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# Methodenstreit 2011? 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 gave rise to a lively and apparently wide-ranging debate over the state of the discipline and the need (if any) for significant reform. But has the actual debate been robust enough to contemplate significant reform, or has it fallen short? We propose a framework for assessing the depth of methodological debate and apply it to the current methodological debate. Doing so, we find that the current debate has been shallow, especially when compared to historical precedents of deep debate such as the late 19<sup>th</sup>-century *Methodenstreit* and the period of Keynesian innovation. Moreover, we conclude that unless the few voices pressing for deep debate are given their due, there is little hope that the recent disciplinary crisis will be met with an appropriate level of reform.

**Keywords** financial crisis, methodology, ontology, history of economic thought, Keynes, *Methodenstreit* 

#### 1 INTRODUCTION

The general failure of economists to predict the financial crash of 2008 has given rise to a lively and apparently wide-ranging debate over the state of the discipline and the need (if any) for significant reform. Paul 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. Thomas 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." (Sargent, 2010). Similar exchanges are taking place in the pages of journals and the halls of conferences around the world (see section 3 below). Clearly, a period of serious debate and introspection is at hand...or is it?

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 1970s, to name some of the most notable. But, of course, not all methodological debates are created equal. Some involve a questioning of the very foundations of the science, while others are mainly concerned with issues of incremental reform or merely cosmetic change. In this paper we explore the question of what distinguishes profound from shallow methodological debate and place the current debate over economic methodology within this continuum. We argue that the key distinction between profound and shallow debate is whether or not the ontological premises of incumbent scientific practice are subject to reconsideration—i.e. whether or not the discipline's conception of the nature of its subject matter is on the table. A reconsideration at this level has profound implications for all aspects of the discipline's practice, whereas debates that take the incumbent ontology as presumptively appropriate inherently limit the range of possible reconsideration and reform.

Applying this distinction to the current debate we find that it is relatively shallow, confined for the most part to purely methodological considerations while leaving the incumbent ontology (and epistemology) unexamined. In support of this judgment, we review two historical instances of profound methodological debate for purposes of contrast: the *Methodenstreit* of the 1880s and Keynes's innovation of the 1930s. The *Methodenstreit* provides a clear example of an

instance in which a moment of ostensibly methodological crisis was in fact grounded in deeper issues regarding the nature of the realm under study, that is, the ontology of the field. 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. 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 if economists are willing to probe deeply into the incumbent order. As we identify in section 3 below, such probing voices do exist today but they are currently being shunted to the sidelines. Our analysis suggests that it is crucial they be given more attention.

Our discussion is presented in six sections. In section 2 we present a framework for distinguishing deep from shallow 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 discuss the distinction between profound and shallow debate through the examples of Keynes' innovations and the *Methodenstreit*, respectively. Section 6 concludes.

#### 2 "DEEP" VERSUS "SHALLOW" DEBATE

We would like to be able to make a judgment about the extent to which the current debate has the potential to effect significant reform in the way economics is done—in other words, to assess the depth of the debate. But what, exactly, does it mean to assess the "depth" of a debate? There is by now a substantial body of work discussing distinctions between fundamental aspects of a science and more surface aspects. 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. 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.

Consonant with these traditions, our approach to assessing the depth of the current debate is to focus on the contributions of individual practitioners and assess the extent to which they are discussing issues that are, by their own lights, surface or fundamental. This approach is based on a presumption that there is a meaningful distinction to be drawn between surface and fundamental aspects of scientific practice but also a recognition that this distinction is intersubjective and subject to local variations rather than objective and universal.

We measure the relative depth of a contribution by the extent to which it is concerned with issues of methodology *only* or delves deeper into issues of epistemology and/or ontology.

The ontological is the most fundamental level, encompassing the practitioner's conception of the extant objects of the universe under study—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. And 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 are not simply nested—with methodology being determined entirely by epistemology and epistemology entirely by ontology. But, as we discuss below, in both the current debate 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 requires no reconsideration of the incumbent epistemology or ontology of economics. 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 would require a change in methodology. As such, we classify debates directly engaging with ontology as deeper than those pitched at a purely methodological level.

# 3 REFORMING ECONOMICS: THE CACOPHONY IN 2011

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 August 12<sup>th</sup>, 2008, about a month before the collapse of Lehman Brothers, IMF Chief Economist and MIT professor Olivier Blanchard wrote:

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 macro economist, Willem Buiter, would write about the need for a "new paradigm":

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) "Connect economics to the economy." <sup>ii</sup> We will discuss each of these categories below. Before doing so, however, it will be helpful to depict a newly emerging "consensus" view within the discipline as a starting point.

# 3.1 The emerging "consensus" view

Our organization of the current debate into four categories of response is intended 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 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 why no one had predicted the financial crisis, two panels organized by the Allied Social Sciences Association at its 2009 and 2010 annual conferences,

testimony before Congress by David Colander, and a keynote address by Larry Summers to the 2011 Institute for New Economic Thinking conference at Bretton Woods.<sup>iii</sup>

Although there is some diversity among these four sets of pronouncements, all ultimately conclude that no profound changes need be considered. 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 Colander 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 undermodeled 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 undermodeled complexity. Summers echoes these views in his INET keynote address. While he explicitly criticizes real business cycle and dynamic stochastic general equilibrium (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 "Connect economics to the economy"—go a bit farther, with some voices in the latter group even calling for profound reconsideration of current practice. It is in this group that we find the only evidence of the seeds of deep debate.

#### 3.2 Do Nothing

The "Do Nothing" view has been articulated largely in recent interviews with major economists, including Thomas Sargent (2010), Eugene Fama (Cassidy 2010a) and John 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 "[r]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 Casey 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:

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 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 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. Cochrane echoes this sentiment. Replying specifically to Paul Krugman's charge (quoted at the beginning of this article) that economics' overemphasis on mathematical modeling was a major factor in its recent failure, he 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 Dynamic Stochastic General Equilibrium (DSGE) models, but simply make certain important aspects of the financial sector endogenous to the models. This is the possible near-future of macroeconomics envisioned by James 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 seriallycorrelated financial intermediation shocks and suboptimal fiscal policy." Daron 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.

A stronger version of "Add Finance and Stir" calls not only for incorporation of finance into macro models, but also 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 crucial to the reform of economics. The Post Keynesian movement has been among the most vocal in calling for an overturning of the dominant paradigm. In this case the plea is for a return to the ideas of Keynes, especially in the modified version of Hyman Minsky, whose model of financial fragility and the endogeneity of financial boom and bust has gotten him more attention recently than almost any other single economist of the past other than Keynes himself. George Akerlof and Robert 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.

Thomas Lux and Frank Westerhoff (2009, p. 3) share Akerlof and Shiller's concern that macroeconomics fails to adequately model systemic (financial) risk, but their solution is the adoption of methodology of statistical physics, which "shows that relatively simple models with plausible behavioral rules have the potential to replicate key empirical regularities of financial markets." Along similar lines, Colander et al. (2009) write that "the possibility of systemic risk has not been entirely ignored but it has been defined as lying outside the responsibility of market participants...the deliberate ignoring of the systemic risk factors or the failure to at least point them out to the public amounts to a sort of academic 'moral hazard.'"

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, it dovetails nicely with reforms already underway within economics—most notably the rise of behavioral finance. As such, it requires relatively minimal

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 in addition 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 deep debate—suggesting that current models do not adequately capture the complexity of economic reality could be an entrée into a consideration of how precisely we should even be conceiving of the economic reality. We find, however, that only some of the contributors in the Add Institutions and Complexity Group take the debate to this deep level, while others confine their notion of complexity only to the kind of complexity that can already be comprehended by current economic practice.

Ricardo Caballero (2010) and Davis Colander et al. (2009) provide a examples of this latter type of contribution. Vi Caballero (2010) 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.

In a similar vein, Colander et al. (2009) emphasize the "heterogeneity of actors" in contrast to the representative agent of the DSGE tradition, and call for a rejection of the efficient market hypothesis, writing that the "interplay between leverage, connectivity and system risk needs to be investigated at the aggregate level."

But while this could be an entrée into a deep critique—i.e. one where no restriction other than the goal of providing the best explanation guides the choice of methodologies tabled for consideration—these mainstream calls for reform generally do not stray from the confines of the incumbent paradigm's toolkit. Caballero concludes only 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). And Colander's articulation of the complexity of the economy, and the way it ought to be approached by economists is couched entirely in mathematical terms. "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>vii</sup>

Such conclusions have been the rule among calls for greater complexity in economic methodology from within the mainstream of the discipline. The notable exceptions of 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 Paul Krugman's (2009) recent criticism of economics' *a priori* commitment to mathematical modeling only throw into sharper relief the almost complete

lack of similar appeals within the mainstream.

# 3.5 Connect economics to the economy

It is important to note, however, that there have been numerous methodological interventions from outside of the mainstream that counsel a deepening of the debate. Old-style institutionalist economists, Critical Realists and some Post-Keynesian economists have argued not only that rigid adherence to incumbent practices in the lead-up to the crisis masked deeper complexities but, further, that it is incumbent upon us at least to consider that an *a priori* commitment to methodological individualism or mathematical modeling generally may be more an obstacle than a conduit to empirical understanding. Put another way, these voices have called for reforms that would better "Connect Economics to the Economy."

As with Caballero (2009) and Colander et al. (2009), these voices indict rigid adherence to incumbent 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 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, "[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 is 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." (p. 763) He calls for a "more grounded framework." to better understand this "open, structured, totality in motion."

From the review above, we can see that the majority of the voices in the current debate are focused on methodology only, whereas only the "Connecting Economics to the Economy"

camp is probing significantly more deeply. With respect to the former voices, there is no consideration of the need to look outside of the incumbent methodology. This position can only be tenable under the assumption that the epistemology and ontology that underlie this methodology are valid. Concretely, an *a priori* methodological commitment to the principle that mathematical modeling (and *only* mathematical modeling) is adequate implies a commitment to the principle that knowledge of the economy is attainable through the sole use of mathematical modeling and that the social world is so-constructed that its elements can be thought of as being structured and interconnected in a manner similar to that of mathematical objects.

With respect to the Connecting Economics to the Economy camp, some authors make direct reference to ontology (e.g. Lawson), while others make implicit reference to it through their methodological statements. Hodgson (2008, p. 276), for example, references "the intractabilities of uncertainty, complexity and system openness in the real world." But while this seems similar to statements made by, e.g., Colander and Caballero (referenced above), Hodgson's attendant appeal to jettison the *a priori* commitment to mathematics-only methodology makes clear that he envisions this complexity as taking a form different from that envisioned by economics' incumbent ontology. The statements of Akerlof and Krugman are even more oblique, and it is not yet clear just what kind of methodological reform they would consider necessary. Nonetheless, their statements at least open the door to the kinds of ontological discussions that could include voices like those of Hodgson and Lawson. Our review of the debate *as a whole*, however, indicates that it is weighted heavily toward the methodology-only category. Although contrary voices are present, it is clear from their absence in the various articulations of the Blue Ribbon consensus that they are currently only fringe contributions.

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

In the history of economics there have been several methodological debates that have gone deeper than the current debate, taking up issues related to the discipline's foundational assumptions about the nature of the social world. Of these deep 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 engendered an intense debate and significant innovations in both economic methodology and policymaking, but as we will argue below 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. These reconceived economic entities did not fit comfortably within the incumbent methodology, and as a result the discipline's consideration and digestion of them required a deep debate—i.e. one that at least considered the possibility that the foundations of economic practice needed to change.

To demonstrate the depth of the debate engendered by Keynes, we organize our discussion around three interactions between Keynes and the rest of the profession, each of which highlights a particular aspect of his ontological innovation. The first is the debate between Keynes and Hayek in the early to mid-1930s, highlighting Keynes' reconceptualization of uncertainty; the second is that between Keynes and Pigou, which focused on the nature of

savings and its relationship to investment; and the third is the debate between Keynesians and Post Keynesians, mostly taking place well after Keynes's death, which highlighted the epistemological and methodological tensions wrought by Keynes' new economic ontology.

# 4.1 Keynes versus Hayek

The most intense debates between Keynes and Hayek took place in the early 1930's, with Hayek having just arrived from Vienna to take up a position at the LSE. The debate revolved around Keynes's (1971 [1931]) A Treatise on Money, but it concerned claims that would also underpin The General Theory. Specifically, Hayek accused Keynes of lacking a theory of capital and thus a serious theory of interest. To some extent, Hayek was right: In the Treatise, Keynes had begun to develop the liquidity preference theory of interest, but its centrality to his overall view had not yet become clear, even to Keynes himself. Nonetheless, in the Treatise Keynes wrote of saving as a means of responding to uncertainty and the consequent desire for liquidity. This view implied a delinking of the saving decision from any future consumption stream, the premise of the Austrian theory of capital that Hayek espoused.

The Hayek-Keynes disagreement over the theory of the interest rate had major implications both for the approach to economic agency and the understanding of economic downturn generally. The Austrian theory presumed a smooth causal chain from saving to investment. Keynes understood saving as motivated in part by uncertainty. Greater uncertainty about the future provoked a desire for greater liquidity, which generated a higher level of saving.

Keynes's liquidity preference theory of the rate of interest gave the result that the interest rate could settle at a rate that would not bring an adequate level of investment for the attainment of full employment. Thus for Keynes underinvestment was the central feature of economic

slump, and government spending to pump up demand was the necessary policy response. Hayek argued the opposite—that the slump resulted from an excess of investment relative to consumer demand. An economic downturn is the "process of eliminating the unsustainable investment" not financed by genuine saving. Once the downturn had ended, however, government intervention would only delay a sustained recovery; the quickest cure would be for people to save more, thus supporting a sustainable recovery in investment.

Hayek thus resisted Keynes's effort to reverse the direction of causality between saving and investment. Hayek saw investment as generally financed by saving and thus the latter causing the former. Keynes's reply to Hayek (Keynes 1931) included an attack on Hayek's *Prices and Production* (Hayek 1931). Keynes wrote: "It is an extraordinary example of how, starting with a mistake, a remorseless logician can end up in Bedlam." (Keynes 1931, p. 252)

Hayek invited Robert Bryce, who had attended Keynes's 1932-1935 Cambridge lectures as a student, to give a series of lectures in 1935 at LSE to explain *The General Theory*. Bryce interpreted Keynes's theory of unemployment as being primarily the result of wage stickiness, a perspective that fed into Hayek's view that his own theory was more general than Keynes's and that Keynes's theory was the special case involving rigid wages. In 1935, Bryce went to Harvard where he was considered one of the most knowledgeable about Keynes's theory. Paul Samuelson also learned about Keynes's theory from Bryce.<sup>ix</sup>

According to Keynes, interest is paid in order to induce individuals to part with their money or, as he writes, "[T]he rate of interest at any time, being the reward for parting with liquidity, is a measure of the unwillingness of those who possess money to part with their liquid control over it" (Keynes 1964 [1936], p. 167). Keynes viewed his attack on the "classical" theory of the rate of interest as an attack on the reigning theory that the interest rate was simply the

price of money that equilibrated savings and investment, a view Keynes saw to be a version of Say's Law. Once Keynes established the liquidity preference theory of the rate of interest, it was a short step to the idea that this rate of interest can be different from that required for full employment investment, and that this difference can persist for long periods of time. For Keynes, saving was a leakage from demand and occurred not because people preferred to wait for a larger consumption bundle in the future (the classical explanation of saving) but because they felt uncertain or anxious about the future. As Skidelsky (1994, p. 595) writes, "At this level Keynes felt he had overturned the classical paradigm. It was the hunger for money, not the hunger for goods, which controlled macroeconomic outcomes." From the orthodox perspective, a low level of output resulted from too little saving, since this pulled resources otherwise available to entrepreneurs. For Keynes, the primary issue was liquidity preference and the desire to save out of income to satisfy this preference, with the result that consumption would be weakened and along with expected sales proceeds and investment. The result would be a lower income, 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.

# 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's disagreement with Pigou was not simply a technical one about the nature of market adjustment. It went much deeper, into the general 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 was Panglossian rather than clarifying, as it entirely missed the nature of capitalism as a "monetary production system."

Keynes's 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. 544-545). 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's departure from the classical conception of causality, since it is not rooted in the demands of scarcity. She writes:

[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 (1992, p. 557), "after some initial discussions of the 1930s and 1940s, most of th[e] interpretive literate [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."

The more radical aspects of Keynes's rethinking of economic agency and thus of the nature of capitalism, are largely absent from the "Keynesianism" that emerged out of Hicks's 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 the money market. The model was, by Hicks' own admission, a simplifying formalization of Keynes's 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.

While 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 it left out many of Keynes's 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's 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's 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's 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. Robinson (1971) referred to Hicks's version as "Bastard Keynesianism." 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's rich notion of individual agency and economic change. According to Lodewijks (2003, pp. 28-29):

[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 disortion that forces the General Theory in the older neoclassical mould."

Kregel (1977) reinforces this point, pressing the significance of the ontological divide between the methodology of the Keynesians and the Post-Keynsians, writing that

one does not "tame" 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 to others, it is clear that in the epistemological realm he is intentionally following the very traditional criteria of inductivism. But Keynes's 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 were at issue—but also in the hostile response of the Post-Keynesians to attempts to reduce Keynes' insights to IS-LM analysis. And while one may argue about the extent to which the discipline's current state accurately reflects the spirit of Keynes' contribution, it is clear that the deep nature of his intervention necessitated substantive and deep debate and ultimately resulted in more than surface change in economic practice.

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

Although the milieu of late 19<sup>th</sup>-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>x</sup> in the late 19<sup>th</sup>-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>xi</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>xii</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>xiii</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 *Methodonstreit*: *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.

Schmoller responded swiftly and directly to the *Investigations*, writing a sharply negative review (Schmoller 1883) for publication in his Jahrbüch 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 a much deeper level—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, 217-9). 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. Further, 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.xiv Schmoller's response to the Investigations does little better at constructively addressing the ontological gulf that lies at the core of the disagreement. While 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 nonengagement of the participants with the major issues at hand. The fact that twentieth century economics moved squarely in the direction of Mengerian methodology and away from that of 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>xv</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, Max Weber, 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. 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. 345-6; cited in Backhaus 1993)

The GHS's 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 deeply over the core ontological issues 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 seventy 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). This time, the participants were the Critical Rationalists (e.g. Karl Popper), espousing a position consonant with that of modern economics, and the Critical Theorists (e.g. Max Horkheimer and Theodor Adorno), who espoused a more sophisticated and philosophically sound version of the GHS approach to social inquiry. And although *Positivismusstreit* is not 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.

Although 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 agreement on the proper goals of science. Specifically, they agreed 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." 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 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 interpretive/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.

This hybrid vision of the *Methodenstreit*—its actual history juxtaposed with the Positivismusstreit (playing the role of its possible history)—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—a situation inherently unfamiliar, and in which superficial remedies may well be inadequate.

#### 6 CONCLUSION

The economic crisis that began in 2008 was so severe and without prior warning from economists that there were initial expectations that it would result in a major reform of economic thought. We contrasted the debate of the past three years with earlier debates over economic theory. While Keynes's innovations were certainly not inspired by the German Historical School

per se, they are much more consonant with the tradition of social inquiry from which the GHS sprang (and with the message of the Critical Theorists in the *Positivismusstreit*) than with that of Neoclassical economics. The *Methodenstreit* of the 1870s and '80s involved a deep debate over the nature and interpretation of economic life, questioning the ontological foundations of deductivism and historicism.

The current debate over the reform of economics has lacked any of this depth. It has been taking place almost entirely on the level of methodology, driven largely by a Friedman-like criterion of the need to raise predictive power. Lawson (2009, p. 759), writing about the current disgruntlement with economics, notes that "mostly this criticism focuses on the substantive economic theories and policies that previously have been in favour. Seemingly little attention is given to the modes of analysis that have been utilized in support of these positions."

Perhaps a reconsideration of the ontological foundations of economic thought is not merited. But our review of the wide array of economists' reflections on the apparent failure of economics in recent years indicates that none of the mainstream responses—Add Finance and Stir and Do Nothing—reveal a willingness, ability, or even the necessary language, to debate the question at this level.

It is one of the central features of such 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 appropriateness 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.

#### **NOTES**

<sup>i</sup> See, e.g., Shapin (1995) for a general overview and Hands (2001) for an overview from the economist's perspective.

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.

iii 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)

iv 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..."

<sup>&</sup>lt;sup>v</sup> Four books seeking to rehabilitate the ideas of Keynes are Taylor (2010), Davidson (2009), Skidelsky (2009), and Eatwell and Milgate (2011).

- vi Colander's co-authors in this paper are Hans Föllmer, Armin Haas, Michael Goldberg, Katarina Juselius, Alan Kirman, Thomas Lux and Brigitte Sloth.
- vii This statement is from Professor Colander's written testimony before Congress, to which the paper Colander et al. (2009) was appended.
- viii A detailed argument in support of this claim is beyond the scope of this paper. See Spiegler (2005, esp. chs. 2-3; 2006) and Spiegler and Milberg (2009) for more (and also Lawson (1997) for a similar argument with different prescriptions).
- ix See Colander and Landreth (1996) and Davidson (2009, appendix).
- <sup>x</sup> 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.
- xi 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.
- xii See Bostaph (1978, p. 139) and Tribe (2007, p. 74).
- xiii 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 19<sup>th</sup> century Germany and Austria.
- xiv See, e.g., Menger (2009 [1883], pp. 60-3).
- There is one significant caveat to this statement. 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 1st 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.

That these innovations eventually gained a foothold in mainstream economics should not be interpreted as a vindication of the flexibility of the ontological framework of Neoclassical economics. For one, the *introduction* of the ideas into economics required stepping outside of the incumbent ontology. In addition, the innovations only gained a secure foothold in mainstream economics after they had been fully formalized, and the extent to which their formalization captured the essence of Keynes' meaning is still a subject of debate. What is important about these innovations for our purposes is that their source was an alternative ontology.

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xvi See, e.g., the comments from Schmoller's 1882 comments cited in Nau (2000, p. 510)

xvii See Adorno et al. (1976) for a collection of contributions to the debate.

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