

## ***Methodenstreit* 2013? Historical Perspective on the Contemporary Debate Over How to Reform Economics**

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**Abstract** The general failure of economists to predict the financial crash of 2008 has given rise to a wide-ranging debate over the need for methodological reform. But has this debate been adequate to the task at hand? We introduce a framework for classifying methodological debates according to their scope. The scope of debate is especially important in a time of economic crisis, when it is unclear what kind of disciplinary reforms are needed. We find that the current debate is confined largely to the *methodological* level, taking the incumbent *ontology* and *epistemology* as given. We contrast the current debate with two other moments of internal questioning in economics—the *Methodenstreit* of the 1880s and Keynes' innovations of the 1930s. These were more fundamental, ontological debates, and the contrast with the current debate indicates that reform in economics is likely to be minimal and slow in the wake of the crisis.

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## 1. INTRODUCTION

The general failure of economists to predict the financial crash of 2008 has given rise to a lively and wide-ranging debate over the state of the discipline and the need—if any—for significant reform. Krugman (2009), for example, in a widely cited *New York Times* article, wrote that “the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth,” indicating that the science should become less mathematical and more . . . something else. Sargent (2010) takes a contrary view, suggesting that “a rule of thumb is that the more dynamic, uncertain and ambiguous is the economic environment that you seek to model, the more you are going to have to roll up your sleeves, and learn and use some math.” Similar exchanges are taking place in the pages of journals and the halls of conferences around the world, as we show in detail below. Clearly, a period of serious debate and introspection is at hand. But how does this debate compare to historical precedent?

In its short history, economics has seen several episodes of methodological controversy: the *Methodenstreit* (debate over method) among German and Austrian social scientists in the 1880s, the period of Keynesian revolution in the 1930s, the “F-twist” debate in the 1960s over the importance of realism of assumptions and the “Cambridge controversy” over the meaning of capital in the 1960s and 1970s, to name some of the most notable. There is a sense in which each of these debates is idiosyncratic. Each was responding to the particular issues of the time and occurred within a specific period of the discipline’s development. In this sense, it is difficult to meaningfully “compare” debates across these diverse milieux. Nevertheless, they are in another sense bound in a common discourse about the possibilities and limitations of analytic social science. Concerns about the ability of state-of-the-art mathematical models to adequately capture the nature of economic experience, for example, were as much at the center of many of these past debates as they are at the center of today’s debate. The concerns about modeling assumptions drove the F-twist debate much as the concerns over the assumptions of dynamic stochastic general equilibrium (DSGE) models concern current economists. And just as the two Cambridges recognized the importance of forging a more concrete understanding of the nature of “capital,” current economists are grappling with the need to embed a more fine-grained depiction of capital market dynamics into the foundations of macroeconomics.

In comparing these debates, our focus will be on one particular facet—namely, the extent to which the debate involves a reconsideration of the ontological premises of incumbent scientific practice, that is, whether the discipline’s conception of the nature of its subject matter is on the table or whether the

incumbent ontology is taken as given and the debate takes place entirely within its conceptual vocabulary. We will refer to these different foci for debates as “levels,” e.g. a methodological debate with roots in the ontological level versus one occurring entirely at the methodological level or the epistemological level, or some combination. (These distinctions are discussed in more detail in Section 2.) Although the level of a debate is only one of its many facets, we believe that it is a potentially informative and important one. Especially in times of crisis, when it is unclear precisely what kind of disciplinary reforms (if any) are needed, it is helpful to be prepared to explore all aspects of the incumbent practice, including its ontological foundations. The question of whether the current situation requires ontological exploration is one that we cannot answer here. Indeed, it will likely only be decidable in retrospect. Also, it can be just as suboptimal to get mired in metaphysical discussions when they are not necessary as it can be to ignore them when they are. Nevertheless, it is useful in its own right to have a sense of just where the current debate is pitched. Moreover, these debates are central to the understanding of social economics as an alternative approach, as they focus precisely on the issues of description versus prediction, on the way in which agents (and economists) experience the social, on the inextricable link between ethics and economic analysis, and of the role of the economist herself in defining the nature and depth of scientific inquiry. Social economics is historically grounded analysis, and thus our emphasis on historical comparison is not just an academic exercise in the history of economic thought but an effort to come to terms with the social nature of our discipline.

Applying this lens to the current debate, we find that it is confined for the most part (though not entirely) to purely methodological considerations, taking the incumbent ontology and epistemology as given. To illustrate this, we review two historical instances of profound methodological debate for purposes of contrast: the *Methodenstreit* of the 1880s and Keynes’ innovation of the 1930s. The *Methodenstreit* provides a clear example of an instance in which a moment of ostensibly methodological crisis revolved around the deep, ontological concern with the nature of the individual and the relation between the individual and society. Keynes’ intervention provides an example of an instance in which ontological innovation (i.e. Keynes’ reconceptualization of the economic sphere) showed the way through a methodological crisis—a path that required economics to go beyond its incumbent paradigm and reconfigure its epistemology and methodology.

Taken together, these examples of current and past debate offer reason for both concern and hope. On the one hand, despite the seriousness of the recent failures of economics, the current debate demonstrates that crisis is not a sufficient condition for prompting profound debate. To the extent that reconsiderations at

the ontological level are necessary to address the shortcomings exposed by the recent crisis, this is a reason for concern. On the other hand, the example of the *Methodenstreit* demonstrates that crisis is not a necessary condition for debate either, and the example of Keynes' innovations demonstrates that crises can offer fertile ground for profound debates and fundamental reform. As we identify in Section 3, such probing voices do exist today, but they are currently being shunted to the sidelines. Our analysis suggests that it would be beneficial to give them more attention.

In Section 2, we present a framework for distinguishing different levels of debate. In Section 3, we review a selection of contributions to the current reform debate and characterize the competing positions in terms of the types of reform proposed. In Sections 4 and 5, we explore precedents of debates at the ontological level through the examples of Keynes' innovations and the *Methodenstreit*, respectively. Section 6 concludes.

## 2. THE LEVEL OF DEBATE

We would like to be able to make a judgment about the extent to which the current debate is foundational in nature—in other words, to assess the level of the debate. But what, exactly, does it mean to assess the “level” of a debate? There is by now a substantial body of work discussing distinctions between foundational aspects of a science and those aspects that sit atop the foundation—we will refer to these latter aspects as “higher-level.” Kuhn (1996), for example, drew the distinction between revolutionary change brought about by a shift in the paradigm of a science and normal scientific activity that takes place within a stable paradigm. In Lakatosian terms, a scientific research program is based on a stable core of ideas and practices, within which peripheral ideas and practices may be altered or sacrificed without the need for revolutionary change (Lakatos, 1978). More recent work in the sociology of scientific knowledge (SSK) has made the case that concepts like “paradigm” and “scientific research program” are likely to be vague at best. From the point of view of SSK, a scientific discipline is constructed day-by-day by the myriad activities of the community of scientists.<sup>1</sup> And while there may be a consensus view regarding what constitutes the fundamental aspects of the discipline, attempts to concretize that consensus will often be belied by the rough-and-tumble nature of practice on the front lines.

Our analysis presumes that there is a meaningful distinction to be drawn between foundational and higher-level aspects of scientific practice, but also recognizes that

this distinction is intersubjective and subject to local variations rather than objective and universal. To fix terms, we associate the “foundational” elements of a science with its ontology and epistemology—i.e. its conception of the make-up of its subject matter and its theory of what constitutes scientific knowledge (and relatedly, what is required to establish that a statement is proper scientific knowledge), respectively. The “higher level” we associate with a science’s methodology—i.e. its means of pursuing that knowledge. The ontological is the most foundational level, encompassing the practitioner’s conception of the nature of the economy itself—e.g. independently constituted individuals versus individuals inextricably bound in a mutually constituting complex of culture, institutions, and history. The epistemological level contains conceptions of what kind of knowledge is possible within the universe under study—e.g. algorithmic knowledge versus experiential knowledge, whereas the methodological level deals with the appropriate methods for producing the kind of knowledge possible within the universe under study—e.g. game theoretic analysis versus ethnography.

Of course, these three categories are interrelated (and not simply nested, i.e. with methodology being determined entirely by epistemology and epistemology entirely by ontology). But in both the current and historical debates in economics, those positions discussing reform at the level of methodology—only generally take the incumbent epistemology and ontology as given, whereas positions considering a reform of ontology are necessarily advocating more thoroughgoing change. For example, suggesting that one ought to employ a game theoretic analysis of financial interactions rather than a representative agent maximization analysis (i.e. a change in methodology) does not necessarily require reconsideration of the incumbent epistemology or ontology of economics.<sup>2</sup> On the other hand, suggesting that the concept of an independent individual is meaningless and that individuals can only be understood in relation to their socio-historical context (i.e. a change in ontology) would necessarily require a change in methodology. Similarly, suggesting that a true understanding of the nature and dynamics of economic life (i.e. knowledge of it) will always be inherently local and can only be generated by immersion in particular instances of economic life (i.e. a change in

<sup>1</sup> See, for example, Shapin (1995) for a general overview and Hands (2001) for an overview from the economist’s perspective.

<sup>2</sup> Spelling out precisely and in detail what is included in the incumbent ontology and epistemology of economics is beyond the scope of this paper. We rely in part, here, on the reader’s acquaintance with economic practice and the sense of the incumbent ontology and epistemology that she has gleaned from this acquaintance. In brief, however, we can point to the seminal accounts of these elements included in the works of (inter alia) Jevons (1871, 1874), Robbins (1984), and Samuelson (1947). Jevons and Robbins articulate a modular, mechanistic ontology of economic life—colorfully expressed by Robbins in the passage: “Here we are, sentient creatures with bundles of desires and aspirations, with masses of instinctive tendencies all urging us in different ways of action” (Robbins, 1984, pp. 12 and 13). And Jevons and Samuelson posit mathematical articulability as a necessary condition for a putative knowledge statement to pass the bar. For more on the ontological and epistemological implications of Jevons’ work, see Spiegel (2012). We thank an anonymous reviewer for pointing out the need for this clarification.

epistemology) would also necessarily require a change in methodology. As such, we classify debates directly engaging with ontology and/or epistemology as deeper than those pitched at a purely methodological level.<sup>3</sup>

Determining the level at which a debate is pitched is generally a matter of interpretation. A given debate may occur on more than one level, depending on its complexity; its explicit field of conflict may be resting on deeper issues that are only implicitly addressed by the participants themselves; and its participants may have different ideas about the level on which the debate is occurring. As such, we present our exploration of the level of the current methodological debate within economics in terms of a proposed interpretation rather than a discovery of truth. In what follows, our primary goal is to shine a light on the various contributions of the debate and provide a framework for assessing what the content of these contributions implies for the overall level of the debate. We will propose our own conclusions, but we do so in the spirit of beginning a discussion rather than providing a definitive account.

### 3. REFORMING ECONOMICS: THE POST-CRISIS CACOPHONY

In the aftermath of the general failure of economists to predict the financial crash of 2008, there was a widespread view that economics would have to undergo significant reform. This sense was so strong in part because the pre-crash period was not one of particularly sharp internal debate over theory. Writing on 12 August 2008, about a month before the collapse of Lehman Brothers, IMF Chief Economist and MIT professor Olivier Blanchard wrote the following:

For a long while after the explosion of macroeconomics in the 1970s, the field looked like a battlefield. Over time, however, largely because facts do not go away, a largely shared vision both of fluctuations and of methodology has emerged. Not everything is fine. Like all revolutions, this one has come with the destruction of some knowledge, and suffers from extremism and herding. None of this is deadly, however. The state of macro is good. (Blanchard, 2008)

Just a year later, things had changed so drastically in the economies of the industrialized world that another prominent macroeconomist, Willem Buiter, wrote about the need for a “new paradigm”:

<sup>3</sup> Note that we are not suggesting that all discussions of methodology are inherently “higher level” only, but rather that discussions of methodology that take the incumbent ontology and methodology as given are. It is also important to note the relationship of ontology, epistemology, and methodology to “theory.” Any economic theory will involve all of these elements—whether explicitly or implicitly. The question of whether a particular theoretical reform is higher level or foundational will depend upon the extent to which the reform engages with methodology only, or also with epistemology and/or ontology. See our discussion of Keynes’ innovations in Section 4.

Standard macroeconomic theory did not help foresee the crisis, nor has it helped understand it or craft solutions . . . [B]oth the New Classical and New Keynesian complete markets macroeconomic theories not only did not allow the key questions about insolvency and illiquidity to be answered. They did not allow such questions to be asked. A new paradigm is needed. (Buiter, 2009, p. 1)

Buiter was one of the first economists to publicly attack economic orthodoxy in the wake of the crash. But his intervention became part of a massive wave of articles, essays, letters, and blog posts, seeking to explain the reason that economic theory gave no hint of a coming economic collapse and to offer views on how economic theory should be reformed. Buiter’s views were not extreme: the severity of the crisis led to a sense that required reforms would be significant, and the ensuing debate offered extremely divergent perspectives on the needed reform.

Amidst the blinding array of articles, essays, books, blog posts, and blue ribbon panels analyzing the failure of economics, one can nevertheless place most of the responses into one of four categories, which we call (1) “Do nothing,” (2) “Add finance and stir,” (3) “Add complexity and institutions,” and (4) “Reconsider formalism.”<sup>4</sup> We discuss each of these categories below. Before doing so, however, it will be helpful as a starting point to describe a newly emerging “consensus” view within the discipline.

#### 3.1. The Emerging “Consensus” View

Our organization of the current debate into four categories of response is intended not only to bring some structure to the analysis of the reform of economics today, but also to give a sense of the variety of responses. This variety has not, in general, been reflected in the consensus view that has recently begun to emerge from this cacophony—essentially, the view that although the discipline of economics did not perform optimally, the remedies for any shortcomings are to be found within existing economic methodology. All that is needed in response to the crisis, this view contends, is a more robust application of certain aspects of the incumbent methodology—a kind of methodological “doubling down.” This view has been articulated more or less explicitly in several “blue ribbon” venues, including two letters from the British Academy to Queen Elizabeth in response to her question of

<sup>4</sup> There is a fifth category of response to the failure of economics that we will mention only briefly here: the capture thesis. Kapur (2009) and Epstein and Carrick-Hagenbarth (2010), as well as the film *Inside Job*, document that many economists were being paid large sums by financial firms and related business organizations and thus had a direct interest in certain policies and the economic models that support them. While we have no reason to doubt the veracity of the claims made in these papers, we leave this issue aside. We can assume, at least, that many of the participants in the current debate are not subject to capture by the financial sector.

why no one had predicted the financial crisis, two panels organized by the Allied Social Sciences Association (ASSA) at its 2009 and 2010 annual conferences, testimony before Congress by several leading economists, and a keynote address by Larry Summers to the 2011 Institute for New Economic Thinking (INET) conference at Bretton Woods.<sup>5</sup>

Although there is some diversity among these four sets of pronouncements, there is virtual unanimity (with one exception, discussed below) in the view that there is no need to look outside of economics' current toolbox for answers. The British Academy letters take the position that there was not enough of a culture of questioning within British academic and government economic circles, and that this needs to change in order to let the (essentially correct) incumbent methodology do its work. The ASSA panels and the congressional testimony essentially take the position that, to the extent anything ought to be done differently, economists should add new mathematical components to their models to capture currently under-modeled complexities in the world; for example, including the explicit modeling of deleveraging cycles and other feedback effects of bursting asset bubbles, and the intensification of the use of behavioral economics. Significantly, however, the suggestion is that the discipline need not consider new methods for incorporating the under-modeled complexity.<sup>6</sup> Summers echoes these views in his INET keynote address. Although he explicitly criticizes real business cycle and DSGE approaches to macroeconomics and praises the nuanced crisis theories of John Maynard Keynes, Hyman Minsky and Charles Kindleberger, his recommendation is to pursue such insights using the existing mathematical toolbox of mainstream (including behavioral) economics. When one looks carefully into the collection of individual responses to the crisis from the wider community of economists, however, one finds significantly more diversity than is suggested by the blue ribbon consensus. We organize these

voices into four broad categories according to the extent of their call for change. The first two categories—"Do nothing" and "Add finance and stir"—generally recapitulate the spirit of the blue ribbon consensus in counseling either no change or only minor methodological change. The other two categories—"Add complexity and institutions" and "Reconsider formalism"—go a bit farther, with some voices in the latter group even calling for profound reconsideration of current practice. It is in this group, and only in this group, that we find evidence of engagement at the ontological level.

### 3.2. Do Nothing

The "Do Nothing" view has been articulated largely in recent interviews with major economists, including Sargent (2010), Fama (Cassidy, 2010a) and Cochrane (Cassidy, 2010b). For this group, the dominant macroeconomic paradigm proved perfectly adequate for predicting and explaining the recent downturn. Contrary to the view that unexpected financial collapse caused the current recession, these economists point to natural frictions in the economy and market distortions caused by public policy. Cochrane, for example, comments that "[f]ight now ten percent of people are unemployed. Many of them could find a job tomorrow at Wal-Mart but it is not the right job for them . . . [S]ome component of unemployment is people searching for better fits after shifts that have to happen. The baseline shouldn't be that unemployment is always constant . . ." (Cassidy, 2010b). And Mulligan (2009), Cochrane's colleague at the University of Chicago Graduate School of Business, argues that the real business cycle model was highly successful in identifying the underlying causes of the current downturn. He writes the following:

When it came to this recession, the neoclassical decomposition quickly led me to look further at public policies—absent from some of the other recessions—that might have caused the supply of labor to shift relative to its demand. Like others, I noticed that the federal minimum wage was hiked three consecutive times. I also turned up a major policy (the Treasury and FDIC plans for modifying mortgages) that creates marginal income tax rates in excess of 100 percent. Much research remains to be done, and undoubtedly other users of the neoclassical growth model will make convincing cases for the roles of monetary and other factors. Paul Krugman's scorn is all we have to suggest that marginal tax rates in excess of 100 percent are not worthy of attention, and that today's low employment is not even partly a consequence of public policy.

For these economists, the role of the financial crisis has been overplayed relative to other factors that are well understood by current models.

But these economists do not simply ignore the financial crisis or claim that it was unimportant. On the contrary, they recognize its importance and argue that

5 For the British Academy letters, see Besley and Hennessy (2009, 2010); for the ASSA panels, see Allied Social Sciences Association (2009, 2010); for Colander's "Testimony," see Colander (2009); for Summers' remarks, see Summers (2011).

6 The one possible exception to this is the plea by Colander, Kirman, Leijonhufvud, and Meinring (2008) for agent-based computer simulation, and Colander's (2009) additional plea for a bifurcation of the discipline into mathematical analytic and interpretive branches, with more attention given in the future to training individuals in the latter group. These suggestions constitute searching outside of the current toolbox only to the extent that one considers the kind of mathematical modeling involved with agent-based simulations to be different in type from mathematical modeling (including computational simulation) methods that are already within the toolkit of mainstream economics. One possible way to evaluate this claim would be to consider whether the methodology of agent-based simulation *per se* (e.g. leaving aside the question of the quality of the argument and the status of the author) would make a paper's publication in one of the discipline's top generalist journals less likely. This is an empirical question, and one that we do not attempt to answer here. (For a judgment on this matter from within the mainstream, consider the comments of V. V. Chari at a recent congressional hearing on the crisis, expressing the opinion that agent-based modeling is perfectly compatible with DSGE methodology (U.S. House of Representatives, 2010, p. 58).) In any event, its answer does not affect the main themes or arguments of this paper.

while the traditional models may not have performed particularly well in predicting the crisis, this cannot be seen as an indictment of them because they were never meant to predict such things. Sargent, for example, argues that

[t]he criticism of real business cycle models and their close cousins, the so-called New Keynesian models, is misdirected and reflects a misunderstanding of the purpose for which those models were devised. These models were designed to describe aggregate economic fluctuations during normal times when markets can bring borrowers and lenders together in orderly ways, not during financial crises and market breakdowns. (Sargent, 2010)

But that does not mean that mainstream economics lacks models for the world as we actually encounter it. In fact, according to the Do Nothing group, mainstream economics is replete with such models. “Pretty much all [macroeconomists] have been doing for 30 years,” Cochrane (2009) writes, “is introducing flaws, frictions and new behaviors, especially new models of attitudes to risk, and comparing the resulting models, quantitatively, to data.” What is needed for an adequate understanding of the macroeconomy, in this view, is not new methods, but rather the skills and the fortitude to continue pushing the mathematical complexity that is necessary to refine the existing models. Replying specifically to Krugman’s charge (quoted at the beginning of this article) that economics’ overemphasis on mathematical modeling was a major factor in its recent failure, Cochrane asserts that

[t]he problem is that we don’t have *enough* math. Math in economics serves to keep the logic straight, to make sure that the “then” really does follow the “if,” which it so frequently does not if you just write prose. The challenge is how hard it is to write down explicit artificial economies with these ingredients, actually solve them, in order to see what makes them tick. Frictions are just bloody hard with the mathematical tools we have now. (Cochrane, 2011)

Thus, although there is a recognition that economists can do better at predicting and understanding financial crises and recessions, the remedy proposed by the Do Nothing group is a more intensive application of existing methodology rather than methodological reform.

### 3.3. Add Finance and Stir

Contrary to their Do Nothing colleagues, a substantial group of mainstream economists believe that the recent crisis has revealed inadequacies in existing methodology—most notably, the failure to adequately incorporate the financial

sector into our macroeconomic models. Paul Krugman, for example, has recently argued that “[u]ntil now the impact of dysfunctional finance hasn’t been at the core even of Keynesian economics. Clearly, that has to change . . . [Economists] have to do their best to incorporate the realities of finance into macroeconomics” (Krugman, 2009). How, precisely, to do this is a matter of some controversy. But the general sentiment that economic theory needs to incorporate the financial sector more effectively *somehow* is widespread. As such, we refer to this position as “Add Finance and Stir.”

A relatively tame version of this position advocates using mainstream methodology in new ways. For example, one could retain the existing framework of DSGE models, but simply make certain important aspects of the financial sector endogenous. This is the possible near future of macroeconomics envisioned by Morley (2010) in a recent posting on Brad DeLong’s blog, where he writes that “it is a safe bet that future versions of DSGE models will incorporate more complicated financial sectors and allow for different types of fiscal policies. And guess what? The new-and-improved DSGE models will turn out to imply (ex post) that the Great Recession was actually due to serially correlated financial intermediation shocks and suboptimal fiscal policy.”<sup>7</sup> Acemoglu (2009) makes a proposal in a similar vein. He has argued that the overvaluation of the “reputation capital” of firms has led to an inability of economic models to detect overly risky behavior by firms. (Clearly, he has financial firms in mind.) His proposed remedy is to simply incorporate a mathematical representation of reputation capital into our models, with the attendant concepts of investment in and returns to that capital allowing us to judge when this element is being treated efficiently by market participants. Along similar lines, Bernanke (2010) has argued that the core methodology of macroeconomics is sound, but that “understanding the relationship between financial and economic stability in a macroeconomic context is a critical unfinished task for researchers.” To accomplish this, he counsels building on existing work using the current methodological toolkit. Such proposals are pitched at the level of methodology: they do not propose that economists reconsider the nature of the economic universe or its epistemic possibilities, only that they reform their existing tools to illuminate certain elements of that universe more effectively.

A stronger version of “Add Finance and Stir” calls not only for incorporation of finance into macro models, but also for a reform of the manner in which we model finance. Included in this approach are those who focus specifically on the efficient market hypothesis—the model of financial markets adopted by most macro models—with a subset of this group explicitly positing the abandonment of this hypothesis as

<sup>7</sup> Morley sees this as an undesirable outcome. He concludes his statement ruefully: “Alas, these conclusions will be driven much more by the DSGE framework than by the data . . .”

crucial to the reform of economics. The post-Keynesian movement, in particular, has been associated with calls for overturning the dominant paradigm. In assessing the level of post-Keynesian contributions to the debate, however, it is important to distinguish between the different strains of this work. In our view, many of these contributions have been pitched at the methodological level, insisting mainly on the need to add “financialization” into otherwise relatively standard post-Keynesian and Kaleckian modeling (for a review of the flurry of literature, see Stockhammer and Onaran (2012)). This is true even of many of the contributions promoting Minsky’s notion of financial fragility because the main prescription of these proposals is that we understand finance as an endogenous driver of cycles, which does not in itself require a fundamental rethinking of the way economics is done or that economic actors are understood to behave.

In contrast to these contributions, there has been a contingent of economists promoting a revival of some of the key concepts of traditional Keynesianism—most prominently that of the importance of fundamental uncertainty in macroeconomic dynamics. These contributions have come mostly in the form of new books on Keynes (e.g. Davidson, 2009; Eatwell & Milgate, 2011; Skidelsky, 2009; Taylor, 2010). Akerlof and Shiller (2009) also hearken back to Keynes in emphasizing that irrationality—they adopt Keynes’ term “animal spirits”—rather than rationality may drive the psychology of markets, including financial markets, and that economics must integrate this insight into its models. The level at which these contributions are pitched depends on how seriously they take their Keynesian elements. All of them have methodological aspects—counseling structuralist or behavioral economics methods—but some of these calls for methodological reform are based on, or have implications for, epistemological and/or ontological issues. Those counseling a greater focus on behavioral finance methods are based on the claim (explicit or implicit) that models of perfect rationality misconstrue the reasoning capacities of economic actors and, therefore, are pitched at the epistemological level.<sup>8</sup> But behavioral finance methods do not fundamentally challenge the incumbent ontology. They envision changes in the mathematical specification of the decision-maker’s environment, not a reconceptualization of it that includes new entities.<sup>9</sup> Those contributions calling for a return to Keynes’ theories (i.e. as they were articulated in the *General Theory*; and not in their New Keynesian interpretation) of the role

of the financial sector in the macroeconomy are more radical, touching on the ontological level. This is the case because such contributions require the introduction of, *inter alia*, a concept of radical uncertainty that is entirely absent from mainstream macroeconomics in its current form. (See Section 4 for more on this.)

The “Add Finance and Stir” position has been quite prominent in the current debate and is likely to remain so, primarily for two reasons. First, it is championed by a number of high-profile economists, including Nobel laureates and other scholars holding prestigious positions inside and outside of academia. Second, at least with respect to some of the more prominent contributions, it dovetails nicely with reforms already underway within economics—most notably the rise of behavioral finance. As such, it does not necessarily require substantial deviation from trends in current practice.

### 3.4. Add Complexity and Institutions

In addition to the voices counseling no change or only minor change, there are a number of calls for a more thoroughgoing reform of economic methodology. One finds such appeals around the issue of the complexity of existing models, and, specifically, the urgent need to reconstitute our methods to accommodate such complexity. The spirit of these appeals gestures toward the ontological level, suggesting that current models do not adequately capture the complexity of economic reality. They could provide an entrée into a consideration of how precisely we should be conceiving of the economic reality, although not all in this group engage at that level.

Caballero (2010) provides an example of a contribution that does not reconsider the incumbent ontology. He begins by arguing that current macro models are not nuanced enough to comprehend the web of interdependencies that transform individual actions into aggregate economic activity. “One of the core weaknesses of the core [mainstream macro models],” he writes

stems from going too directly from statements about individuals to statements about the aggregate, where the main difference between the two comes from stylized aggregate constraints and trivial interactions, rather than from the richness and unpredictability of the linkages among the parts.

But while Caballero’s view could be the basis for a critique of the incumbent ontology, he counsels only that we explore the elements of the standard ontology more deeply, concluding that “[w]e need to spend much more time modeling and understanding the topology of linkages among agents, markets, institutions, and countries” (Caballero, 2010, p. 9).

In contrast, some calls for the recognition of greater complexity engage directly with at least some features of the incumbent ontology. The body of

<sup>8</sup> One could argue that behavioral economics challenged the incumbent ontology by denying the existence of perfectly rational beings and positing the existence of imperfectly rational beings (or, alternatively, rational beings with inherent limitations in their ability to perfectly employ their reason). We would argue, however, that this represents a change of degree rather than the introduction of a new type.

<sup>9</sup> For example, individuals engaging in herd behavior are still standard utility maximizers based on their beliefs, as are individuals engaged in loss-averse behavior, those with status-quo bias and those engaging in hyperbolic discounting. No new concepts of information, preferences, exchange, markets, or the like are required.

literature promoting agent-based computational economic (ACE) modeling is one example. This approach eschews both the representative agent and the equilibrium aspects of much DSGE modeling in favor of a computational approach that defines characteristics of various types of agents and simulates the dynamics of their interactions under various conditions. It takes its inspiration not from economic modeling, but from recent developments in physics and biology (see, for example, Bouchaud, 2008, p. 292; Colander et al., 2008; Keen, 2009, p. 5; Kirman, 2009; LeBaron & Tesfatsion, 2008). Agent-based models are complex mathematical models—often with hundreds of differential equations. In fact, the models' primary distinctive feature is that they cannot be solved but can only be simulated in an effort to understand their properties. Heterogeneous individual agents are not assumed to behave optimally. Instead, agents may behave according to specified rules, and these may be connected to both behavior of other agents and to macro trends that emerge out of the behavior of all agents.<sup>10</sup>

This can be seen as proposing a method based on an alternative to the incumbent ontology to the extent that one views the idea of an equilibrium economy and the possibility of a representative agent (or at least an economy that functions as if this were possible) as a part of that ontology. One could argue, however, that we should view these aspects of current mainstream macroeconomics as methodological simplifications driven by epistemological constraints—i.e. the view that DSGE methods are best suited for producing the kind of knowledge we can actually achieve, and that the kind of fine-grained knowledge ostensibly supplied by ACE is not possible. In this latter case, promotion of ACE over DSGE would entail a conflict at the level of epistemology and methodology rather than ontology. At the very least, though, the promotion of ACE raises questions about economic ontology, even if it does not *necessarily* entail a challenge to the incumbent ontology. And in this sense, it is a contribution that touches on the ontological level.

One element of the incumbent ontology that the proponents of ACE do not challenge is the view of the social world as inherently and (conceptually speaking) unproblematically representable by mathematical models.<sup>11</sup> This ontological commitment stems from the discipline's methodological commitment to mathematical

modeling as its core explanatory method. Although ACE proponents do believe that the incumbent ontology fails to recognize certain types of social complexity, they do not believe that this complexity can have other-than-mathematical structure. Colander (2009), for example, counsels a mathematical doubling-down in the face of this currently untheorized complexity. "Inevitably," he writes,

complex systems exhibit path dependence, nested systems, multiple speed variables, sensitive dependence on initial conditions, and other non-linear dynamical properties. This means that at any moment in time, right when you thought you had a result, all hell can break loose. Formally studying complex systems requires rigorous training in the cutting edge of mathematics and statistics. It's not for neophytes. (Colander, 2009)<sup>12</sup>

In contrast, some scholars calling for the incorporation of increased complexity have argued that the discipline's commitment to formal methodology itself should also be up for reconsideration.<sup>13</sup> It is to this final group that we now turn.

### 3.5. Reconsider Formalism

From the diverse perspectives of old-style institutionalism, critical realism and some post-Keynesianism, a small group of economists has argued not only that rigid adherence to orthodoxy in the lead-up to the crisis masked deeper complexities but also, further, that it is incumbent upon us at least to consider that an a priori commitment to methodological individualism and mathematical modeling may be more an obstacle than a conduit to empirical understanding. As with Caballero (2010) and Colander et al. (2009), these voices indict rigid adherence to orthodox models as dangerous and untenable. Hodgson (2009), for example, suggests that the dogmatic adherence to belief in the efficiency of markets led economists to ignore warnings of a coming crisis: "When economists believe in the informational efficiency of markets and their self-correcting capacity, then warnings of collapse are disregarded because they go against the conventional wisdom." And Leijonhufvud (2009) notes that "the repeated

10 See Farmer and Foley (2009) for a discussion of the relevance of agent-based models to the post-crisis world.

11 One might object that a commitment to using mathematical models does not necessarily entail the ontological commitment that these models accurately represent the nature of their subject matter. Indeed, the rejection of this view is at the heart of Milton Friedman's seminal 1953 essay "The Methodology of Positive Economics." Many subsequent commentators have argued, however, that the purely instrumentalist interpretation of models that Friedman argues for is inherently problematic and that the actual use of economic models requires a representational relationship of some kind between model and subject. As Hands (2001, p. 57n32) points out, "[t]his creates a rather quizzical situation where many, perhaps even most, practicing economists endorse Friedman's view (at least in a pro forma way), while almost all of the commentary written on the paper is quite critical." See Hausman (1992, p. 163n17) for a partial listing of the voluminous critical literature. We thank an anonymous reviewer for requesting clarification on this point.

12 This statement is from Professor Colander's written testimony before Congress, to which the paper Colander et al. (2009) was appended.

13 Colander et al. (2009) assert that the kind of mathematical modeling they have in mind (e.g. ACE) need not be highly formal in nature. In our view, this is a mistake. Mathematics may be used informally—e.g. if it is used merely as a heuristic, or if its terms are used loosely, or, more radically, if it is presented as a piece of visual art. However, mathematical objects *qua* mathematical objects are inherently formal. This is as true of Arrow and Debreu's topology as it is of, say, Acemoglu and Robinson's extensive form games, even though the latter is used in a more informal way—i.e. to make broad, sometimes vague claims about actual politics. As such, we see ACE as a formal analytic methodology, although one that uses a different formal approach from current mainstream economic modeling methods like DSGE. See also Lawson (2009).



occurrence of financial crashes or crises hardly seems consistent with intertemporal equilibrium theory.”

Unlike Caballero (2010) and Colander et al. (2009), however, these writers urge us to consider letting our choice of methodologies be guided by a nuanced understanding of empirical reality, regardless of what methodologies are best suited to this. Hodgson (2009, p. 1218) sees the unseating of mathematics as the prime concern in moving to “a discipline more oriented to understanding real-world institutions and actors.” To achieve this, he argues that “[t]here must be an end to the use of mathematics as ‘an end in itself’ and to dogmatic teaching styles that leave no place for critical and reflective thought.” Elsewhere, he writes that “[t]he pressing question now is whether the financial crisis of 2008, which is the most severe crisis since the Great Depression, will reverse this fascination with mathematical technique over real-world substance” (Hodgson, 2008, p. 276). He adds that

[o]ne likely reaction to the current downturn is that we should try harder to develop better models. Perhaps we should. But we must also learn the vital lesson that models on their own are never enough. Economists need to appreciate the limitations of modeling. These limitations are generic and result from the intractabilities of uncertainty, complexity and system openness in the real world.

Tony Lawson has been equally outspoken on this issue. In Lawson (2009), he argues that the problem

is not so much the use of specific inappropriate models, but the emphasis on mathematical deductivist modeling per se. Such models can provide limited insight at best into the workings of the economy (or any other part of social reality). Indeed, I will suggest that the formalistic modeling endeavor mostly gets in the way of understanding. (Lawson, 2009, p. 760)

Lawson’s opposition to mathematical formalization is rooted in his particular version of realism—namely that mathematics imposes a closed-system ontology that does not reflect the reality of economic life, since “the nature and conditions of social reality are such that the forms of mathematical deductivist reasoning favoured by economists are almost entirely inadequate as tools of insightful social analysis.” He calls for a “more grounded framework” to better understand this “open, structured, totality in motion” (p. 763).

The calls for reconsidering the discipline’s commitment to formal methodology have generally come from outside of the mainstream, but there have been a small number of similar pleas from inside it as well.<sup>14</sup> Two of the

most notable examples are George Akerlof’s recent appeal for more “fine-grained” (read: qualitative/interpretive) methods in economics (Akerlof, 2011)—to ensure that our models are actually capable of representing the phenomena we claim to explain—and Krugman’s (2009) recent criticism of economics’ a priori commitment to mathematical modeling. Such contributions from inside the mainstream, however, are exceptional. Judging by their absence in the various articulations of the Blue Ribbon consensus, it is apparent that they are currently only fringe contributions.<sup>15</sup>

#### 4. HISTORICAL PERSPECTIVE ON THE CURRENT DEBATE I: KEYNES

How does the current debate over the problems with economics compare to past debates? In the history of economics, there have been several methodological debates pitched at the level of ontology. Of these debates, the one that most obviously parallels today’s rethinking of economics is that engendered by Keynes’ innovations in the wake of the Great Depression. Like today’s economists, Keynes was struggling to understand an economic collapse whose depth and scope had been inadequately anticipated by the discipline. His contributions provoked an intense debate and led to significant innovations in both economic methodology and policy-making. But as we will argue later, it was his ontological interventions that were the ultimate drivers of this change. At the level of the individual, he offered a new conception of the nature of uncertainty and of economic agents’ response to it. At the level of the macroeconomy, he insisted that the monetary and real sides of the economy were inextricably intertwined and thus could not be analyzed separately.

<sup>14</sup> By “outside of the mainstream” we mean, roughly, that the approach to economics taken by these contributors is unlikely to be represented in the discipline’s top generalist journals.

<sup>15</sup> One additional contribution that does not fit easily into our taxonomy should be mentioned here. In several venues, Colander has called for a kind of division of labor within economics—with one branch engaging in highly complex mathematical basic research, and another branch engaging in interpretation of this research for policy purposes. The call for this latter, interpretive work sounds in some ways like Hodgson and Akerlof’s plea for a more fine-grained economics. This interpretation is supported by Colander’s statements in support of methodological pluralism. However, in our view, this position does not constitute a reconsideration of the incumbent paradigm for two reasons. First, Colander’s division of labor still insists upon mathematical analysis as the necessary core of economics. Any interpretive work is a posteriori and therefore does not enter into the formulation and execution of the basic research. Second, contrary to Colander, and as previously mentioned, we do not find the concept of informal mathematics to be meaningful (see note 10). As such, the economics that Colander is envisioning maintains, in our view, an a priori commitment to mathematical methodology as the engine of discovery, which, in turn, rests easily on the incumbent ontology. This is not a criticism of Colander’s proposals. Indeed, we applaud his calls for pluralism, for improvement of the peer-review process and for additional attention to the applications of economic research. Our comment here is simply meant to clarify the extent to which his proposals engage critically with the incumbent ontology.

which was the Keynesian adjustment factor bringing saving into line with investment, but normally at a level of income that did not support full employment.

This dispute between Hayek and Keynes cashes out in different theoretical representations of the functioning of the economy, but the roots of the theoretical distinctions lie in their differing ontologies. Keynes' liquidity preference theory and his depiction of the capital market equilibration mechanism as being driven by income rather than the interest rate grew explicitly out of his inclusion of radical uncertainty as a constitutive part of the economic universe. What is important to recognize is that Hayek's theory is not only different from Keynes' theory in certain important respects, it is actually incompatible with Keynes' economic ontology. As such, the real locus of their disagreement was at the level of ontology, and any reconciliation of their views would have required direct engagement at this level. The same is true of the clashes between Keynesian theory and that of Pigou and the New Keynesians, respectively, to which we now turn.

#### 4.2. Keynes versus Pigou

Pigou and most of his contemporaries saw the absence of wage flexibility as the greatest obstacle to solving the unemployment problem. Keynes attacked this view head on in the first two chapters of *The General Theory*, arguing (1) that workers bargain over the money wage not the real wage and thus have incomplete control over the real wage. It is the real wage which is the equilibrating price in the "classical" conception, and (2) that downward wage flexibility could even worsen the unemployment problem, since it would send a signal to businesses of declining demand and thus a reduced need for investment spending going forward.

Again, as with the debate with Hayek, Keynes' disagreement with Pigou was not simply a technical one about the nature of market adjustment. Ultimately, it was concerned with the very conception of a capitalist economy. Pigou depicted the labor market as by nature a stable equilibrium system on top of which society might impose complications, distortions, and obstacles, such as the downward stickiness of wages. Removing these complications, in this view, would lead to a natural and efficient—full employment—outcome. For Keynes, the social and monetary dimensions of the wage bargain could not be separated from the workings of the system, and so Pigou's conclusion was based on a fundamental misconception of the nature of the labor market. Understanding the market as Keynes did—i.e. as an inextricable complex of social, monetary and real

dimensions—led him to conclude that equilibrium unemployment was not a market failure that could be cured by making the market more pure, but rather a possible natural outcome of market dynamics in the face of radical uncertainty. For him, Pigou's attempts to analytically separate the real and monetary sides of the economy were Panglossian rather than clarifying, as they entirely missed the nature of capitalism as a "monetary production system."

Keynes' attack on the "classical" postulates was essential to his theory of effective demand. Keynes identified precursors to his view that demand determined the level of output (Malthus) and money played a prominent role in economic activity (mercantilists), but "Keynes was the first leading modern economist to focus analytical attention on the level of demand, or spending, as the determinant of the level of activity" (Skidelsky, 1994, pp. 544 and 545). This rejection of Say's Law had, again, deep implications about the nature of economic life, according to which investment does not require prior saving and in fact the causality is reversed because of the dependence of saving on income. Shapiro (1977) brings out the radical nature of Keynes' departure from the classical conception of causality, since it is not rooted in the demands of scarcity. She writes the following:

[T]he differences between post-Keynesian and neoclassical economics are not so much differences in their subject matter as they are differences in their treatment of economic life. The neoclassicists' concern with [the problem of scarcity] is an expression of their view of the economic process as the adjustment of resources to the given needs of individuals, that is, 'the allocation of scarce resources among competing ends.' The problem of scarcity is absent in post-Keynesian economics precisely because this view is absent. (Shapiro, 1977, p. 552)

#### 4.3. Keynesians versus Post-Keynesians

Many of the fundamental issues raised in the 1930s were swept aside in the initial moments of debate, and only somewhat later would the depth of the differences between Keynes and the reigning orthodoxy become clear. (This lag in the treatment of fundamental differences may be one of the lessons for today of our historical case studies of major moments of debate over the future of economic theory.) According to Moggridge,

after some initial discussions of the 1930s and 1940s, most of th[e] interpretive literature [on *The General Theory*], at least in book-length form, dates from after 1961. It followed a period of over twenty years of professional agreement as to what the General Theory was essentially about. (1992, p. 557)

The more radical aspects of Keynes' rethinking of economic agency and thus of the nature of capitalism are largely absent from the "Keynesianism" that emerged out of Hicks' 1937 interpretation of the General Theory in his *Econometrica* article "Mr. Keynes and the 'Classics'." In that article, Hicks presented the now-famous IS-LM model, which attempted to capture the theory of effective demand in a four-equation representation of simultaneous equilibria in the goods market and in the money market. The model was, by Hicks' own admission, a simplifying formalization of Keynes' theory of unemployment, but it had almost universal acceptance from the economics profession, and for almost 40 years was the main textbook representation of Keynesianism.

Although Keynes himself did not articulate dissatisfaction with the IS-LM model, Joan Robinson and the small but vocal group of post-Keynesians that followed her referred to Hicks' approach as "bastard Keynesianism" because they claimed that it left out many of Keynes' fundamental insights about capitalism, especially its emphasis on uncertainty and liquidity preference. Capturing this distinction, Coddington (1983) identified two types of Keynesians: fundamentalist and hydraulic. The former type includes the post-Keynesians, such as Sidney Weintraub, Paul Davidson, Jan Kregel, Victoria Chick, and others, who insisted on the importance of uncertainty and expectations and the short-run nature of economic equilibria. The latter includes the mainstream Keynesians, from Paul Samuelson and Robert Solow to Joseph Stiglitz and Gregory Mankiw, who embraced the IS-LM model and its conclusion that monetary and fiscal policy can be more or less effective in raising aggregate demand depending on the slopes of the curves. The post-Keynesians argued that the IS-LM model not only lacked essential elements of Keynes' theory, such as a theory of the price level and a clear connection to nominal wage formation, but also that it failed to capture Keynes' insistence on the importance of fundamental uncertainty (Weintraub, 1977). The implication of this somewhat technical attack was that the IS-LM model was at odds with Keynes' approach to understanding capitalism as a monetary production economy whose movements had to be understood in historical time rather than in the logical time emphasized by the simultaneous equilibrium in the goods and money markets in IS-LM. The post-Keynesian view that outcomes in the short run relied heavily on expectations formation under uncertainty was absent from the Hicks-Samuelson neoclassical synthesis, and thus, the post-Keynesians argued, so was Keynes' rich notion of individual agency and economic change. According to Lodewijks (2003, pp. 28 and 29):

[T]he "Economics of Keynes" cannot be analysed in timeless, perfect information, general equilibrium models. A world of fundamental uncertainty moving through

historical time is essential to the message of Keynes. Interpreting Keynes through IS-LM is a distortion that forces the General Theory in the older neoclassical mould."

Kregel (1976) reinforces this point, pressing the significance of the ontological divide between the methodology of the Keynesians and the post-Keynesians, writing that

one does not "name" the problems of the real world by creating and analyzing a world in which they are absent, and then searching for the minimum conditions for the existence of such a world. Rather one attempts to make an ordering of the categories of the real world that are the object of analysis . . . Keynes argued that his approach could not assume perfect foresight and full information, for under such an assumption his main theoretical contribution, the theory of effective demand, had no meaning. (Kregel, 1976, p. 222)

#### 4.4. Summing up the Keynesian Debate

In the well-known, one-page first chapter of *The General Theory*, Keynes makes the traditional scientific case for the merits of his model: that the existing view is a special case of his more general model. This criterion is borrowed from mathematics, or logic generally, and while Keynes makes a case for the empirical relevance of his perspective compared with others, it is clear that in the epistemological realm he is intentionally following the very traditional criteria of inductivism. But Keynes' major contribution was at the ontological level, regarding the nature of uncertainty and expectations and the very conception of capitalism (as a monetary production system) and markets (as inextricable complexes of social, psychological, institutional, and "real" economic factors). Consequently, the debate engendered by these contributions could not have been on the level of methodology alone. This is apparent not only in the debates with Hayek and Pigou—in which the nature of fundamental objects of the economic universe was at issue—but also in the hostile response of the post-Keynesians to attempts to reduce Keynes' insights to IS-LM analysis.

### 5. HISTORICAL PERSPECTIVE ON THE CURRENT DEBATE II: THE *METHODENSTREIT*

Although the milieu of late nineteenth-century Germany and Austria provides a less direct historical parallel to today's debate than the post-Depression Anglo-American world, that era's debate over social science methodology—the *Methodenstreit*—is still a very useful case study in the distinction between deep and shallow debate. Specifically, the *Methodenstreit* demonstrates the importance

of recognizing the relationship between ontology and methodology, and the potential dangers of attempting methodological reforms in the absence of such recognition.

Broadly speaking, the *Methodenstreit* consisted of a clash between two schools of economic thought in German-language academic economics<sup>17</sup> in the late nineteenth century: the new abstract-deductivist school led by Carl Menger and the then-dominant concrete-inductivist German Historical School (GHS) led by Gustav von Schmoller.<sup>18</sup> The origins of the dispute lay in the negative reception of Menger's *Principles of Economics* (1976 [1871]) in Germany. Only three of the four major German economics journals published reviews of the work, and these were for the most part unfavorable and dismissive. The fourth major journal—Schmoller's own—failed to review the work at all.<sup>19</sup> Outside Germany, the reception of Menger's ideas was somewhat better. He was able to secure himself a lectureship at the University of Vienna on the strength of the *Principles* and was soon after elevated to the rank of *professor extraordinarius* (Hayek, 1934).<sup>20</sup> But Germany was the center of academic economics in the German-speaking world, and the hostility of the GHS to Menger's ideas was both a significant obstacle to their wider propagation and (judging from the vehemence of Menger's critique of the GHS in Menger (2009 [1883]) a source of considerable personal irritation.

The negative-/non-response to the *Principles* in Germany prompted Menger to take the unusual (and unwanted) step of explicitly defending his methodology against that of the GHS. The result was the methodological *magnum opus* of the *Methodenstreit: Investigations into the Methodology of the Social Sciences with Special Reference to Economics* (2009 [1883]). In the preface to the work, Menger related his reluctance to take what he considered to be a detour into methodological debate, ultimately concluding, however, that the methodological problems of the GHS were choking off progress in German-language economics and had to be addressed head-on (Menger, 2009 [1883], p. 27). What followed in the *Investigations* was a detailed and wide-ranging argument in favor of securing a place for abstract-deductive methods in political economy. It was only these methods, Menger argued, that could educe the fundamental underlying truths of political economy. The methods of the GHS were not up to the task.

17 As the participants in the *Methodenstreit* are from Germany and Austria, we will use the term "German-language economics" when referring to the realm within which the debate took place.

18 The seminal figures of the German Historical School were Wilhelm Roscher (1817–1894), Karl Knies (1821–1898), and Bruno Hildebrand (1812–1878). In 1871, however, when Menger's *Principles* was published, it was Schmoller (1838–1917) who was the school's recognized leader.

19 See Bostaph (1978, p. 139) and Tribe (2007, p. 74).

20 The rank of *professor extraordinarius* lies between that of lecturer (*Privatdozent*) and full professor. Unlike full professors, extraordinary professors generally would not have had a guaranteed salary in late nineteenth-century Germany and Austria.

Schmoller responded swiftly and directly to the *Investigations*, writing a sharp negative review (von Schmoller, 1883) for publication in his *Jahrbuch für Gesetzgebung, Verwaltung und Volkswirtschaft* (which had declined to review the *Principles*). This might have been the beginning of a lively, open debate—though Schmoller's review was, in Hayek's words, "a magisterial rebuke . . . couched in a tone more than usually offensive" (Hayek, 1934, p. 407); it nonetheless signaled some level of engagement with the issues raised in the *Investigations*. As it turned out, however, this was the last time that Schmoller would engage so directly. Menger quickly published a pamphlet (Menger, 1884) in response to Schmoller's rebuke that mostly reiterated the points made in the *Investigations*, but Schmoller refused to review it, publishing only his letter of refusal in his journal in place of a review. When Schmoller took the additional step of using his influence to effectively ban all adherents of the new Austrian School from German academia, the main action of the *Methodenstreit* was brought to an abrupt close (Bostaph, 1978, p. 6). While the next generation of GHS and Austrian School economists continued to develop their respective theories, they would never again engage in direct debate in the manner that Schmoller and Menger had.

On its surface, the *Methodenstreit* was a debate over methodology. But fundamentally, it was a clash at the level of ontology, as the methodological positions of the participants were driven by their ontological commitments. Menger believed that the social world was a collection of generic types, that this assumption was not up for empirical assessment, and that we could proceed directly (i.e. without any direct experience of any particular manifestation of a given type) to formal analysis of the causal relationships between these types (Menger, 2009 [1883], pp. 63 and 217–219). One consequence of this was that one need not know anything about the particulars of a situation to analyze it. Accordingly, his method was abstract, axiomatic, and deductive. The GHS also believed in a structured social universe, though not one with trans-historical individual-level types as its basic elements. Furthermore, they believed that one could only access the structure of the social world inductively, via the particulars of any individual event. Accordingly, their method was concrete and inductive.

A debate solely on the level of methodology, then, was destined to be barren—Menger's methodology is clearly inappropriate from within the GHS ontology and vice versa. Unfortunately, this is largely the debate in which Menger and the GHS engaged. The charges against the GHS in the *Investigations* are couched almost entirely in methodological (and, in some cases, epistemological) terms. Although Menger does explicitly reveal his ontological commitments, he presents them as self-evident truths for which no further explanation is needed or offered.<sup>21</sup>

21 See, for example, Menger (2009 [1883], pp. 60–63).

Schmoller's response to the *Investigations* does little better at constructively addressing the ontological gulf that lies at the core of the disagreement. Although he did in many places object to Menger's methodological criticisms at the level of ontology, he did so only by asserting the wrongness of Menger's world view and the rightness of his own. Consequently, the meaning of the *Methodenstreit* was left to be assessed on the basis of the essential non-engagement of the participants with the major issues at hand.<sup>22</sup> The fact that twentieth-century economics ultimately moved squarely in the direction of abstract, marginal analysis, and away from the methodology favored by the GHS has left the unfortunate and inaccurate impression that Menger "won" the debate and relegated the GHS to the dustbin of history.

But this interpretation is false and misleading, for at least two reasons. First, it presumes that there was an explicit debate over the competing philosophies of science of the two sides, and that the Mengerian philosophy of science was found to be superior (or at least the most appropriate) for the purposes of political economy. What actually occurred was a clash in which neither side ceded any ground.<sup>23</sup>

Second, the idea that the GHS and its philosophy of science simply disappeared or became immediately obsolete is false. This is especially obvious if we recognize that the philosophy of science of the GHS is a branch of the tree of Hegelian philosophy of history—a branch whose later development includes the social inquiry approaches of, *inter alia*, Wilhelm Dilthey and the critical theorists (e.g. Max Horkheimer and Theodor Adorno). It is connected to this tradition by certain elements of its social ontology—in particular, a denial of the existence of objectively determined, trans-historical, individual-level types as the primitive building blocks of social phenomena.<sup>24</sup> The methodology attached to this world

view is recognizable as the kind of "hermeneutic circle" reasoning of many current social theorists. Schumpeter summarized the methodology as follows:

Approaching the material with a minimum burden of *a priori*, thereby capturing interdependencies which enter as additional *a priori*; this yields the (provisional) framework for investigation, a framework that is further refined in a continuing interplay of subject matter and mental process. (Schumpeter, 1926, pp. 345 and 346; cited in Backhaus, 1993)

The GHS' approach to social inquiry, then, clearly did not simply disappear in 1884.

Still, even if we reject the facile view of the *Methodenstreit*, the fact that the two sides in the debate failed to engage explicitly over the ontological issues that were actually the crux of their disagreement leaves us without a clear idea of what the *Methodenstreit* might have been if the participants had engaged with these issues explicitly. Fortunately, we have an example of what such a debate might have looked like. Some 70 years after the *Methodenstreit*, competing visions of the proper approach to social inquiry flared into conflict in Germany once again in what has retrospectively been dubbed the *Positivismusstreit* (controversy over positivism).<sup>25</sup> This time, the participants were the critical rationalists (e.g. Karl Popper and Hans Albert), espousing a position consonant with the prevailing attitude toward science within mainstream economics of the time, and the critical theorists (e.g. Theodor Adorno and Jürgen Habermas), who espoused a more sophisticated and philosophically sound version of the GHS approach to social inquiry. And although the *Positivismusstreit* is by no means a perfect proxy for the *Methodenstreit*, the ontological differences between the participants are strikingly similar to those between Menger and the GHS, and, significantly for our purposes, their debate touched explicitly and deeply on ontological issues.

A full account of the debate is beyond the scope of this paper. What is interesting for our purposes is that the two sides were able to bridge the gap between their radically different ontologies through their implicit agreement on the proper goals of science. Specifically, both sides held that any proper mode of scientific inquiry must, among other things, prevent dogma from masquerading as truth. Popper believed that this was best accomplished by requiring that proper scientific statements must be falsifiable "basic statements" and by promoting an environment of open and rigorous critique within the scientific community of properly scientific hypotheses. The critical theorists countered that the belief that such an evaluative framework was a priori appropriate to any realm of phenomena was itself dogmatic—and, importantly, they held that the dogma being smuggled

<sup>22</sup> See, for example, Caldwell (2004, p. 76).

<sup>23</sup> In fact, although both sides were intransigent, there was more commonality in their views than the dispute itself suggests. Menger recognized the importance of empirical exploration in economics and wrote in 1894 that "[b]oth [sides] recognize the necessary basis for the study of real phenomena and their laws is that of experience" (Hutchinson, 1973, p. 33, cited in Caldwell, 2004, p. 77n8). In addition, in the aftermath of the debate, Menger took to heart some of the criticisms of the GHS and revised the *Principles* to acknowledge the importance of historical factors and to limit the application of his ideas to the modern exchange economy. He even prevented reprinting and translation of the first edition for this reason. The revised manuscript was published (in German) posthumously in 1923. But when Hayek reprinted the *Principles* in German in 1933, he used the first edition, and this is the only edition to have been translated into English. See Hodgson (2001, p. 90). As such, there is a distinction between the conceptual continuity of Menger, on the one hand, and other Austrian School adherents on the other. On the side of the GHS, Schmoller himself acknowledged the importance of theory and deduction to economics, notwithstanding his extreme position in the *Methodenstreit*, writing in 1897 "[i]nduction and deduction are both necessary for the science, just as the right and the left foot are necessary for walking" (Senn, 1993–1994, p. 278, cited in Caldwell, 2004, p. 77n8).

<sup>24</sup> See, for example, the comments from Schmoller's 1882 comments cited in Nau (2000, p. 510).

<sup>25</sup> See Adorno et al. (1976) for a collection of contributions to the debate.

in was ontological in nature. To truly avoid such dogmatic infection, they argued, one must draw out the ontology of the realm under study through the use of dialectical and hermeneutic methods. These views parallel those of Menger and the GHS, respectively (although in more sophisticated form), and the engagement between Popper and the critical theorists on this subject gives some idea of what a deeper version of the *Methodenstreit* might have looked like.

Of course, there are important differences between the *Methodenstreit* and the *Positivismusstreit*, as well as between the critical rationalists and Menger on the one hand and the critical theorists and the GHS on the other. For example, the critical theorists might well object to being associated with the rather unsophisticated methods of the GHS, and Menger might well object to the connection to Popperian philosophy of science—while Popper emphasized the importance of falsification in his demarcation criterion, Menger insisted that the pure theory he proposed in the *Principles* was not subject to empirical testing. (Though, with respect to this latter point, Menger presented pure theory not as the *only* proper method for economics but as one essential method, and he considered empirical justification to play an important role in science. In addition, Popper's "rationality principle" and "situational logic" bear strong parallels to Menger's pure theory of economics. So, one must be careful in overstating the divergences between the two men's philosophies of social science.) By drawing parallels between the two debates, we do not mean to suggest that the *Positivismusstreit* was either a self-conscious continuation of the *Methodenstreit* or a continuation of the latter even if its participants themselves were not aware of it. Rather, we mean only to suggest that some of the important ontological issues we identified at the core of the *Methodenstreit* were also central points of controversy in the *Positivismusstreit*.

Specifically, in claiming that the position of the critical rationalists and those of Menger are somewhat parallel, we are referring principally to Menger's social ontology—i.e. his essentialist view that the world is composed of generic types whose nature, properties, and laws of interaction can be described abstractly and objectively. This is precisely the kind of ontology that, the critical theorists charged, the critical rationalists implicitly committed themselves to on the basis of their philosophy of science. The ontological commitment arose from the latter's unwillingness to place the scientific viewpoint itself under scrutiny. Adorno's remark that "[t]he Popperian concept of criticism inhibits logic by restricting it to scientific statements without regard for the logicity of its substratum which it requires in order to be true to its own meaning" (Adorno, 1976, pp. 25 and 26) could just as readily be applied to Menger's concept of the pure theory of economics. The latter achieves the analytical truth which Menger ascribes to it only to the extent that the substratum to which it applies (i.e. social life) is a

modular collection of the kinds of generic, transhistorical objects that Menger posits. Otherwise, the theory is at least undertheorized and at most unintelligible. For Menger, this logicity of the substratum was a self-evident fact. For the critical theorists, all such reputed social facts must be understood to be in dialectical relationship with the societal totality from which they emerge and which they simultaneously constitute (in part). Elevating aspects of one's situated experience to the status of transhistorical facts "... simply treats society, potentially the self-determining subject, as if it were an object, and could be determined from outside. It literally objectivates what, for its part, causes objectivation and what can provide an explanation for objectivation" (Adorno, 1976, p. 33). This is the crux of the critical theorists' dispute with the critical rationalists. And while the adherents of the GHS never expressed their misgivings in as sophisticated or perspicuous a manner as Adorno and Habermas did in the *Positivismusstreit*, the former's position was nonetheless consonant with the latter's.

This hybrid vision of the *Methodenstreit*—its actual history juxtaposed with the discursive possibilities suggested by the *Positivismusstreit*—demonstrates the importance of understanding the connections between methodology and ontology, and the danger of attempting merely methodological reforms in the absence of such an understanding. The *Methodenstreit* in its actual form represents a missed opportunity and a conflict left unresolved and deferred. Although there is no question that the consequences of the *Methodenstreit* were momentous, they cannot properly be seen as consequences flowing from an actual engagement with the differences of the opposing views. What was missed was an opportunity to strengthen economics by forcing its various schools to justify their approaches to social inquiry. The *Methodenstreit* is an especially bitter disappointment because the stage was set for just such an engagement. The *Positivismusstreit* shows us what such an engagement could have looked like. Even though the two sides remained largely unconvinced by the others' arguments, they had been forced to foreground the most fundamental (and often hidden) aspects of their philosophies of science. The record of such an explicit confrontation with the foundations of various scientific approaches is precisely the sort of storehouse of knowledge to which it is helpful to turn when a science is confronted with a crisis.

## 6. CONCLUSION

The recent economic crisis and ensuing global recession have prompted a lively and wide-ranging debate among economists about the current state of the discipline and the possibilities for its reform. In this paper, we have explored the extent to which reconsiderations of the incumbent ontology have been a part of

the debate. Although such reconsideration *in itself* is neither a necessary nor a sufficient condition for fruitful debate, it has historically played an important role in opening new horizons for the discipline. Keynes' ontological reconsideration led to a deeper understanding of the nature and importance of uncertainty, the relationship between savings, income, and investment, and the very nature of the capitalist economy. The *Methodenstreit* highlighted significant tensions inherent in formal (specifically axiomatic-deductive) analysis of social phenomena. And while that debate was largely inconclusive, it remains a touchstone for explorations of non-formal social science methods as well as a store of information for periods—like the current one—when these tensions percolate to the surface in consequential ways.

Our review of the current debate has found that although substantial reconsiderations of the incumbent ontology have been put forward, these contributions have generally come from outside of the discipline's mainstream. As a consequence, they have been almost completely absent from the most visible and influential loci of the debate—e.g. the panels, symposia, and hearings that form the basis of the blue ribbon consensus.<sup>26</sup> In our view, this is detrimental to the debate and should be addressed by creating new venues for dialogue between these positions and those in the first three categories of our taxonomy. Such active inclusion is important for two related reasons. First, as Colander has suggested, pluralism is the ally of scientific advancement. Certainly, there are trade-offs between coherence/focus of disciplinary activity and breadth of pluralism, and extreme pluralism verging on anarchy is undesirable. But in light of the historical importance of ontological reconsiderations and the credentials of the scholars putting forward these views, we would argue that inclusion should have the benefit of the doubt in this case. For this reason, the emergence of the Institute for New Economic Thinking and its funding support for a broad range of heterodox research—including post-Keynesian economics and ACE, as well as work we would classify in the "Reconsider Formalism" category—is a very positive development that moves the discussion toward more pluralism.

The enormity of the recent crisis and the almost complete lack of foresight on the part of economists suggest that it is important to cast our net wide in our current methodological introspection. It is one of the central features of crises that one does not *know* whether the source of the problem lies at the surface or at the root of the science. As such, one must approach the issue of diagnosis and reform with an assumption of ignorance: given that we do not know how deep the

problem is, what is the appropriate level at which the debate ought to be pitched? In such a case, it is essential that the debate at least be able to recognize, conceptualize, and articulate deep issues, whether or not it turns out that the crisis is a profound one. Or, in Kuhnian terms, we can say that it is necessary to be capable of considering the possibility that the crisis cannot be resolved from within the current paradigm. Typically, considering such a possibility requires seeing the world from a perspective different from that afforded by the existing tools and concepts of the science. And, specifically, this means being capable of entertaining reconceptualizations of one's science (including the subject matter of the science) that are at odds with elements of the current paradigm.

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<sup>26</sup> These views have, though, appeared in other peer-reviewed venues—e.g. the *Cambridge Journal of Economics*, the *Journal of Economic Methodology*, the *Real World Economic Review*, and the *Review of Radical Political Economy*.

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