Thoughts on the Prospects for Substantial Methodological Reform in Economics

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Introduction

There is widespread acknowledgment, even among many mainstream economists, that the economics profession as a whole did a poor job of anticipating and understanding the recent financial crisis.[1] Given the severity of the crisis and its aftermath, this acknowledgement has, predictably, been accompanied by calls for a critical examination of economic methodology. But what are the prospects that this period of re-examination will issue in substantial reforms? And what would constitute "substantial" reforms? In this brief paper, I will offer a preliminary answer to these questions. I find (a) that the recent crisis has provided evidence that interpretive, qualitative methods are needed to supplement economics' formal, quantitative methods, (b) that the current debate within economics is unlikely to move the discipline in this direction, but that (c) there may be an opening for targeted interventions from outside the mainstream—and, where possible, in partnership with sympathetic mainstream economists—to have a positive effect. In particular, I advocate for collaborations between anthropologists and economics to flesh out the conceptual landscapes of social realms with major public policy relevance—e.g. the health insurance market, the large U.S. entitlement programs, clean energy markets, and the political lobbying industry.

I. The mainstream and the current debate

Since 2009, there has been a lively discussion among economists regarding what, if anything, we can learn from the financial crisis. And there is some reason for optimism that this discussion may lead to reforms in the practice of economics. For one, there is historical precedent for moments of crisis leading to new directions in economics. The most notable of these is the advent of Keynesian macroeconomics in the wake of the Great Depression, but there have been others as well. Various theories of imperfect competition and a greater emphasis on empirical

[1] Although this sentiment is widespread it is not universal, as I discuss below.

economics arose out of widespread dissatisfaction with the highly formal economics of the mid-twentieth century and its foundations in perfect rationality assumptions. Behavioral economics also arose out of this ferment, and was given a significant boost in the wake of the failure of Long Term Capital Management in 1998.

A second reason for optimism is the extent to which even some of the most highly respected economists have been willing to question the very foundations of economic methodology. A notable example is Nobel laureate Paul Krugman's lengthy New York Times Magazine piece decrying the state of contemporary economics, and pointing specifically to excessive faith in mathematical models as a primary culprit. "[T]he central cause of the profession's failure," he wrote, "was the desire for an all-encompassing, intellectually elegant approach that also gave economists a chance to show off their mathematical prowess" (Krugman 2009). And Krugman is not alone among mainstream economists in expressing concern over the state of contemporary economic methodology. The American Economic Association has convened several Blue Ribbon panels over the past two years to discuss the nature of the global economic crisis and the implications to the economics profession of its general failure to predict or understand the crisis. Additionally, several highly respected economists have published post-mortem articles and books suggesting—to varying degrees—that although we should not throw out the baby with the bathwater, there are improvements that we could and should make to economic methodology. Robert Shiller and George Akerlof have been among the most vocal in this regard, and their book *Animal Spirits* (2009) is a spirited appeal for a more robust model of the financial markets and their role in the macroeconomy.

Unfortunately, despite these positive signs, there are also reasons for pessimism about the depth of the discipline's appetite for reform. First among these reasons is the fact that we have seen this movie before. Here was the state of mainstream dissatisfaction in 1970, for example, as described by two past presidents of the Econometric Society (one of whom was also a Nobel Laureate):

> The achievements of economic theory in the last two decades are both impressive and in many ways beautiful. But it cannot be denied that there is something scandalous in the spectacle of so many people refining the analysis of economic states which they give no reason to suppose will ever, or have ever, come about.... It is an unsatisfactory and slightly dishonest state of affairs.[2]

[2] Hahn (1970, p. 1)

> To deepen the foundation of our analytical system it will be necessary to reach unhesitatingly beyond the limits of the domain of economic phenomena as it has been staked out up to now. … To penetrate below the skin-thin surface of conventional consumption functions, it will be necessary to develop a systematic study of the structural characteristics and of the functioning of households, an area in which description and analysis of social, anthropological and demographic factors must obviously occupy the center of the stage.[3]

For the purposes of assessing the prospects of the present-day debate, what is particularly significant about these quotations is the manner in which the spirit behind them was and was not ultimately addressed by the discipline. There is no question that the discipline in the 1980s and 1990s increasingly eschewed perfect market models and embraced empirical work. But the *a priori* commitment to mathematical modeling as the engine of discovery of economics remained, as did the attendant denigration of the kinds of qualitative and interpretive methods Leontief asserted "must obviously occupy the center of the stage." This, in fact, has been the general pattern of economic methodological reform since Keynes: dissent is acceptable and can lead to methodological reform only if it can be expressed mathematically. Thus, Keynes' insights entered into the mainstream as the IS-LM model, Leontief's concerns were answered through imperfect competition models and more sophisticated econometrics, concerns about the complexity of human psychology gave rise to hyperbolic discounting, prospect theory and mathematical models of fairness, and so on.

The current debate, thus far, largely follows this pattern. Although there are some important voices calling for methodological deepening beyond mathematics, the preponderance of the mainstream response is a call for more of the same—what might be referred to as "methodological doubling down." In another paper, William Milberg and I identify two versions of this mainstream response, which we call the "Do Nothing" and "Add Finance and Stir" positions (Spiegler and Milberg 2010). I will briefly review the content of these positions below, and argue that they are inadequate as a response to the current crisis without the addition of qualitative, interpretive methods into the core of economic methodology.

a. 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

[3] Leontief (1971, p. 4). The previous quotation, from Hahn, was cited verbatim in Leontief's piece.

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. In Sargent's (2010) words, "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. That's life…" Cochrane echoes this sentiment. Replying specifically to Paul Krugman's charge 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 2009)

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.

b. 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 we need 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 serially-correlated financial intermediation shocks and suboptimal fiscal policy."[4] 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.[5] George Akerlof and Robert Shiller (2009) also hearken back to Keynes in emphasizing that irrationality—they adopt Keyne's 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. Lux and 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

[4] 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…"
[5] Three books on the crisis by Keynesian experts are Taylor (2010), Davidson (2009), and Skidelsky (2009).

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 several Nobel laureates as well as 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.

c. The inadequacy of the mainstream response

The mainstream responses to the crisis provide little impetus for major methodological reform within economics. Obviously, the Do Nothing response counsels no substantial change, but even the Add Finance and Stir response would leave the core of economic methodology not only unchanged but also unexamined. The most radical alternatives that this group envisions are an intensification of the behavioral economics project, especially with respect to the modeling of the financial sector, and explicit modeling of systemic risk and deleveraging cycles. But while this would represent reform on a superficial level—i.e. finance and macro models would use different functional forms, parameters, etc.—it would also represent wholesale continuity on a deeper level—i.e. the core of economic analysis would still consist of, and only of, the deployment of mathematical models of generic types that ostensibly represent the actual phenomena of the economy. This is an inadequate response, primarily because it completely ignores what ought to have been the central lesson of the discipline's recent failure: namely, that it is not enough for economists to have models of generic situation types on the shelf that are deployed as explanations of social phenomena simply on the basis of an *ad hoc* determination that the realm to be explained falls under that generic category. At the very least, we require a supplement to this process—a method (i.e. not mere *ad hockery*) for judging the extent to which our generic models can plausibly be understood as representations of actual sets of phenomena. And by definition, the refinement of mathematical models alone can never accomplish this—it requires the understanding of the realm under study on its own terms, and this requires qualitative, interpretive methods.

To illustrate the manner in which the current use of mathematical models is *ad hoc*, and the problems this can cause, it is helpful to consider an example from the economic literature on housing prices shortly before the crisis. In a 2003 paper, Robert Shiller and Karl Case—two economists widely recognized as experts on the housing market—considered the question of whether or not there was a housing bubble in the United States. The larger context of the paper was the question of whether or not the elevated prices in U.S. housing ought to be a matter of concern for economic activity in general—a question the authors address in the presentation and interpretation of their results. Notably, the authors' approach to the question was to deploy the standard tools of economic analysis of asset prices. Specifically, they used regression analysis to decompose housing prices into the component generated by "fundamentals"— i.e. factors that the authors deemed to be directly relevant to a rational assessment of the value of housing—and the residual component, which was interpreted as being attributable to irrational factors.[6] The greater the latter component of price, the more evidence for a bubble. From their regressions, the authors concluded that "income alone explains patterns of home price changes in all but eight states" and that they "cannot reject the hypothesis that a bubble exists in these [eight] states" (Case and Shiller 2003, p. 312).

On the basis of this evidence, the authors concluded that there was some, limited evidence of a housing bubble in some states, but that activity in the housing market was unlikely to have a significant negative effect on the economy. "[J]udging from the historical record," they wrote, "a nationwide drop in real housing prices is unlikely, and the drops in different cities are not likely to be synchronous: some will probably not occur for a number of year. Such a lack of synchrony would blunt the impact on the aggregate economy of the bursting housing bubble" (Case and Shiller 2003, p. 342). Clearly, the authors were wrong in their relatively mild assessment of the potential problems brewing in the housing market, but the point is not *that* they were wrong but *why* they were wrong.[7] In considering the question of whether there was a

[6] "Rational" here includes the expanded notion of rationality employed by behavioral economics.
[7] A related point is that the authors' conclusions were *not* wrong when considered narrowly from within the mainstream economics conceptualization of the economy. Based on their definition of "bubble" it is true that most areas of the United States were not experiencing a housing bubble and that, in light of this, the potential damage to the economy as a whole should have been limited. The problem with this, however, is that the inflation of U.S. housing prices during this period was the product of a deeper process that was both outside the scope of the authors' field of

problem in the housing market with respect to asset valuation, the authors envisioned the market as a standard asset market and tested their model against data that was only capable of giving them evidence about the internal consistency of their rational reconstruction of the situation. The results of their empirical analysis were expressible within their presumed ontology of the housing market (i.e. the housing market as a standard asset market), but this was not enough to indicate whether or not their results were a description of the actual housing market or just a logically possible but inaccurate version.

What we have learned about the U.S. housing market of the 1990s and 2000s in the past few years indicates quite clearly that it was not simply another, standard asset market. Not only were its dynamics being driven in large part by other financial products markets, those dynamics, in turn, were being driven as much by political and regulatory dynamics as "economic fundamentals." As such, the question of whether or not elevated housing prices were problematic for the economy could not be answered by focusing narrowly on the housing market alone. One might argue that it is unfair to indict Case and Shiller for missing this point in 2003, before the crisis began in earnest, but if the purpose of economic analysis is to detect the deeper patterns of economic activity that might not be obvious to the lay person, then this argument is unfounded. The dynamics underlying the crisis were well entrenched by the early 2000s, and, moreover, the information that would have made this clear was available. As Case and Shiller themselves point out in their paper, several writers in the financial press had begun to express concern about the housing market by the time they were writing their paper. In addition—as we now know from insider accounts and investigative journalism—the individuals involved in creating and trading credit default swap contracts and sub-prime mortgage-backed securities and collateralized debt obligations were, for the most part, well aware by that time that massive amounts of risk were building up in a manner that could only be justified by the assumption that US housing prices would *never* fall.[8] The most ominous failing of economics during this period was that the information necessary to predict and understand the crisis was available but out of the reach of the standard tools of economics.

attention and crucially relevant to answering the question they claimed to be answering: i.e. whether or not the elevated prices in U.S. housing ought to be a matter of concern for economic activity in general.

[8] There are many such accounts. See, for example, Smith (2010), Cohan (2009), Lewis (2010), Roubini and Mihm (2010) and the 2009 PBS Frontline documentary *The Warning*.

The mainstream responses to the crisis thus far have missed this crucial point. In fact, even those in the reformist wing of the Add Finance and Stir camp are essentially calling for more of the same. Robert Shiller has advocated for a more aggressive financialization of the economy. He argues that two of the major culprits in the recent crisis were the inability of individuals to accurately assess their risk exposure and the lack of a sufficient array of housing-risk hedging markets. As a remedy, he suggests government subsidization of financial advice along with the creation of new risk-hedging markets to counter this (Shiller 2009). Along similar lines, Larry Summers has recently argued that we should be careful about placing too much blame at the feet of the "financial innovation" at the center of the crisis—for example, the over the counter derivates market in which credit default swaps were traded (Summers 2011)—as though "financial innovation" could be understood simply as the theoretical functioning of the products emerging from such innovation. What both of these positions miss is that markets and financial products are human-made and embedded in the activities of actual communities with their own cultures and institutions. Continuing to attempt to understand the financial markets and the objects that circulate within them in isolation from these cultures can only lead to our being "blindsided" once again in the not-too-distant future.

II. The prospects for substantial reform

Thankfully, economists do not need to go into the future with the blinders that have kept the discipline from incorporating relevant information in the past. There is a long tradition in the social sciences outside of economics—most notably in anthropology—of the kinds of methods necessary to elicit richer conceptualizations of social landscapes, and it is from these traditions that economists must begin to draw. Moreover, although the mainstream responses thus far have been inadequate, there have been some encouraging signs that there is still an opening to introduce these methods in a meaningful way. What is needed is a strategy of targeted interventions to seize on this opening.

a. The existing foundation

The elements of a foundation for a more robust, effective economics already exist. They consist of literatures and practices both inside and outside economics. Within economics, institutional economics (of the "old institutionalism" stripe), Post-Keynesian economics and

Critical Realism have established beachheads for the use of more fine-grained methods. This is evident in the response from these groups to the recent crisis, which has been much deeper than the mainstream responses reviewed above.

In general, these economists see the recent failures of economics to have been caused by a lack of adequate connection between economic models and economic life. From the Post-Keynesian perspective, what is needed is additional complexity in macroeconomic models—especially with respect to the financial sector—to reflect the complex interactions and feedbacks within and among economic institutions. Axel Leijonhufvud (2009), for example, argues that "the repeated occurrence of financial crashes or crises hardly seems consistent with intertemporal equilibrium theory," and he proposes the development of an "adaptive dynamic theory" so that negative feedbacks from, say, systemic deleveraging, are captured. Such feedbacks, he writes, "may be reached at a level where the economic, social and political consequences are such as to irrevocably change the entire structure of society." (p. 753) The extent to which Leijonhufvud and other the pro-Minskian Post-Keynesians would be satisfied with a purely mathematical solution to this problem is yet to be determined. But it does seem that there is at least a willingness among this group to probe the dynamics of the larger social system within which economic activity is embedded.

The response of the institutional economists and Critical Realists has been even more radical, suggesting that formal modeling itself may be a part of the problem. Hodgson (2009, p. 1218) sees the de-fetishization of mathematical modeling as an essential step in moving to "a discipline more oriented to understanding real-world institutions and actors…There 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." 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. (Hodgson 2008, p. 276)

Tony Lawson—a leading Critical Realist—is perhaps the most outspoken on this issue, writing 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." Even Paul Krugman, a great defender of formalization in his previous writings on economic methodology[9], would write in his 2009 *New York Times* attack on the profession that "a problem over the past 10 years is that economists became enamored with mathematical technique" (Krugman 2009).

In addition to these voices outside of the mainstream, several economists sympathetic to a more fine-grained economics have begun to emerge from within the mainstream. George Akerlof—a Nobel Laureate for his work on the role of imperfect information—is perhaps the most prominent example. Although throughout his career he has been committed to mathematical modeling as the engine of economic analysis, he has also been committed to exploring the manner in which actual experience of economic systems deviates from the picture portrayed by the discipline's favored models.[10] Most promisingly, Akerlof has recently explicitly called on economists to explore new, more fine-grained methods for connecting their models to reality (Akerlof 2011). Although the specifics of his appeal have yet to be elaborated, at the very least this is a signal that ideas previously only on the fringes of the discipline could well have a champion within the mainstream. The Yale macroeconomist Truman Bewley is another advocate for a more fine-grained economics. His 1999 book *Why Wages Don't Fall During a Recession* was groundbreaking—from the point of view of mainstream economics—in its use of direct interaction with relevant economic actors as a prelude to model construction. Moreover, Bewley explicitly stated that he adopted this approach out of frustration with the failure of mainstream formal methods to shed sufficient light on the issue of wage adjustment through the

[9] See, e.g., Krugman (1996).
[10] See, e.g. Akerlof (1970; 2000).

business cycle. Although he has met with considerable resistance within mainstream economics, Bewley has persisted in his approach, and is currently working on a project examining price adjustments using the same methodological approach as Bewley (1999).

In addition to individual voices sympathetic to substantial methodological reform, there is a growing institutional presence as well—most notably that of the Institute for New Economic Thinking (INET) and its related projects. INET is a shrewdly organized effort to substantially broaden the range of ideas feeding the next generation of economic work. It has significant funding, an impressive advisory board that includes five economics Nobel laureates, and a mission that includes an explicitly stated desire to fund worthwhile projects from outside of the mainstream that would be unlikely to get support elsewhere. Although it is still in its infancy—having been launched only in late 2009—INET has already become a focal point for reform-minded economists both within and outside of the mainstream.

Outside of economics (and even on the fringes of economics), there is a substantial and growing anthropological literature on the culture of various aspects of economic life that can help to address the kinds of blind spots that bedeviled economic analysis in the lead-up to the financial crisis. While it may well be the case that refining our models of individual choice-making under risk and uncertainty will help to explain particular economic environments, such models can be used successfully *only if* we can identify when/where (if anyplace/anywhere) an actual social situation is an example of the generic type of situation envisioned by the refined model.[11] This, of course, requires an understanding of social environments that does not begin by presuming the existence of the kind of the generic types envisioned in our formal models, but rather by engaging with the environment in a more open-ended manner, to allow the conceptual map of the environment to reveal itself. Anthropological studies of financial markets and institutions such as Ho (2009), MacKenzie (2006; 2010a; 2010b) and Zaloom (2006) provide excellent examples of the type of groundwork that can and should be drawn upon by economists both in constructing their models and in determining when/where (if anyplace/anywhere) the models will actually be applicable.

b. Building on the foundation

[11] This is a necessary condition. The question of whether it is, or can be, fulfilled in any individual instance is an empirical question and is by no means guaranteed—that is: we must (as empirical scientists) presume that it is possible that some models may envision generic types and behaviors of those types that are incapable of representing any social experience in the manner proposed by the researcher.

Although the mainstream response to the crisis appears to be heading in the direction of relatively superficial reform (i.e. a behavioral-economics-heavy version of the Add Finance and Stir position), the foregoing discussion indicates that the pieces are in place for more substantial reform. Specifically, the pieces are: a reservoir of relevant methodological knowledge and expertise in anthropology and related fields, the existing beachheads of deep reform within economics and the significant (though closing) window of opportunity for reform afforded by the financial crisis. What is needed is a strategy for using these elements as effectively as possible. Perhaps the most significant challenge in devising such a strategy is the tension between the need for deep change, on the one hand, and the need for quick action on the other. The window of opportunity is unlikely to remain open for long, especially if the impetus for deep change is dissipated by the enactment of superficial reforms, as seems likely. But how can quick action on the part of those interested in deep reform be anything but superficial itself?

In light of this tension, it would seem that a two-stage approach is in order: a near-term element aimed at building a set of concrete examples of methodologically deep economic work, and a longer-term element aimed at building a theoretical and institutional foundation for the continuation of such work. In the short-term, reform-minded economists can begin doing economics—ideally in collaboration with anthropologists of the economy—in a manner that self-consciously espouses a commitment to qualitative, interpretive methods. To ensure the greatest visibility and impact of such work, it should deal with issues widely identified as relevant to important current and near-future public policy issues. With respect to the longer term, economic methodology scholars (in conjunction with practicing economists and anthropologists of the economy) must develop a comprehensive critique of current economic methodology that demonstrates in terms accessible to practicing economists that the current methodology has blind spots that cannot be addressed without the infusion of qualitative, interpretive methods. This latter effort should culminate in making the case for a new field of economics, analogous to econometrics, tasked with developing methods and standards for exploring the appropriateness of formal models of social phenomena. In the remainder of this section, I will elaborate briefly on the content of each of these two stages.

Near-term work can capitalize on the current interest within economics on alternative methodological approaches and the reservoir of expertise of anthropologists of the economy by producing *anthropologically informed economic analyses of important issues*. What, precisely, would this mean? Consider the italicized phrase in two pieces. First, *anthropologically informed economic analysis*. One of the strangest aspects of current economic methodology is its schizophrenic attitude toward fine-grained information about social experience. On the one hand, economists are adamant that one ought not waste one's time trying to systematically collect such information—e.g. through ethnographic methods. On the other hand, economists regularly use such information—in the form of, e.g., "stylized facts," accounts from historical or journalistic literature or from personal experience—to motivate their analysis and corroborate their results as long as that information happens to be available to them in some way other than their own systematic collection of it. While this maybe methodologically suspect, it can be turned to the advantage of the reform cause, as one of its implications is that economists are not *inherently* hostile to fine-grained information about social experience as long as it is only given a supporting role and is not presented as a necessary element of the analysis. This provides an important opening to reform-minded economists: it should, in principle, be possible to begin producing, immediately, economic work that is both deeply embedded in social experience and acceptable to the mainstream.[12]

While such work could be a part of any economics research agenda, in order to further the cause of reform it will be crucial that this work be targeted (at least initially) toward *important issues*—i.e. areas of high near-term policy relevance, such as health insurance markets, renewable energy legislation and markets, and the political economy of fiscal austerity, to name just a few. This work should, ideally, consist of collaborations between anthropologists and economists and proceed in two stages. The first stage would involve the construction of a conceptual map of the realm under study using qualitative, interpretive methods; the second stage would involve the construction of a formal model based on the conceptual map generated in the first stage. A project focused on health insurance markets, for example, would begin by attempting to understand the culture within which the relevant actors operate. This would likely require an exploration not only of relationships between the realms of medical practice, insurance practice, regulatory practice and legislative practice, but also the manner in which political lobbying currently infuses all of these. The information gleaned from this part of the analysis could be presented in a similar manner to the way in which stylized facts are used in economic analysis—

[12] Of course, such work has been appearing in mainstream economics journals from time to time for decades. The difference between such work and what I am envisioning here is that the former does not self-consciously or systematically employ qualitative, interpretive methods whereas the latter will do so.

i.e., as background and motivation of the analysis, and as justification for modeling choices and corroboration of the results of formal empirical tests.

The goal of this type of economic work would be not only to generate a better understanding of the issues under study, but also to generate a critical mass of this new type of economics work that will be available as an alternative to traditional economic analysis as these important policy issues receive increased focus. To the extent that economists can position this new type of economic analysis as more reliable than traditional analyses—e.g. because its connection to actual experience is much more straightforward than that of the latter, and because the lack of such connection was the hallmark of the failed economics of the run-up to the financial crisis—this approach will be influential regardless of the position of more traditionally-minded economists. Toward this end, it will be a great help to have high-profile economists and funding organizations participating in this effort. The impressive institutional presence of INET, and the explicit commitment to the sort of project envisioned above by George Akerlof, among others, gives some reason for optimism on this front.

The longer-term work of the reform effort should aim at building a theoretical and institutional foundation for this new type of economic analysis. Further, this work should get underway immediately so that the groundwork for these foundations is ready when/if the type of near-term work discussed above begins to attract attention and gain adherents. The theoretical foundation must accomplish two goals: (1) <u>Critical Goal</u>: demonstrate the need for the introduction of qualitative, interpretive methods into economics in a manner immediately accessible (and potentially convincing) to practicing economists; and (2) <u>Constructive Goal</u>: provide a concrete proposal for effecting this reform. In previous work, I have attempted to construct a critique that accomplishes these two goals, and a book-length, general statement of this critique is the subject of my current work.[13] A detailed review of the critique is beyond the scope of this paper, but, briefly, its central claim is that mathematical economic models are metaphors for the social phenomena they are meant to represent, and, like all metaphors, the models work by projecting the structure of their world onto the world of the primary subject matter. (For economic models, this means projecting mathematical structure onto social phenomena.) But, as I demonstrate in the critique, it is invalid to presume that social phenomena

[13] See Spiegler (2005; 2006) and Spiegler and Milberg (2009). I do not suggest that this is the only or best possible critique for accomplishing the long-term goals discussed above, only that it may be one such critique.

may always be interpreted within a mathematical framework—i.e. such an interpretation may literally be meaningless. Whether, and in what manner, such projection can be meaningful is an empirical matter and one that can only be determined by first exploring the characteristics of the social phenomena *without* presuming that they are interpretable within a mathematical structure. I further demonstrate that failing to perform this kind of exploration can lead to the construction of seriously flawed economic models whose flaws are not detectable using the standard testing methods of economics. In such cases, economists only discover these flaws when contrary facts emerge in a manner that cannot be easily ignored or explained away through econometric tinkering. The unexpected collapse of the global financial market and ensuing massive worldwide recession is a particularly salient example of such a dynamic.

The Constructive Goal requires a blueprint for a permanent institutional presence within economics to support the kind of economic work being performed as part of the near-term strategy discussed above. One way of accomplishing this would be to add to current disciplinary standards of good practice the requirement that economists be able to provide a plausible justification of the structural match (as discussed above) between their models and the social phenomena they ostensibly represent. The first step in getting such a standard incorporated into standard practice is to demonstrate the need for it—and this is the goal of both the near-term agenda and the critical work discussed in the previous paragraph. This, on its own, is not enough, however. Because the (qualitative, interpretive) methods necessary to assess the match between model and social phenomena are not currently a part of the standard conceptual toolkit of economics, it will be desirable to establish a new sub-field tasked with developing such methods and the standards and tailoring them for use in economics. What I have in mind is something very similar to the development of standards of proper empirical analysis in economics through the establishment of the field of econometrics.[14] In the late 19th and early 20th centuries, as data on economic activity were becoming more plentiful, many economists began utilizing data to test their theories. The problem with such early attempts at empirical assessment of economic hypotheses was that they were *ad hoc*. Specifically, they proceeded without adequate methods or standards for assessing what would constitute sufficient grounds for accepting or rejecting hypotheses. What was needed, the early econometricians of the 1930s argued, was a

[14] For a general account of the development of econometrics, see Morgan (1990). For a seminal statement of the foundational ideas of econometrics, see Haavelmo (1944).

recognition that empirical assessments must be interpreted from within the context of probability theory. The *ad hoc* assessments certainly were not unintelligible—it was easy to understand an economist's claim that his/her predictions were "close to" or "not close to" the data—but they were technically ambiguous (if not meaningless) without some standard against which to judge such statements. Similarly, I argue that although the current claims of formal economic analyses are not unintelligible, their status is technically ambiguous without the proper standards to judge the relevance of their analytic method to the subject matter under study. And just as the econometricians proposed the creation of a new field to tailor probability and statistics theories and methods to the purposes of economists, I propose the creation of a new field to tailor anthropological theories and methods for use in economics.

Whether this two-pronged strategy of pushing the reform agenda—or something like it—could be successful is unclear at this point, of course. What is certain, however, is that such work can begin immediately and that there is no shortage of interesting and important areas of study that would benefit from such work. In addition, there is very little to lose. Even if such a strategy does not produce fundamental change in the mainstream of economics, at the very least it will produce informative work that will be capable of providing insights different from that of standard economics.

Conclusion

Economics' poor performance during the recent financial crisis has generated a good deal of discussion about methodological reform as well as some action toward that goal. Among mainstream economists, there appear to be two poles of consensus forming. The first is that the focus on dysfunctional financial sector activity has been over-emphasized, and that the primary causes of the deep recession following the financial crisis were economic distortions created by government intervention that led to artificially high real wages and taxes. Those espousing this view argue that the standard models of economics performed adequately, and that there is no need for fundamental reform. The second pole is the belief that dysfunctional finance was the central (proximate) cause of our current economic problems, and that these dysfunctions were not adequately understood by economists. Those espousing this view advocate for methodological reforms centered around the introduction of more behaviorally sensitive models of financial decision-making and financial market dynamics into our macroeconomic models.

I have argued that these responses ignore another major inadequacy of economic methodology that may have been an important cause of the recent failures of economics—namely, a blind spot in economic methodology with respect to the possibility that mathematical models of social phenomena may be incapable of representing those phenomena in the manner claimed for them by the economist. Covering this blind spot requires the use of qualitative, interpretive methods, and the introduction of such methods into the core of economics would require a much deeper level of reform.

There is some reason for optimism that such reforms might be achievable. There are voices within economics—not only on the fringes, but also in the mainstream—calling for a more fine-grained economics, a new, well-funded institutional presence in the form of the Institute for New Economic Thinking, and a reservoir of methodological expertise among anthropologists, which could form the basis for strategic interventions in the cause of deeper reform. I have proposed a general strategy for pursuing this reform, consisting of two stages: near-term production of economic analyses of high profile issues that self-consciously utilize qualitative, interpretive methods; and a longer-term effort to provide a persistent theoretical and institutional foundation for such work.

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