoundations and disaggregation represent an a priori, methodologically mandatory position. Brunner believes that there is some point at which further disaggregation would not stabilize the empirical behavior of an economic model (establish "permanencies" as he puts it) but would allow it to be swamped by kaleidoscopic changes in individual behavior and institutional structure (Brunner 1972b, 268). The main philosophical point is that *some* aggregate level can be a legitimate stopping point in the development of a cognitively satisfying economic

model and that there can be an appropriate level of aggregate macroeconomic theory that need not bow to microfoundational fundamentalism unsupported by empirical success.

## 12.6 Microfoundations and the New Classical Macroeconomics

As Brunner approached the end of his life, the Keynesian macroeconomic modelers, who had been his principal antagonists, were eclipsed by the rise of the so-called New Classical macroeconomics, associated most notably with Robert Lucas, Thomas Sargent, Finn Kydland, and Edward Prescott, which advocated a market-clearing approach and famously introduced John Muth's (1961) rational expectations hypothesis into macroeconomics (for a comprehensive account, see Hoover 1988). Those who see people's intellectual positions as governed by interests or ideology might find it odd that Brunner was unwilling to board the New Classical bandwagon. Just as he did, the New Classicalists trusted markets, favored limited government, and argued for stable policy rules, as opposed to the discretionary actions of technocrats and politicians. Brunner's erstwhile Keynesian opponents certainly saw little difference between them. Tobin branded the New Classicism as "monetarism mark II" (Tobin 1981, 35).<sup>12</sup> An important part of Brunner's dissent from new classicism derives from his philosophy of science.

Although Brunner appreciated the focus that Lucas brought—especially in his 1972 article in the *Journal of Economic Theory*—to the role of information in the macroeconomy and the importance of a price-theoretic approach, he saw New Classical aspirations as, in many respects, difficult to distinguish from those of the Keynesian macroeconomic modelers (Brunner 1980, 312). Although New Classical macroeconomic models were typically small—no larger than Brunner and Meltzer's own models in most cases and certainly not on the scale of hundreds of equations, as with the Brookings and MPS models—Brunner saw New Classical aspirations as being aligned with the Keynesian macroeconomic modelers in important ways: the New Classical macroeconomics typically adopts the fully disaggregated Walrasian model as the ideal theoretical type and embraces a microfoundational fundamentalism. Brunner rejected both as illicit appeals to a small set of a priori, "Cartesian" first principles. The microfoundational thesis, in Brunner's view, "seemed also to have convinced New Classical macroeconomists that all work need to start from 'first principles,' anything else being unacceptable. Such methodological isolation is a travesty of science" (Brunner 1984, 16; cf. Brunner 1989, 195).

Any superiority of New Classical microfoundational, market-clearing models must be empirically established as the end point of an evidential chain and must not be taken as self-evident, self-supporting premises.

It is unusual to see the New Classical economics as belonging to the same methodological approach as the Keynesian macroeconomic modelers, but it makes perfect sense within Brunner's philosophical framework. Lucas's (1976) "critique" was a sustained attack on the large-scale macroeconomic models and on their use in policy analysis. His targets were Klein and Tinbergen and their fellow travelers. The main argument begins with an observation that could easily have come from Brunner: the large macroeconomic models may forecast well in the hands of economists willing to constantly adjust their structure and replace counterintuitive predictions with their personal professional judgments but at the cost of undermining their theoretical consistency or utility for counterfactual policy analysis. Lucas, however, lays the problem at the door of aggregation. The difficulty with the macroeconomic models is that they do not take the economy apart at its joints and fail to capture the decision problems facing individuals.

The instability of the models is, in Lucas's view, the result of the changing constraints in the economy that change the patterns of individual behavior—with aggregate consequences. Beyond that, the failure to incorporate the rational expectations hypothesis into the model implies that individuals are treated as making systematic errors, which further undermine the stability of *estimated* aggregate relationships because real people are unlikely to persist in *systematic* errors over time.

The solution to the critique in Lucas's view is to provide adequate microfoundations. As he puts it in later work, economists must model the optimization problems of individuals in the economy, taking only "tastes and technology" as given, and ultimately must replace macroeconomics with microeconomics (Lucas 1980, esp. 702, 707; 1987, 107–108). Aggregate relationships under this strategy were supposed to seamlessly adapt to changing constraints and thus be more stable and less in need of subjective adjustment.

In some ways, the New Classical went less far down the road toward microfoundations than did the Keynesian macroeconomic modelers. New Classical empirical models were typically *more* aggregated, with the early New Classical models focusing on the incorporation of rational expectations. Even later New Classical models that more explicitly sought to represent decision problems typically did so using a single representative agent (sometimes several) who took GDP and its components

as direct objects of choice rather than attempting to model a very large number of agents. The main point was that the New Classicalists insisted on applying the formal optimization procedures of microeconomics to these aggregate problems.<sup>13</sup>

Initially, the New Classicalists did not object to the Cowles Commission's methodology of securing structural identification from a priori theory. The New Classical problem with the Keynesian models was not an objection to their econometric methodology. On the one hand, it was a suggestion that the methodology be elaborated, along the lines that the Keynesians should find acceptable, to incorporate the complications implied by the rational expectations hypothesis. On the other hand, it was the belief that the Keynesians had embraced the wrong theoretical model—a factual rather than methodological objection. Early New Classical models were subjected to standard econometric tests, mostly with less than happy outcomes.

Brunner challenged the standards of empirical evidence employed by the New Classicalists, just as he had done with respect to the Keynesian macromodelers, as failing to generate empirical tests that would permit comparative discrimination among competing theories. Lucas offered a simulacrum account of models: a model takes the form of an explicit computer program that mimics key characteristics of the economy, and the question of whether that model is true is irrelevant (Lucas 1980, 696, 709). Kydland and Prescott followed Lucas's lead, adopting his simulacrum approach with its focus on the ability of a model to mimic selected characteristics of the economy. They also advocated replacing econometric estimation with *calibration*—that is, with supplying parameter values indirectly based on other information, including microeconomic studies, stylized facts of national accounting, or simply personal beliefs—and judging the performance of a model by its subjective match to the characteristics of interest (e.g., Kydland and Prescott 1991; see Hoover 1995). Kydland and Prescott adopt a criterion of models mimicking data, but only on certain dimensions: "A model may mimic the cycle well but not perfectly" (Prescott 1983, 10). Structural macroeconomic modelers are faulted on the calibrationist view for insisting on evaluating models on their degree of match to too many dimensions of the data. The problem, as they see it, is that econometric estimation is too sensitive to uninteresting characteristics of the data: "Unlike the system-of-equations approach, the model economy which better fits the data is not the one used. Rather, currently established theory dictates which one is used" (Kydland and Prescott 1991, 174).

Kydland and Prescott's approach fails in Brunner's view on multiple grounds. First, in placing theory above evidence, it reverses the logic of scientific inference by implying that truth transmits downward. Some theory is assumed to be true, and Kydland and Prescott's methodology offers no method of logically challenging the assumed theory, essentially untethering it from empirical evidence.

Second, the point of theory, according to Brunner, is to provide a cognitive resource for understanding data, and, given the limitations of human knowledge and intelligence, such theoretical resources can develop only in a piecemeal and comparative manner. By anointing a "currently established theory" a priori, Kydland and Prescott essentially work from first principles—a move that Brunner rejects wherever he see it (Brunner 1989, 195). In doing so, they also abandon the notion that theories are to be comparatively evaluated and can be effectively elaborated only in a comparative and competitive context. Even when tempered to a criterion of mimicking on limited dimensions, mimicking is, in Brunner's view, a weak criterion. It is just too easy to generate incompatible models that mimic data (195). What is needed is the elaboration of competing theories so that the states of the world allowed under some models are forbidden by others. Then, discriminating tests are possible, and the standard of relative confirmation can guide the cognitive elaboration and the development of theory.

Although always an advocate of formal clarity and logical precision and although frequently a critic of Friedman and the Keynesians for failing in that regard, Brunner sees the New Classics as going too far—as overvaluing the formal derivation of models from optimization problems and taking tastes and technology as given (Lucas 1980, 702, 707. Once again, Brunner tars the New Classics and the Keynesians with the same brush. His point is threefold. First, like the Keynesians, the New Classics display a Cartesian attachment to first principles; therefore, despite references to data, they fail to employ a genuine empiricist methodology. Second, their methodology fails to recognize the logical theoretical hierarchy in which truth is established from the observational ground up and not from the theoretical top down.

Brunner's third point is a new one, although it is related to his hierarchical understanding of the scientific enterprise. Considerations of tractability lead New Classical economists to work with simple models in which the economist is actually able to solve the optimization problem. The tractability constraint encourages economists to ignore empirically relevant institutional facts, for which a derivation from utility and production

functions is not easily worked out, and reliable low-level empirical generalizations have yet to be given a satisfactory theoretical account. An example of such a reliable low-level generalization is, in Brunner's view, the fact of price stickiness: we do not, he believes, have an acceptable optimizing account of price stickiness, yet no macroeconomic model is likely to be empirically successful without acknowledging its reality. Brunner asks whether medicine should fail to use a drug for which there is reliable evidence of efficacy just because we have yet to give a theoretically satisfactory account of why it works (Brunner 1989, 205; 1991, 27).<sup>14</sup>

Given his threefold objection to the New Classical methodology, Brunner is pointed in deprecating the manner in which the New Classics insist on formal, microfoundational optimization models as the only acceptable form of theorizing. Yes, there is merit in formalization; but "legislating that whatever is not explicitly and rigorously formalized does not count and cannot possibly contribute any relevant knowledge" pays too high a price in relevance for a doubtful gain of rigor (Brunner 1984, 16). Wielding what he regards as misguided methodological principles as a cudgel is, in Brunner's view, the opposite of the genuine scientific method, which is to articulate theories in good faith and to allow them to be challenged in a competitive, comparative environment in which, ultimately, empirical success is decisive. The methodological strictures of the New Classics run the risk of dismissing genuine additions to knowledge simply because they do not arrive packaged in an a priori acceptable framework.<sup>15</sup>

## 12.7 Brunner's Philosophy of Science in Practice

It is fashionable to complain about the scientific status of economics—especially macroeconomics—and to worry that important questions never get resolved. There is a variety of views about why that might be the case. Mark Blaug (1980) diagnosed the problem caused by excessive attention to formal theory and, at best, lip service to empirical testing (see also Blaug 2003). Economists, he thought, were in principle Popperian falsificationists, but in practice they did not really subject their hypotheses to very stringent tests. Empirical testing, Blaug (1980, 256) suggested, was like "playing tennis with the net down." While not denying Blaug's thesis, Edwin Kuh laid the problem at the feet of the data—they were too weak to be discriminating (Brunner 1972b, 280). Brunner rejects both Kuh's claim about data and Blaug's claim about theory: "The fault lies largely with the nondiscriminating power of theories. No matter how



much data we have, if we can specify nothing about the theory, we cannot test very much" (281). Sciences progress, in Brunner's view, in the middle perspective—neither from extravagant elaboration of theory untethered to observation nor from an atheoretical empiricism. Progressive cognitive development always occurs, in his view, at the interface of observation and theorizing and always in a critical perspective. We must cultivate relevant theories and hypotheses about accessible data and let them compete with each other. Any field that uncouples its theorizing from observations, or protects particular theories or methodologies from empirical scrutiny, or fails to propose theories that might potentially advance beyond the first level of empirical generalization of the data to form more general explanations is a cognitive failure and hardly counts as a science at all.

Brunner was an optimist. He believed that a scientific economics was possible. Qua philosopher, he spent his life trying to articulate a sound methodology for such an economics. Qua macro- and monetary economist, he tried to implement that methodology in his own practice. Few other economists have so clearly articulated their philosophies of science, and fewer have tried to implement their philosophies so systematically in their own practice of economics.

## Notes

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1. Brunner's thought is more positively expressed than Keynes's (1936, 383) famous and analogous remark: "Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist."
2. In addition, Brunner published a number of articles that deal more or less squarely with issues in political and moral philosophy, including two articles that, for present purposes, offer an especially valuable mixture of the philosophy of science with political philosophy (Brunner 1972a; Brunner and Mechling 1977).
3. The contrast is stark: Friedman's 1953 essay is cited 666 times in JSTOR journals, whereas Brunner's 1969 article is cited 7 times, after eliminating self-citation; according to Google Scholar, Friedman's essay is cited 6,400 times and Brunner's 37 times (all data as of August 22, 2018).
4. *First-order predicate logic* comprises propositional logic and logic with quantification over nonlogical terms, which is sufficient for simpler mathematics; in contrast, *higher-order logics* allow quantification over logical terms and functions needed for higher mathematics, such as set theory.
5. Lakatos (1968) argues, in fact, that the idea of Popper as a "dogmatic falsificationist" is a caricature and that Popper's position has always been more nuanced and, by the end of the 1960s, quite close to Lakatos's own view.

6. Elsewhere, he shifts the metaphor, denying that REMM implies that man is "a dried up, shriveled homunculus, a dubious representation of reality" (Brunner 1987, 370).

7. Like Brunner, Mayo and Spanos require a genuine test to have well-defined null and alternative hypotheses. Whichever way the test points—to the null or the alternative hypothesis—it is considered severe only if the outcome would have been highly unlikely had the opposite hypothesis been true. See also Mayo (1996).

8. Brunner (1968, 779) refers to a planned paper, which appears to be one intended for a conference volume on econometric methodology (Brunner 1972b). Although Brunner edited the conference volume, his own paper was not included (Brunner 1972b). The volume does, however, include Jan Kmenta's "Summary Discussion" (pp. 262–284), which, along with Brunner's introduction to the volume, gives some idea of the positions that Brunner championed.

9. Brunner (1970, 16–18) brackets Andersen and Jordan's St. Louis-equation research with the earlier work of Friedman and Meiselman (1963) in the well-known AM/FM debate with Ando and Modigliani (1965). On the one hand, he defends the general logic of drawing theoretically compelling conclusions from reduced-form equations. On the other hand, he notes the need for some additional information and constraints to make that logic watertight. Similarly, with respect to Tobin's (1970) criticisms of the use of time-series evidence, Brunner points to the need to supplement money-income correlations with information about the money-supply process to draw conclusions about causal direction. But he points out that monetarists (e.g., Friedman and Schwartz [1963]) had, in fact, provided the relevant evidence.

10. Gustav Schmoller (1838–1917) was a leading member of the German historical school of economics, which opposed the marginalist economic theory of Carl Menger and the Austrian school, favoring detailed factual studies of economies without the scaffolding of higher-order theory.

11. The terms "macroeconomics" and "microeconomics" were coined by Ragnar Frisch (see Hoover 2012, 22). Frisch, along with the econometric model builder Jan Tinbergen and Klein, shaped the development of Keynesian empirical macroeconomics as much as, if not more than, Keynes himself did.

12. Hoover (1984; 1988, chap. 9) addresses the related puzzle of why Friedman, despite obvious commonalities, rejected the New Classical view.

13. I have argued elsewhere that this is the simulacrum of microfoundations and not the real article (Hoover 2001, 127).

14. In the years after Brunner's death, New Classical macroeconomists have more or less conceded his point with respect to price stickiness, although they persist in the practice of supporting their models from higher-level theory rather than from lower-level empirical evidence (for example, in embedding Calvo pricing, which derives sticky prices from the theoretical premises that lack any empirical support).

15. See Hoover (2015) for a similar argument.



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