

The New Interventionist Economics

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RUNNING HEAD: BRACE Economics

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Abstract:

The Great Recession seems to be creating a New Interventionist Economics characterized by bubbles, radical uncertainty, animal spirits, complexity dynamics, and extra-market control. When placed in that order, these characteristic create the acronym BRACE. I review some evidence and literature pointing to BRACE economics and briefly suggest some avenues to challenge the new interventionists.

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I. Introduction

The publication of “The Age of Friedman,” Andrei Shleifer’s (2009) full-throated celebration of Milton Friedman and “free market policies,” is a strong sign that academic economics in the West may be turning away from Milton Friedman and back toward interventionism and Keynesianism. A point of free-market saturation may have come and a more tempered view of markets may follow. The Great Recession (Rampell 2009) seems a likely occasion for such a turn. Alan Greenspan’s *mea culpa* (Andrews 2008) and Richard Posner’s conversion to Keynesianism (2009a, 2009b) suggest that a kind of Minsky moment (Cassidy 2008) may be at hand, not for the economy, but for the economics profession. The moment may be hand when economists choose to reject the Age of Friedman in favor of the view of Hyman Minsky and others that “capitalism is flawed” (Mynsky 1975, p. 169). We may call the likely product of this Minsky moment “New Interventionist Economics.”

The basic outlines of the emerging New Interventionist Economics are beginning to form. The New Interventionist Economics will probably be distinguished by five characteristics. They are Bubbles, Radical uncertainty, Animal spirits, Complexity dynamics, and Extra-market control. When put in that order, these characteristics form the acronym BRACE. I will briefly consider how the new interventionists might be answered, but my purpose here is to identify the likely characteristics of their models and arguments.

Minsky (1975, 1982) is an important precursor to BRACE economics and often cited in the literature on the Great Recession. Prychitko (2010) provides a penetrating analysis and critique. He notes how little Minsky's theory of financial fragility really explains. It cannot explain how the theoretical baseline of "tranquility" is achieved or why the bust must finally come. He quotes Mises favorably saying, "[P]sychological and other related theories of the crisis amount to nothing more than tracing one unknown factor back to something else equally unknown" (Mises 1928 as cited in Prychitko 2010, note 10). Prychitko criticizes Minsky's policy proposals using Big Player theory (Koppl 2002). It is hard predict how far Minsky's star will rise in the wake of the Great Recession. On the one hand, his *oeuvre* is a kind of Ur-text for the New Interventionist Economics. It contains all the characteristic features of BRACE economics with the possible exception of complexity dynamics. (Minsky worked out the basics of his system prior to the complexity revolution in economics, but it is possible to build a complexity model of financial fragility as do Delli Gatti, Gilegatti, & Gardini 1994.) On the other hand, Minsky's work is strongly associated with Post Keynesian economics and the Levy Economics Institute of Bard College, both of which are "heterodox" in the sociological sense of Colander, Holt, & Rosser (2005) and therefore unclean and taboo. The scales may tip in favor of Minsky simply because the phrase "financial fragility" is irresistible. Yellen (2009) explicitly endorses Minsky's model and applies it to the Great Recession. As far as I can tell, Minsky's fate is a relatively unimportant detail in the context of this paper. The New Interventionist Economics will have the contours I suggest whether or not Minsky is properly recognized as its most important precursor other than J. M. Keynes.

II. The interventionist turn in macroeconomics

Shleifer (2009) assembled evidence that “free market policies” have brought substantial benefits to both rich and poor countries in recent decades. Between 1980 and 2005, the world’s real per capita income grew over 57%, which is about 2% per year. Infant mortality fell almost 42% in that period. Average schooling grew from 4.4 years in 1980 to “almost 6” years in 1999. “Between 1980 and 2000, the share of the world’s population living on less than \$1 a day fell from 34.8 percent to 19 percent. These improvements coincided with a move to more liberal economic policies. The median national inflation rate fell from 14.3% per year in 1980 to 4.1% per year in 2005. The top marginal income tax rate went from a “population-weighted average of 65 percent in 1980 . . . to 36.7 percent in 2005” (p. 125). Black market exchange rates converged to official levels and “nearly vanished” by 2005, which suggests financial liberalization (p. 125). Tariffs “fell from the population-weighted world average of 43 percent in 1980 to 13 percent in 2004, in parallel with vast expansion in world trade” (pp. 125-126). Finally, the number of procedures required to start a lawful business fell in most of the world from 1999 to 2005 (p. 134). The exceptional regions were “East Asia Pacific” and “South Asia,” both of which showed slight increases. Shleifer concludes, “On strategy, economics got the right answer: free market policies, supported but not encumbered by the government, deliver growth and prosperity” (p 135).

Peter Leeson (2009) has examined Shleifer’s evidence more closely and reached what may be a stronger conclusion. (Shleifer posted a spreadsheet of the evidence at

http://www.economics.harvard.edu/faculty/shleifer/files/data_friedman.xls.) Leeson says, “The data clearly support capitalism’s superiority for development and merit its unqualified defense by social scientists who believe that wealth is better than poverty, life is better than death, and liberty is better than oppression. Full-force cheerleading for capitalism is well deserved” (pp. 4-5). Indeed, “Global capitalism’s effect is clear to the point of smacking one in the face: it has made the world unequivocally better off” (p. 32).

Shleifer’s paper and evidence provide strong unambiguous support for “capitalism” and “free market policies.” So too does Shleifer’s evidence as further analyzed by Leeson. So too, I think, does the economic history of Europe and North America as reflected in, say, Hume *History of England*, Weber’s *General Economic History*, Friedman and Schartz’s *A Monetary History of the United States, 1867-1960*, or any number of more recent efforts. So too do any number of empirical, theoretical, and historical examinations of the question. The evidence is in and capitalism is definitely the Promised Land.

As Shleifer and, presumably, Leeson understand clearly, the superiority of free market policies and capitalism does not imply that policy is easy or that we have settled all policy questions. As we have seen, Shleifer says economics got it right on *strategy*, which leave tactics an open question. It is never easy to know what the best “free market policies” are for a given time and place or how to get them. Vernon Smith, for example, points to the perils of deregulation (Smith 2008, pp. 53-60). But the issue should not be whether we want capitalism and free market policies. The issue should be which free market policies, and how to implement them. Tactics are open, but strategy is settled.

Before the fall of the Berlin Wall competent observers could reasonably believe that Soviet planning was a workable system. After the fall everyone had to recognize that Warren Nutter (1957) had been right all along that Soviet output was falling increasingly behind the US and that the official Soviet output numbers were less reliable than those in US. The end of the Soviet system settled the question whether red socialism might be better than democratic capitalism. It was still possible to think that “development” should be planned. Maybe capitalism and free market policies are luxuries that help to make rich countries richer, but do not help the poor. Free market reforms in many poor countries have allowed us to put that one to the test and the answer is in. Poor people should be free too. Shleifer and Leeson have shown us that the evidence since 1980 points to one and only one reasonable conclusion. Capitalism for all, restraint for none.

The “strategic” question has been settled. Capitalism “works.” Capitalism works better than other systems. More capitalism increases wealth, education, health, and longevity. The tactical questions remain and they are difficult, but we can no longer say with Hyman Minsky, “capitalism is flawed” (1975, p. 169). And yet two prominent, long-standing defenders of capitalism have recently expressed the view that capitalism is flawed. In testimony before Congress in October 2008 Alan Greenspan said he had “found a flaw” in his model of capitalism and “Those of us who have looked to the self-interest of lending institutions to protect shareholders’ equity, myself included, are in a state of shocked disbelief” that bankers seemingly failed to do so (Andrews 2008).

A similar statement of flawed capitalism comes from Richard Posner, who says,

Some conservatives believe that the depression is the result of unwise government policies. I believe it is a market failure. The government’s myopia, passivity, and blunders played a critical role in allowing the recession to balloon into a depression, and so have several fortuitous

factors. But without any government regulation of the financial industry, the economy would still, in all likelihood, be in a depression. We are learning from it that we need a more active and intelligent government to keep our model of a capitalist economy from running off the rails. The movement to deregulate the financial industry went too far by exaggerating the resilience—the self-healing powers—of laissez-faire capitalism. (Posner 2009, p. xii)

The interventionist turn of Greenspan and Posner seems likely to herald a new market failure literature in macroeconomics. Andrei Shleifer's "The Age of Friedman" marks the moment of capitalism's final unambiguous intellectual victory in the economics profession. Perhaps we should not be surprised that it seems destined to mark, at the same time, the moment economist returned in earnest to the Minskyian view that "capitalism is flawed."

Such a turn in macroeconomics may have been inevitable given the monsony power of central banks in demanding macroeconomics, as White (2005) documents for the US. Consider the career choices facing ambitious PhD students in Chicago, Stanford, Princeton, MIT, or Harvard who are unusually skilled in macroeconomics. They will not wish to make themselves unattractive to state institutions such as the Fed and the World Bank. They will not be eager, therefore, to criticize such institutions or explain too clearly why these institutions may be bad for the populations they purportedly serve and support. Neither of the two major American political parties has an interest in macroeconomic advisors who suggest a smaller role for state action in the economy. Most of their opportunities outside of academia will be with agencies and entities seeking to justify state action and economic management in one way or another. This interventionist bias in the hiring practices of non-academic demanders of economic expertise bleeds into the top economics departments in our universities for at least two

reasons. First, members of hiring committees in such departments circulate between government service and the academy and will generally have a bias in favor of young candidates tending to celebrate that particular revolving door. Second, the status – and therefore resources – of an economics department tends to be increased by professors with experience in high-level government positions.

Considering the powerful influences shaping the career choices of the best young minds in macroeconomics, the marvel is that we had an “Age of Friedman” at all, not that it may be passing. We might not have had the Age of Friedman at all if the evidence for capitalism were not “clear to the point of smacking one in the face.”

III. Characteristics of the New Interventionist Economics

As I indicated in the introduction, I think we can begin to see the outlines of the New Interventionist Macroeconomics already, namely, Bubbles, Radical uncertainty, Animal spirits, Complexity dynamics, and Extra-market control.

bubbles

Asset price **bubbles** are a well-recognized phenomenon in the literature. The recent housing bubble and the earlier tech bubble are likely to make economists more aware of the phenomenon and its importance.

Asset price bubbles can exist even in a rational expectations model. In such a model, an asset's price, $P(t)$ will be its discounted price one period forward, plus the value of its in-period services, $z(t+1)$. Thus,

$$P(t) = (1+r)^{-1} E[P(t+1)+z(t+1)| \Phi(t)], \quad (1)$$

where $\Phi(t)$ is the information available at time t . Forward substitution gives a solution that can be divided into a “fundamental value,” $F(t)$, plus a residual “rational bubble,” $B(t)$:

$$P(t) = F(t) + B(t), \quad (2)$$

where

$$F(t) = \sum_{\tau=1}^{\infty} (1+r)^{-\tau} E[z(t+\tau)|\Phi(t)] \quad (3)$$

and

$$B(t) = (1+r)^{-1} E[B(t+1)| \Phi(t)]. \quad (4)$$

At least two phenomena point to irrational bubbles, however. They are the bubbles in closed-end country funds such as that in the late 1980s (Lee, Shleifer, & Taylor 1990; Ahmed et al. 1997) and bubbles in experimental asset markets (Smith 2008, pp. 227-233). I expect that BRACE economics will rely on the literature addressing both

phenomena to argue that modern asset markets are subject to “speculative bubbles” and are in this sense “unstable.”

radical uncertainty

Mainstream economics has had little room for **radical uncertainty**, which has remained a predominately heterodox idea. Richard Posner, however, has recently embraced the notion. Posner’s turn may help bring radical uncertainty from the fringe to the center of mainstream macroeconomics. Posner (2009b) says “uncertainty – in the sense of a risk that, unlike the risk of losing at roulette, cannot be calculated – is a pervasive feature of the economic environment, particularly with respect to projects intended to satisfy future consumption.” He views this claim as “foundational.” It could be that Posner’s appeal to radical uncertainty will remain idiosyncratic within mainstream economics. I consider it more likely to spread, however.

The economics literature contains the elements required to bring radical uncertainty into mainstream macroeconomics. The concept is unlikely to be called “radical uncertainty.” More common labels include “Knightian uncertainty,” “model uncertainty,” and “ambiguity.” The term “Keynesian uncertainty” is also used, but less often. Epstein & Wang (1994) explain one way to model “Knightian uncertainty” in a general equilibrium setting. Brock, Durlauf, and West (2006) “outline some basic principles for incorporating model uncertainty into the reporting of policy evaluation exercises” (p. 661). They show how the issue of model uncertainty raises the problem of robustness for policy rules.

Ellsberg (1961) showed experimentally that people are averse to ambiguity. This classic study is cited widely in experimental economics and neuroeconomics. The experimental literature shows that people respond differently to risk and uncertainty (Camerer and Weber 1992, Luce 2000). Hsu et al. (2005) use fMRI and brain-lesion studies to investigate the differences between risk and uncertainty. They find that persons respond differently, “on both the behavioral and neuronal level,” to risk and uncertainty (p. 1683). Nevertheless, their results “suggest a unified treatment of ambiguity and risk as limiting cases of a general system evaluating uncertainty” (p. 1683).

The fMRI study of Huettel et al. (2006) tends to contradict the unified view of Hsu et al. They show that differences in behavior toward risk and uncertainty reflect, in part at least, differences in how risk and uncertainty are processed at the neuronal level. They “suggest that ambiguous decision making does not represent a special, more complex case of risky decision making; instead, these represent two types of decision making that are supported by distinct [neuronal] mechanisms” (p. 772). Inukai and Takahashi (2006) also criticize Hsu et al. They reach a result similar to that of Huettel et al. (2006) using a behavioral experiment, rather than brain imaging. They find that Kahneman & Tversky asymmetry between gains and losses disappears in conditions of “Knightian uncertainty” (p. 285). “Therefore, it might be conceivable that neuropsychological processing underlying decision under Knightian uncertainty is distinct from that under probabilistic uncertainty, especially in the loss-frame” (p. 285).

The distinction between risk and uncertainty has a place in the econophysics literature as well. Schinckus (2009) criticizes “neoclassical” economics for reducing

uncertainty to risk and treating risk with “Gaussian” models. He contrasts this approach to risk and uncertainty with those of Knight, Keynes, and Hayek, who are cited favorably in this regard. The “main objective” of econophysics, Schinckus says, “is to provide a more operational form of uncertainty than neoclassical economics by developing several ways of modeling this notion” (p. 4421). Uncertainty is modeled as one or another form of entropy, such as “Shannon entropy” (4421). He favorably quotes Dionisio et al. (2005) “the use of entropy as a measure of uncertainty in finance appears to have many potentialities and a vast field of development, both in theoretical and empirical work” (Dionisio et al. 2005, p. 161 as cited in Schinckus 2009, p. 4421). Takahashi (2009) models uncertainty with one of the entropy measures mentioned by Dionisio, namely, Tsallis entropy.

animal spirits

The concept of **animal spirits** has a long history tracing to ancient medicine (Koppl 1991). John Maynard Keynes imported the concept to economics, defining it as “a spontaneous urge to action rather than inaction” (1936, p. 161). The term has a fair currency in economics, though no fixed meaning. Keynes’ original definition seems to have lost pride of place.

I think it is fair to say that the term “animal spirits” is sometimes tossed about rather casually without reflection. Mankiw and Romer (1991) say, for example, “there can be fluctuations in aggregate output and in welfare driven not by an extrinsic shocks but by changes in ‘confidence’ and ‘animal spirits’”(p. 8). This statement seems to refer

to market psychology. Then they say, “Cooper and John (this collection [reprinting Cooper and John 1988]) provide a unifying framework for examining models of coordination failure and analyze the welfare economics of coordination failure” (p. 8). But Cooper and John (1988) refer neither to “animal spirits” nor to “confidence.” Their paper examines how “spillovers and strategic complementarities” can create coordination failures (p. 442). They explicitly contrast their paper and others they build on from both “new classical macroeconomics” and “articles in the Keynesian tradition,” which “suggest that unemployment arises from nonrational expectations or wage and price rigidities” (p. 441). There is a disconnect between their paper and Mankiw and Romer’s use of the term “animal spirits” to describe their paper. I think that disconnect illustrates the just how unstable the meaning of “animal spirits” has been in the economics literature.

The meaning of “animal spirits” implicit in Howitt and McAfee (1991) is probably the modal use and it is the definition I will stipulate for this essay. They note that one class of trade cycle theory “attributes fluctuations to random waves of optimism and pessimism that are unrelated to fundamental conditions” (p. 493). They note that “this view is often attributed to John Maynard Keynes,” but claim “it could equally be attributed to John Stuart Mill or F. A. Hayek, and goes back at least as far as Henry Thornton (1802)” (pp. 493-494). Under this interpretation, “animal spirits” are degrees of optimism and pessimism concerning overall economic activity. This definition equates “animal spirits” to the “state of confidence,” as the reference to Hayek, Mill, and Thornton reveals.

I expect the “animal spirits” of BRACE economics to be related to radical uncertainty. Such a connection has not characterized the term’s use in the mainstream macroeconomics of recent decades. In that literature, the term is probably most strongly associated with the sunspot models first articulated by Azariadis (1981). In sunspot models, “extraneous uncertainty” (Azariadis p. 380) leads to self-fulfilling prophecies as agents with rational expectations observe variations in an intrinsically irrelevant phenomenon, “sunspots,” and rationally expect corresponding changes in at least one economic variable such as the price level. Howitt and McAfee improve on sunspot models by linking sunspots to economic activity through a “transaction externality” (p. 496) rather than an empirically implausible sensitivity of aggregate labor supply to the expected inflation rate (p. 494). In their rational expectations model, long-term labor contracts and countercyclical marketing costs discourage hiring when employers expect a slump and encourage hiring when employers expect a boom.

Farmer (2009) offers a model broadly similar to those of Azariadis and Howitt & McAfee. Farmer’s model identifies a loss of confidence as the cause of the Great Recession. Farmer wisely says, “As economists, we need to get the economics right before we rush in as saviors” (p. 1). His model allows multiple equilibria in the labor market. Animal spirits determine which equilibrium is realized. In Farmer’s model of the labor market, multiple equilibria are possible in part because search is costly and employers cannot compensate unemployed workers for their time spent searching. Thus, there is a missing market and firms do all recruiting. This infirmity of the labor market creates the possibility of an unemployment equilibrium. As I explain below, a

technological externality in firm recruitment costs creates the possibility of more than one such equilibrium.

Farmer's model of the labor market might be explained by a kind of parable. (I think my parable fairly represents Farmer's model of the labor market.) Workers live on one side of a river and employers live on the other side. Perfectly competitive employers own boats and recruit by assigning some of their workers to cross the river in those boats and pick up workers who are delivered to the work site. It would be more efficient to reimburse workers who interview for the price of a public ferry ride. River crossing is so costly, however, that some workers could make a living by charging multiple employers for one ferry crossing, pocketing the difference, and never accepting an offer. These workers are Akerlofian lemons who prevent firms from paying workers to recruit themselves thereby forcing all search onto the employers' side of the market. There is a missing market in the system. Importantly, Farmer's model makes the implicit assumption that unemployed workers are not able to signal their willingness to work for below-market wages. Instead, workers get their marginal products, but the level of employment is indeterminate. When unemployment is high, so is the marginal product of labor and, therefore, the wage rate. In this equilibrium situation, employers need send only a few boats across the river to pick up those high-price workers; each boat returns with many workers because unemployed workers are packed thickly on the shore. When unemployment is low, so is the marginal product of labor and, therefore, the wage rate. In this equilibrium situation, employers must send more boats across the river to pick up those low-price workers; each boat returns with only a few workers because unemployed workers are spread thinly along the shore. The effect of aggregate output on recruiting

costs is an externality that helps ensure the existence of multiple equilibria in the labor market.

Farmer (2009) resolves the indeterminacy in the labor market with “animal spirits.” Asset prices are exogenous reflections of the state of confidence, but the state of confidence is a self-fulfilling prophecy as in sunspot models. When confidence is high, perceived wealth and demand are high. Firms validate such confidence by producing a large output, which implies a large flow of output and a correspondingly high present value of the flow, i.e., high wealth. When confidence is low, perceived wealth and demand are low. Firms validate such lack of confidence by producing a small output, which implies a small flow of output and a correspondingly low present value of the flow, i.e., low wealth. In Farmer’s model the asset-prices tail wags the real-economy dog. Asset prices are exogenous and dependent on “confidence.” If confidence fails, households loose wealth and demand less.

Farmer’s model is Keynesian, in part because it represents “that sometimes markets fail and that, when this occurs, there is a potential for government policy to improve human welfare” (p. 1). He criticizes many Keynesians, however, for “blindly apply[ing] fiscal policies that do not have a distinguished history of success” (p. 1). Farmer’s policy advice is innovative, but Keynesian after a fashion. He, “suggest[s] there that the Fed should support the stock market directly by trading index funds” (p. 22). Presumably, Farmer’s policy is not a bailout for the rich and powerful, but it is not immediately evident why not. Farmer seems to recognize that his suggestion is probably not intuitively appealing to most of his readers when he directs readers to two of his forthcoming books that, he says, “explain why a policy of that kind makes sense” (p. 22).

While the policy proposal is difficult to understand when described so briefly, it does seem reminiscent of J. M. Keynes' proposal for "a somewhat comprehensive socialisation of investment" (1936 p. 378).

Farmer's assumption of radical infirmities in the price signaling function of labor markets, lets him imagine larger equilibrium unemployment rates than seem consistent with the earlier rational expectations, self-fulfilling prophecy models of Azariadis and Howitt & McAfee. I confess, however, that I do not understand why it would be appropriate to assume that unemployed persons are unable to signal their willingness to work at wages below marginal product. Farmer's model may also imply a more counter-cyclical real wage rate than is empirically plausible given the empirical ambiguity of real-wage cyclicality. It could be that Farmer's model will set the mold for the New Interventionist Economics, which will use more or less same tools as recent mainstream macroeconomics. I think there is reason to believe, however, that rational expectations models will not predominate.

Rational expectations models of self-fulfilling prophecies, such as Azariadis (1981) and Howitt and McAfee (1991) probably cannot reasonably explain the large and persistent unemployment of the Great Recession. Farmer (2009) suggests the possibility of much higher equilibrium rates of unemployment, but at the cost of implausible assumptions about labor market signaling and wage cyclicality. Nor does it seem easy to square rational expectations with the housing bubble that preceded the crisis. It seems rather clear that rents were too small to justify the purchase prices of many housing units. Possible explanations for this gap, such as inflation expectations, would seem hard to square with the relatively rapid collapse of real estate values beginning in 2007. Thus, it

seems likely that rational expectations models of animal spirits will fade, forming no part of BRACE economics. In the New Interventionist Economics, animal spirits are likely to be an irrational, psychological phenomenon. Two considerations strengthen this expectation. First, rational expectations models are representative agent models and thus inconsistent with current vogue for models with heterogeneous agents. Second, prospect theory, experimental economics, and neuroeconomics encourage models of bounded rationality and irrational choice as well as a greater attention to the psychology of human action.

Akerlof & Shiller (2009) provide a salient example. They say that “in modern economics animal spirits” refers “to a restless and inconsistent element in the economy. It refers to our peculiar relationship with ambiguity or uncertainty. Sometimes we are paralyzed by it. Yet at other times it refreshes and energizes us, overcoming our fears and indecisions” (p. 4). In this view, animal spirits are all about the psychological and irrational side of economics. They distinguish and examine five “aspects” of animal spirits, namely, confidence, fairness, corruption and antisocial behavior, money illusion, and stories. It is possible that their innovative use of the term will catch on, but I expect the term will generally be restricted to the first item on their list, confidence, which Akerlof and Shiller describe as a “cornerstone” of their theory. However my prediction pans out, it is that aspect of animal spirits, confidence, to which I principally refer in identifying animal spirits as a characteristic of the New Interventionist Economics.

Akerlof and Shiller (2009) ask eight economic questions:

1. Why do economies fall into depression?
2. Why do central bankers have power over the economy, insofar as they do?

3. Why are there people who can't find a job?
4. Why is there a tradeoff between inflation and unemployment in the long run?
5. Why is saving for the future so arbitrary?
6. Why are financial prices and corporate investments so volatile?
7. Why do real estate markets go through cycles?
8. Why does poverty persist for generations among disadvantaged minorities?

Their question 4 suggests that the New Interventionist Economics is turning away from Milton Friedman and back toward John Maynard Keynes. In their case, this turn emphasizes the irrational and psychological side of economics. They say, “none of these questions can be answered if people are viewed as having only economic motivations which they pursue rationally—that is, if the economy is seen as operating according to the invisible hand of Adam Smith” (p. 6). As this quote suggests, Akerlof and Shiller fall inexplicably into the trap of viewing Adam Smith as a rational-choice theorist. They explicitly attribute to Smith a model of “free, perfect markets” (p. 2) in which “people rationally pursue their economic interests” (p. 3). Akerlof and Shiller say that “none” of their eight questions, “can be answered if people are viewed as having only economic motivations which they pursue rationally—that is, if the economy is seen as operating according to the invisible hand of Adam Smith.” Not surprisingly, they conclude, “the takeaway message from Adam Smith—that there is little, or no, need for government intervention—is . . . unwarranted” (p. 3).

complexity dynamics

Bubbles, radical uncertainty, and animal spirits do not imply or require any particular modeling technique. It seems reasonable to expect, however that economists will rely on the tools of complexity theory to trace out their supposed consequences. In particular I expect models of “**complexity dynamics**” to characterize the New Interventionist Economics. As far as I know, the term “complexity dynamics” has no particular technical meaning. A quick Google search bolsters this view. I use the term to mean simply a model that uses complexity theory and traces out the time path of one or more variable of the system. Complexity and complexity dynamics are not quite the same thing in spite of a large overlap. Your model is an example of “complexity dynamics” only if the dynamics are a central part of what the model is supposed to demonstrate, rather than just the necessary path to the more interesting end state or set of end states. It’s complexity dynamics if the point is the ride not the end point or other aspect of the system.

At least three considerations support the view that the New Interventionist Economics will feature complexity dynamics. First, there is a general trend toward complexity theory in economics and all of the natural and social sciences. The trend within economics has been chronicled by Colander, Holt, & Rosser (2005) and discussed in Koppl (2006). Many persons have noted that the complexity boom was launched when personal computers started to show up on the desks of college professors. Second, complexity models generally have heterogeneous agents. It seems difficult to handle radical uncertainty and animal spirits with representative agent models.

Finally, the complexity models used seem likely to focus on dynamics because they will attempt to show how you get bubbles and cycles endogenously.

Kirman (2009) rejects representative agent models. He argues forcefully that Dynamic Stochastic General Equilibrium (DGSE) models represent the economy poorly and, in particular, cannot adequately represent the Great Recession. For Kirman, “the crisis is a story of contagion, of interdependence, interaction, networks and trust. Yet,” he notes, “these notions do not figure prominently in modern macroeconomic models” (p. 3). Kirman reminds us that Sonnenschein (1972, 1973), Mantel (1974), and Debreu (1974) showed that most properties of individual excess demand curves do not aggregate, thus creating the possibility of multiple equilibria and unstable equilibria in general equilibrium contexts. It seems fair to say, as Kirman does, that “to assume that behavior at one level can be assimilated to that at the other is simply erroneous” (p. 11). These considerations throw representative models into doubt.

Kirman cites the demonstration of Saari and (Carl) Simon (1978) that any price mechanism consisting of differential equations of adjustment and ensuring convergence to general equilibrium will embody large amounts of information. Kirman says such a price mechanisms “would require an infinite amount of information,” which seems to be true in a strict sense. His statement exaggerates, however, if information can be compressed. Saari and Simon use the phrase “a considerable amount of information” (1978, p. 1098). Notwithstanding Kirman’s apparent overstatement, the informational requirements of an effective price mechanism in a general equilibrium context seem to be unrealistically high, which gives us further cause to seek out new models relying on new, computational approaches to modeling social phenomena.

Kirman thinks network topology is the key to understanding financial fragility. The international financial system has grown more connected, which would ordinarily enhance stability. But a few “nodes,” i.e., a few institutions, have acquired a disproportionately large number of connections and become “very central” (p. 18) hubs of the system. Kirman quotes favorably Andrew Haldane of the Bank of England saying, “This evolution in the topology of the network meant that sharp discontinuities in the financial system were an accident waiting to happen. The present crisis is the materialisation of that accident” (Haldane 2009a, p. 4 as quoted in Kirman 2009, p. 18). Kirman sees this skewed network structure as market failure. He quotes Haldane on this a second time.

Deregulation swept away banking segregation and, with it, decomposability of the financial network. The upshot was a predictable lack of network robustness. That is one reason why Glass-Steagall is now back on the international policy agenda. It may be the wrong or too narrow an answer. But it asks the right question: can network structure be altered to improve network robustness? Answering that question is a mighty task for the current generation of policymakers. Using network resilience, as a metric for success would help ensure it was a productive one (Haldane 2009a, p. 31).

De Grauwe provides another example of the sort of “complexity dynamics” model I expect to characterize the New Interventionist Economics. He introduces an expectational ecology of the sort pioneered by Brian Arthur (Arthur 1994, Arthur et al. 1997) to a “stylized version of DSGE-models” exemplified by Smets and Wouters (2003). It is stylized in part because De Grauwe has only three core equations, an aggregate demand curve, an aggregate supply curve, and a Taylor rule. He adds non-rational expectations of inflation and output. In both cases the expectational ecology consists of a variable mix of fundamentalists and extrapolators similar to Brock and

Hommes (1997). As in Arthur (1994), Arthur et al. (1997), the probability of an agent switching to a new forecasting algorithm is a positive function of the algorithm's recent relative success. In other words people learn and adapt to recent experience. (The switching mechanism in Brock & Hommes (1997) is cost based.) Granger causality runs both ways between output and "animal spirits," which are irregular and unpredictable waves of optimism and pessimism. De Grauwe calibrates the model and compares it favorably to the calibrated rational expectations version of the same thing. Although the waves of optimism and pessimism are unpredictable, De Grauwe attempts to draw policy implications by searching for parameter values in the Taylor rule that minimize the variances of inflation and output. A non-linear relationship between those variances implies a tradeoff between them for certain parameter values.

extra-market control

As we have seen already, representatives of the New Interventionist Economics favor **extra-market control** of the financial system. We have seen Kirman (2009), for example, favorably quoting Haldane on the supposed evils of deregulation. His speech and other recent statements (Haldane 2009b, 2009c) include analyses of the causes of the Great Recession. They are probably also descriptions of the kind of economics the Bank of England would like to purchase more of in the near future. I imagine the PhD students I referred to earlier will take note and act to fill this demand. And the product Haldane is ordering fits my description of BRACE economics well. He uses complexity models to represent the Great Recession as a bursting bubble brought on when agents with

Knightian uncertainty (p. 4) lost confidence (p. 12) in asset prices. This waning of the animal spirits (p. 24) was a market failure (p. 31) and “regulation” (p. 4) is needed to improve system performance.

In the US, Janet Yellen, president of the San Francisco Fed, has described the Great Recession as a “Minsky meltdown” (Yellen 2009). As I noted above, Yellen explicitly endorses Minsky’s model and applies it to the Great Recession. In a remark that might allude to network theory, she says, “the Minsky meltdown is global in nature, reflecting the ever-increasing interconnectedness of financial markets and institutions around the world” (2009, p. 6). She says, “The severity of these financial and economic problems creates a very strong case for government and central bank action” (p. 7). Yellen argues in favor of policies to dampen bubbles and even allows for the possibility that monetary policy should sometimes play that role (p. 12). She also envisions a greater role for “macro-prudential supervision” of financial markets, to help prevent excess leverage. For example procyclical capital requirements could be “a kind of ‘automatic stabilizer’ for the financial system” (p. 15). She also recommends differential “micro-prudential” supervision of “systemic” institutions. “Systemic institutions would be defined by key characteristics, such as size, leverage, reliance on short-term funding, importance as sources of credit or liquidity, and interconnectedness in the financial system—not by the kinds of charters they have” (pp. 13-14). Yellen seem to want discretion in the choice of regulatory rules to apply to different financial institutions. Such discretion would push the “rule of men” (as opposed to the “rule of law”) beyond even the alarming levels White (2009) chronicles for the Great Recession.

IV. Tensions within the New Interventionist Economics

Critics of the New Interventionist Economics would seem to have two lines of attack.

First, they may question the diagnosis of market failure. Second, they may question the policy prescriptions offered by the new interventionists.

Challenges to the diagnosis of New Interventionist Economics have two aspects, namely, theory and history. Of these the more important is history. Facts matter. Some have made much of unsustainable forms of state support for home ownership. In September 2008 Steve Horwitz (2009a) penned “An Open Letter to my Friends on the Left,” emphasizing this point. More recently he has said, “The two key causes of this crisis are expansionary monetary policy on the part of the Fed and a series of regulatory and institutional interventions that channeled that excess credit into the housing market, creating a bubble that eventually had to burst” (Horwitz 2009b).

The facts about housing that Horwitz and others draw to our attention matter. I think two other facts about the Great Recession probably matter more, although the second fact identifies an underappreciated mode of state support for the subprime mortgage market.

First, as Horwitz noted, the boom preceding the bust was a time of monetary expansion. In particular, the Fed let interest rates slip below previous Taylor-rule levels, especially in the period 2002-2004, though catch up was not achieved until about the time the housing bubble burst (Taylor 2008). Taylor says, “the housing boom was the most noticeable effect of the monetary excesses” (2008, p. 4).

Second, SEC rules ensured that the rating agencies would not provide unbiased ratings of default risk. Levy & Peart (2008) and Levy (2009) quote the SEC's 1994 "concept release" on "Nationally Recognized Statistical Rating Organizations" (SEC 1994). The release says,

Over time, the NRSRO concept has been incorporated into other areas of the federal securities laws and Congress itself employed the term "NRSRO" in the definition of "mortgage related security." Pursuant to Section 3(a)(41) of the Exchange Act, which was added by the Secondary Mortgage Market Enhancement Act of 1984, a mortgage related security must, among other things, be rated in one of the two highest rating categories by at least one NRSRO. Although Congress did not define what it meant by an NRSRO, its reliance on the term used in Commission rules is significant because it reflects a congressional recognition that the "term has acquired currency as a term of art." (1994, p. 3)

Note that the quote includes the phrase "by at least one NRSRO." Levy & Peart (2008) and Levy (2009) point out that this phrase authorizes rate shopping. If one NRSRO does not rate your mortgage related security in one of the two highest rating categories, go to another for a better rating. This incentive system creates a bias in favor of high ratings. Moreover, the label "NRSRO" is an official designation bestowed by the SEC and requiring an application to the SEC. There are currently ten NRSROs, of which three, Moody's, Standard & Poor's, and Fitch, dominate the market. Presumably, the presence of a few small players encourages the big players to stick with their incentives and provide ratings gratifying to their regular customers.

Theory matters, too. It takes a theory to beat a theory. As Prychitko (2010) explains, the Austrian theory of the business cycle can explain both why the boom gets started and why it inevitably ends in a bust. Facts of the sort I have listed will not persuade others if BRACE economics has the best theory. The Austrian theory is a good alternative. It is plagued, however, by an attachment to unsustainable notions of the

“order of a good” and “length of the production process.” Such ideas can be given meaning only in simplified models and probably cannot be operationalized. It is evident, however, that some sectors are, in some sense, more interest sensitive than others, with housing as an obvious example of an interest-sensitive asset category. It seems plausible to conjecture that the Austrian theory of the trade cycle could be improved by rejecting unworkable ideas such as the “average period of production” in favor of some measure of duration, which is the interest sensitivity of an asset’s price. The most basic of these, Macaulay duration, is defined as

$$D = \sum_{i=1}^n \frac{P_i t_i}{V},$$

where V is present value of the asset, P_i is the present value of the asset’s i_{th} cash flow, and t_i is the number of years from now the i_{th} cash flow accrues. Macaulay duration will be m for a zero-coupon bond maturing in m years. I conjecture that moving from arcane capital notions to duration would not reduce the empirical content of the Austrian theory of the business cycle. Such a move would be a welcome simplification. It would probably also make it more straightforward to apply the theory to historical episodes.

Prychitko’s (2010) criticism of the policy implications of financial fragility models applies to BRACE economics in general. His main point is that the discretionary policies often proposed by followers of Minsky would enhance Big Player influence. Big Players reduce the prescience of economic expectations and, therefore, create instability,

especially in financial markets. Thus policies such as those we have seen Yellen toying with would make the problem worse, not better.

Finally, I think critics of the new interventionism should point out the epistemic limits of interventionism by appeal to computability problems. Mathematicians use the words “computable” and “noncomputable” to distinguish possible from impossible mathematical feats. Mathematicians say that a function is not “computable” if you cannot program a computer to solve it. They require only that the computer has a finite memory and the program has a finite length, since it is not possible to build a computer with infinite memory or write an infinitely long program. If no combination of computer and program can solve a well-formulated mathematical problem, then the problem is “undecidable.” To show that a problem is undecidable, a mathematician must show that no possible combination of program and computer will get you the answer. In the strict mathematical sense, when a function is not computable or a problem not decidable, it is not just that we cannot hope to solve it realistically. It is not merely hard to solve; it is impossible to solve.

In an important paper on “The Impossibility of an Effective Theory of Policy in a Complex Economy,” Velupillai (2007) shows that economic policy can have computability problems. He argues that, in the informal summary of Colander and Salzano, “ultimately there is an undecidability of policy in a complex economy” (2007, p. xvii). More precisely, he shows that “an *effective* theory of economic policy is impossible for such an economy” (p. 273). Velupillai is very careful to give “effective” a precise mathematical meaning (2007, p. 273, n.1). Rosser (1939) expressed the basic idea as meaning, “essentially that an effective method of solving a certain set of problems

exists if one can build a machine which will then solve any problem of the set with no human intervention beyond inserting the question and (later) reading the answer” (p. 56). Thus, Velupillai (2007) has shown that if the economy is complex, then you cannot program a computer to predict the specific outcome of a policy. He carefully specifies “complex economy” to imply “a dynamical system capable of *computation universality* (2007, p. 280). Velupillai’s computability result places a heavy burden of proof on any central bank official or securities regulator who might wish to intervene in financial markets to prevent or “correct” bubbles or animal spirits.

Critics of the New Interventionist Economics should consider using computability results to challenge the new interventionists when they propose regulations and reforms that increase state power and, especially, Big Player influence in financial markets. Somewhat more generally, I think BRACE economics and the new interventionism require a rigorously epistemic response if we are to preserve the Age of Friedman.

V. Closing Remarks

Capitalism is the Promised Land. Events of the Great Recession, however, have led many economists to think capitalism is flawed. Even Richard Posner and Alan Greenspan now think capitalism is flawed. Their change in thinking suggests that macroeconomics is headed toward a new interventionism that will generate models and policies similar those found in the works of Keynes and Minsky. We have reached a Minsky moment in more ways than one.

Bubbles, radical uncertainty, animal spirits, complexity dynamics, and extra-market control will characterize the New Interventionist Economics. Economists who wish to see the Age of Friedman extended to a new generation must take BRACE economics seriously and challenge it forcefully. They must emphasize the statist causes of the Great Recession. They must also offer a better theory of bubbles, booms, and busts. The Austrian theory of the trade cycle is a candidate, but it requires updating and must be supplemented by a more modern and vigorous program of empirical research. The policy prescriptions of the new interventionists must also be challenged. The “computable economics” of Velupillai (2005) and others offers important tools helping to show that policy consequences are hard to compute. In general, the hubris of many new interventionist policy proposals should be met with rigorous epistemic replies.

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