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THE FINANCIAL CRISIS AND THE SYSTEMIC FAILURE OF THE ECONOMICS PROFESSION

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ABSTRACT: *Economists not only failed to anticipate the financial crisis; they may have contributed to it—with risk and derivatives models that, through spurious precision and untested theoretical assumptions, encouraged policy makers and market participants to see more stability and risk sharing than was actually present. Moreover, once the crisis occurred, it was met with incomprehension by most economists because of models that, on the one hand, downplay the possibility that economic actors may exhibit highly interactive behavior; and, on the other, assume that any homogeneity will involve economic actors sharing the economist's own putatively correct model of the economy, so that error can stem only from an*

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exogenous shock. The financial crisis presents both an ethical and an intellectual challenge to economics, and an opportunity to reform its study by grounding it more solidly in reality.

The global financial crisis has revealed the need to rethink fundamentally how financial systems are regulated. It has also made clear a systemic failure of the economics profession.

Over the past three decades, most economists have developed and come to rely on models that disregard key factors—including the heterogeneity of decision rules, revisions of forecasting strategies, and changes in the social context—that drive outcomes in asset and other markets. It is obvious, even to the casual observer, that these models fail to account for the actual evolution of the real-world economy. Moreover, the current academic agenda has largely crowded out research on the inherent causes of financial crises. There has also been little exploration of early indicators of systemic crisis and potential ways to prevent this malady from developing. In fact, if one browses through the academic macroeconomics and finance literature, "systemic crisis" seems to be an otherworldly event, absent from economic models. Most models, by design, offer no immediate handle on how to think about or deal with this recurring phenomenon.[1] In our hour of greatest need, societies around the world are left to grope in the dark without a theory. That, to us, is a *systemic failure of the economics profession*.

Economists' Failure to Anticipate and Understand the Crisis

The implicit view behind standard equilibrium models is that markets and economies are inherently stable and that they only temporarily get off track. The majority of economists thus failed to warn about the threatening systemic crisis and ignored the work of those who did.

Ironically, as the crisis has unfolded, economists have had no choice but to abandon their standard models and to produce hand-waving common-sense remedies. Common-sense advice, although useful, is a poor substitute for an underlying model. It is not enough to put the existing model to one side, observing that one needs "exceptional measures for exceptional times." What we need are models capable of envisaging such "exceptional times."

The confinement of macroeconomics to models of stable states that are perturbed by limited external shocks, but that neglect the intrinsic recurrent boom-and-bust dynamics of our economic system, is remarkable. After all, worldwide financial and economic crises are hardly new, and they have had a tremendous impact beyond the immediate economic consequences of mass unemployment and hyperinflation in various times and places. This is even more surprising given the long academic legacy of earlier economists' study of crises, which can be found in the work of Walter Bagehot (1873), Hyman Minsky (1986), Charles Kindleberger (1989), and Axel Leijonhufvud (2000), to name a few prominent examples. This tradition, however, has been neglected and even suppressed. Much of the motivation for economics as an academic discipline stems from the desire to explain phenomena like unemployment, boom-and-bust cycles, and financial crises, but dominant theoretical models exclude many of the aspects of the economy that lead to such phenomena. Confining theoretical models to "normal" times without consideration of these aspects might seem contradictory to the focus that the average taxpayer would expect of the scientists on his payroll.

The most recent literature provides us with examples of blindness against the approaching storm that seem odd in retrospect. For example, in their analysis of the risk management implications of CDOs (collateralized debt obligations), Krahnen 2005 and Krahnen and Wilde 2006 mention the possibility of an increase of "systemic risk." But they conclude that such risk should *not* be the concern of the banks engaged in the CDO market, because it is the governments' responsibility to provide costless insurance against a system-wide crash. On the more theoretical side, a recent and prominent strand of literature essentially argues that consumers and investors are *too* risk averse because of their memory of the (improbable) event of the Great Depression (e.g., Cogley and Sargent 2008).

The failure of economists to anticipate and model the financial crisis has deep methodological roots. The often-heard definition of economics—that it is concerned with the "allocation of scarce resources"—is short sighted and misleading. It reduces economics to the study of optimal decisions in well-specified choice problems. Such research generally loses track of the complex dynamics of economic systems and the instability that accompanies it. Without an adequate understanding of these processes, one is likely to miss the major factors that influence the economic sphere of our societies. This insufficient definition of economics often leads researchers to disregard questions about the coordination of actors and the possibility of coordination failures. Indeed, analysis of these issues would require a different type of mathematics than that which is generally used in most prominent economic models.

Economists' Role in Fostering the Crisis

Financial economists gave little warning to the public about the fragility of their models,[2] even as they saw individuals and businesses build a financial system based on their work. There are a number of possible explanations for this failure to warn the public. One is a "lack of understanding" explanation: The researchers did not know the models were fragile. We find this explanation highly unlikely; financial engineers are extremely bright, and it is almost inconceivable that such bright individuals did not understand the limitations of their models. A second, more likely explanation for this failure is that they did not consider it their job to warn the public. We believe that this view involves a misunderstanding of the role of the economist—and an ethical breakdown. Economists, as with all scientists, have an ethical responsibility to communicate the limitations of their models and the potential misuse of their research. Currently, there is no ethical code for professional economic scientists. There should be one.

Economic textbook models, which focus the analysis on the optimal allocation of scarce resources, are predominantly Robinson Crusoe (representative-agent) models. Financial-market models are obtained by letting Robinson manage his financial affairs as a sideline to his well-considered utility maximization over his (finite or infinite) expected lifespan, taking into account with "correct" probabilities all potential future happenings. This approach is mingled with insights from Walrasian general-equilibrium theory, in particular the finding of the Arrrow-Debreu two-period model that all uncertainty can be eliminated if only there are enough contingent claims (i.e., appropriate derivative instruments). This theoretical result (a theorem in an extremely stylized model) underlies the common belief that the introduction of new classes of derivatives can only be welfare enhancing. It is worth emphasizing that this view is not empirically grounded but is derived from a benchmark model that is much too abstract to be confronted with data.

On the practical side, mathematical portfolio and risk-management models have been the academic backbone of the tremendous increase of trading volume and diversification of instruments in financial markets. Typically, new derivative products achieve market penetration only if a certain industry standard has been established for the pricing and risk management of these products. Mostly, pricing principles are derived from a set of assumptions about an "appropriate" process for valuing the underlying asset (i.e., the primary assets on which options or forwards are written), together with an equilibrium criterion such as arbitrage-free prices. From these assumptions springs advice for hedging the inherent risk of a derivative position (for example, by balancing it with other assets that neutralize the risk exposure).

The most prominent example is the development of a theory of options pricing by Fischer Black and Myron Scholes that eventually (in the 1980s) was preprogrammed into pocket calculators. Simultaneously with Black-Scholes options pricing, the same principles led to the widespread introduction of new strategies, under the headings of portfolio insurance and dynamic hedging, that tried to achieve a theoretically risk-free portfolio composed of both assets and options, and to keep it risk-free by frequent rebalancing after changes in its input data (e.g., asset prices).

With structured products for credit risk, however, the basic paradigm of derivative pricing—perfect replication—is not applicable, so that one has to rely on a kind of rough-and-ready evaluation of these contracts on the basis of historical data. Unfortunately, historical data were hardly available in most cases, which meant that one had to rely on simulations with relatively arbitrary assumptions about correlations between risks and default probabilities. This made the theoretical foundations of these products highly questionable—the equivalent to erecting a building's foundation without knowing the materials of which the foundation was made.

The dramatic recent rise of the markets for structured products (most prominently collateralized debt obligations and credit-default swaps) was made possible by the development of such simulation-based pricing tools and the adoption of an industry standard for these under the lead of the bond-rating agencies. Barry Eichengreen (2008) rightly points out that the "development of mathematical methods designed to quantify and hedge risk encouraged commercial banks, investment banks and hedge funds to use more leverage," as if the managers of these institutions believed that the very use of the mathematical methods diminished the underlying risk. He also notes that the models were estimated on data from periods of low volatility and thus could not deal with the arrival of major changes. But such major changes are endemic to the economy and cannot simply be ignored.

A somewhat different aspect is the danger of a *control illusion*: The mathematical rigor and numerical precision of risk-management and asset-pricing tools has a tendency to conceal the weaknesses of models and assumptions to those who have not developed them (as Eichengreen emphasizes). Naturally, models are, at best, only approximations to real-world dynamics and they are built in part on quite heroic assumptions (most notoriously, the normality of asset-price changes, which can be rejected at a confidence level of 99.999 percent). Of course, considerable progress has been made by moving to more refined models with, e.g., "fat-tailed" Levy processes as their driving factors. However, while such models better capture the intrinsic volatility of markets, their improved performance, taken at face value, might again contribute to enhancing the control illusion of the naïve user.

The increased sophistication of extant models, moreover, does not overcome the robustness problem and should not absolve the authors of the models from explaining their limitations to the users in the financial industry. As in nuclear physics, the tools provided by financial engineering can be put to very different uses, so that what is designed as an instrument to hedge risk can become a weapon of "financial mass destruction" (in the words of Warren Buffett) if used for increased leverage. This seems to have been the case with derivative positions that were built up to profit from high returns as long as the downside risk did not materialize.

Researchers who develop such models can claim they are merely neutral academics developing tools that people are free to use or reject. We do not find that view credible. Researchers have an ethical responsibility to point out to the public when the tools that they developed are misused. And it is the responsibility of the researcher to make clear from the outset the limitations and underlying assumptions of his models and to warn of the dangers of their mechanistic application.

Because researchers did not point out the difficulties with their models, the new derivatives markets were flawed in ways that contributed to the financial crisis. One of the most important problems was that while the possibility of systemic risk was not entirely ignored, it was defined as lying outside the responsibility of market participants. In this way, moral hazard concerning systemic risk was a built-in attribute of the system. The neglect of systemic externalities by market participants and policy makers is not only unethical; it is a prudential lapse as well: Market participants' use of these models undermines the stability of the system that the models imply is stable, meaning that participants should *not* use the models if they want to avoid being the victims of the endogenous boom-and-bust fluctuations so typical of markets.

Blame should not only fall on market participants and policymakers; it should also fall on economists, who insisted on constructing models that ignored the systemic risk factors. In failing even to point out their weaknesses to the public, they were participants in what might be called *academic* moral hazard.

What follows from our diagnosis? Market participants and regulators have to become more sensitive towards the potential weaknesses of risk-management models. Since we do not know the "true" model, robustness should be a key concern. The uncertainty of models should also be taken into account by applying more than a single model. For example, one might use an imperfect knowledge economics (IKE) model (Frydman and Goldberg 2007 and 2008) that makes use of probability theory, but also recognizes that no one, including the economist himself, knows the processes driving market outcomes. One might also rely on probabilistic projections that cover a whole range of specific models (cf. Föllmer 2008). The theory of robust control provides a toolbox of techniques that could be applied for this purpose, and it is an approach that should be considered.

Ignoring Market Participants' Own Models of Markets

A related flaw of asset-pricing and risk-management tools is their individualistic perspective, which takes as given (*ceteris paribus*) the behavior of all other market participants. However, if popular asset-pricing and risk-management models are used by a large number (or even the majority) of market participants, then the individualistic assumption is false and can be expected to produce misleading predictions. By the same token, a market participant (e.g., the notorious Long-Term Capital Management) might become so dominant in certain markets that the *ceteris paribus* assumption becomes unrealistic. The simultaneous pursuit of identical micro strategies leads to synchronous behavior and built-in contagion. This simultaneous application might generate an unexpected macro outcome that jeopardizes the success of the underlying micro strategies.

A perfect illustration was the U.S. stock market crash of October 1987. Triggered by a small decline in prices, automated hedging strategies produced an avalanche of sell orders that, out of the blue, led to a fall in U.S. stock indices of about 20 percent within one day. Engaging in massive sales to rebalance their portfolios (along the lines of Black and Scholes), the relevant actors could not realize their attempted incremental adjustments, but instead suffered major losses from the huge ensuing macro effect. The model was self-reflexive; people's collective use of the model changed the model, and brought about a result not predicted by the original model.

Similarly, many economic models are built upon the twin assumptions of "rational expectations" and a representative agent: That is, *all* market participants are homogenized into a single agent with rational expectations, and these are defined to be fully consistent with the structure of *the economist's* own model. Since the economist's model is, of course, treated as true (which is odd given that even economists are divided in their views about the correct model of the economy), the implication is that the representative individual, hence everyone in the economy, behaves as if he had *a complete understanding of the economic mechanisms governing the world*.

Such models do not attempt to formalize individuals' actual expectations: Specifications are not based on empirical observation of how people form expectations. Thus, even when applied economics research or psychology provide insights about how individuals actually form expectations, they cannot be used within rational-expectations models. Leaving no place for real-world individuals' imperfect knowledge and adaptive adjustments, rational-expectations models are typically found to have dynamics that are grossly inconsistent with economic data.

Technically, rational-expectations models are often framed as solving dynamic-programming problems in macroeconomics. But dynamic-programming models as models of the aggregate economy have serious limitations (Colander 2006; Colander et al. 2008). If they are to be analytically tractable, not more than one dynamically maximizing agent can be considered, and consistent expectations have to be imposed on this agent. Therefore, dynamic-programming models are hardly imaginable without the assumptions of a representative agent and rational expectations. This has generated a vicious cycle in which technical tools developed on the basis of the chosen assumptions prevent economists from moving beyond these restricted settings to explore more realistic scenarios. While other currents of research do exist, economic policy advice, particularly in financial economics, has far too often been based (consciously or not) on a set of axioms and hypotheses derived ultimately from a highly limited dynamic-control model that couples the Robinson Crusoe approach with "rational" expectations.

The major problem is that despite its many refinements, this is not an approach based on, and confirmed by, empirical research to anywhere near the degree that it should be.[3] In fact, the assumptions underlying models too often stand in stark contrast to a broad set of regularities in human behavior discovered both in psychology and in what is sometimes called behavioral economics, as well as in experimental economics. The cornerstones of many models in finance and macroeconomics are maintained *despite* all the contradictory evidence discovered by such research. Much of this literature shows that in experiments, human subjects act in ways that bear little resemblance to how they are assumed to act in rational-expectations models. Real-world people do not exhibit ultra-rationality. Rather, agents display various forms of "bounded rationality," using heuristic decision rules and displaying inertia in their reaction to new information. They have also been shown in real financial markets to be strongly influenced by emotional and hormonal reactions (Lo et al. 2005; Coates and Herbert 2008). Incorporating such findings into an economic model may help us better understand dynamics in real-world markets.

What we are arguing is that as a modeling requirement, internal consistency must be complemented with *external consistency*: Economic modeling cannot be inconsistent with insights about real-world human behavior obtained from other branches of science. It is highly problematic to insist on a specific view of humans in economic settings that is irreconcilable with empirical evidence.

Conceptual Reductionism

The representative-agent assumption in many current models in macroeconomics (including macro finance) means that modelers subscribe to the most extreme form of conceptual reductionism (Lux and Westerhoff 2009): By assumption, all concepts applicable to the macro sphere (i.e., the economy or its financial system) are fully reduced to concepts and knowledge in the lower-level domain of the individual agent. This is quite different from the standard reductionist concept that has become widely accepted in natural sciences, which reduces complex phenomena to the interactions of their parts, allowing the scientist to understand new, emergent phenomena at the higher hierarchical level (the concept of "more is different"; cf. Anderson 1972).

By contrast, in economics, the representative-agent approach simply *equates* the macro sphere with the micro sphere. One could, indeed, say that this equation negates the existence of a macro sphere and the necessity of investigating macroeconomic phenomena, in that it views the entire economy as an organism governed by a universal will.[4] Any notion of "systemic risk" or "coordination failure" is necessarily absent from, and alien to, such a methodology.

For natural scientists, the distinction between micro-level phenomena and macro, system-wide phenomena that emerge from the interaction of microscopic units is well known. In a dispersed system, the financial crisis would be seen as an involuntary emergent phenomenon of microeconomic activity. The conceptually reductionist paradigm, however, blocks, from the outset, any understanding of the interplay between micro and macro levels. The differences between the overall system and its parts remain simply incomprehensible from the viewpoint of this approach.

In order to develop models that allow us to deduce macro events from microeconomic regularities, economists have to rethink the concept of micro foundations. Economists' micro foundations should allow for the interactions of economic agents, since economic activity is, essentially, interactive. And since interaction depends on differences in information, motives, knowledge, and capabilities, this implies the *heterogeneity*—not the "representativeness"—of agents.

For instance, only a sufficiently rich model of connections between firms, households, and a dispersed banking sector is likely to allow us to get a grasp on "systemic risk," domino effects in the financial sector, and their repercussions on consumption and investment. The dominance of the extreme form of representative-agent conceptual reductionism has prevented economists from even attempting to model these important phenomena. It is this flawed methodology that is the ultimate reason for the lack of applicability of the standard macro framework to current events. It also explains, in part, the growing separation of academic economics from issues relating to the real-world economy. For example, the recent surge of research in network theory has received relatively scarce attention in economics, even though it could provide a window into the interaction of ensembles of heterogeneous actors. "Self-organized criticality" theory may also help to explain boom-and-bust cycles (cf.

Scheinkman and Woodford 1994). But instead, given the established curriculum of economic programs, a young economist would find it much more tractable to study adultery as a dynamic-optimization problem of a representative husband, and to derive the optimal time path of marital infidelity (and to publish his exercise) than to investigate financial flows in the banking sector within a network-theory framework. This is more than unfortunate in view of the network aspects of interbank linkages that have become apparent during the current crisis.

In our view, a change of focus is necessary that takes seriously the regularities in expectation formation revealed by observations of actual behavior from applied and behavioral research, and also allows for the independent role of the diverse expectations of heterogeneous economic actors. On the one hand, it would not be appropriate, empirically, to universalize laboratory findings on risk aversion to all agents in all contexts; nor, on the other hand, would it be appropriate to insist that homogeneous herd behavior is never possible. Neither conclusion would be empirically warranted. It would also be fallacious to replace rational-choice theory with an insistence that the "non-rational" actor is representative. Rather, an appropriate micro foundation is needed that considers interaction at a certain level of complexity and extracts macro regularities (where they exist) from microeconomic models with dispersed activity.

Once one acknowledges the importance of empirically based behavioral micro foundations and the heterogeneity of actors, a rich spectrum of new models becomes available. The dynamic co-evolution of expectations and economic activity would allow one to study out-of-equilibrium dynamics and adaptive adjustments. Such dynamics could reveal the possibility of multiple and evolving equilibria (e.g., with high or low employment) depending on agents' expectations—or even on the propagation of positive or negative "moods" among the population. This would capture the psychological component of the business cycle, which—while prominent in many policy-oriented discussions—is never taken into consideration in contemporary macroeconomic models. Finally, a focus on the heterogeneity of imperfect knowledge might provide a better framework for the analysis of the use and dissemination of information through market operations and more direct forms of communication. If one accepts that the dispersed economic activity of many economic agents could be described by statistical laws, one might even take stock of methods from statistical physics to model dynamic economic systems (e.g., Aoki and Yoshikawa 2007; Lux 2009).

Whatever Happened to Empirical Testing?

Currently popular models (in particular, dynamic general-equilibrium models) not only have weak micro foundations, but their empirical performance is far from satisfactory (Juselius and Franchi 2007). Indeed, the relevant strand of empirical economics has more and more avoided testing its models, worrying about calibration without explicitly considering goodness-of-fit.[5] This calibration is achieved by using "deep economic parameters," such as the parameters of utility functions derived from microeconomic studies. However, at the risk of being repetitive, it should be emphasized that there is no compelling argument as to why micro parameters should be used directly in the parameterization of a macroeconomic model. The aggregation literature is full of examples that point out the varieties of the fallacy of composition. The "deep parameters" seem sensible only if one considers the economy as a universal organism without interactions. But if interactions are important (as we believe they are), the restriction of the parameter space imposed by using micro parameters is inappropriate. Another concern about aggregation is nonstationarity due to structural shifts in the underlying data. Macro models, unlike many financial models, are often calibrated over long time horizons that include major changes in the regulatory framework of the countries investigated, such as alterations in exchange-rate regimes and the deregulation of financial markets during the 1970s and 80s.

In much of the macroeconomics and finance literature there is an almost scholastic acceptance of axiomatic "first principles" (basically, the building blocks of an intertemporally optimizing representative agent with completely rational expectation formation) independent of any empirical evidence.[6] Even dramatic differences between the model's behavior and empirical data are not taken as evidence against the model's underlying axioms. Quite in contrast to what one would expect of an applied science, most of the contemporary work in macroeconomics and finance is thus characterized by a pre-analytic belief in the validity of certain models that are never meaningfully exposed to empirical tests (Juselius and Franchi 2007). In our view, macroeconomics as an empirical science should not be pursued in an axiomatic fashion, and the goal of macroeconometrics should be to use data to choose among competing models.

David Hendry (1995 and 2009) provides a well-established empirical methodology for such exploratory data analysis as well as a general theory for model selection (Hendry and Krolzig 2005). Clustering techniques such as projection pursuit (e.g., Friedman 1987) might provide alternatives for the identification of key relationships and the reduction of complexity on the way from empirical measurement to theoretical models. Cointegrated Vector Auto Regression (VAR) models could provide an avenue towards identifying robust structures within a set of data (Juselius 2006)—for example, the forces that move equilibria (*pushing forces*, which give rise to stochastic trends) and the forces that correct deviations from equilibrium (*pulling forces*, which give rise to long-run relationships). Interpreted in this way, the "general-to-specific" empirical approach has a good chance of nesting a multivariate, path-dependent, data-generating process and relevant dynamic macroeconomic theories. Unlike approaches in which data are silenced by prior restrictions, the cointegrated VAR model gives the data a rich context in which to speak freely (Hoover et al. 2008).

A chain of specification tests and estimated statistical models for simultaneous systems would provide a benchmark for the subsequent development of tests of models based on economic behavior: Significant and robust relations within a simultaneous system would provide empirical regularities that one would attempt to explain, while the quality of fit of the statistical benchmark would offer a confidence band for more ambitious models. Models that do not reproduce (even) approximately the quality of the fit of statistical models would be rejected. (The majority of currently popular macroeconomic and macro finance models would not pass this test.) Again, we see here an aspect of the ethical responsibility of researchers: Economic policy models should be theoretically and empirically sound. Economists should avoid giving policy recommendations on the basis of models with a weak empirical grounding and should, to the extent possible, make clear to the public how strongly—or weakly—the data support their models and the conclusions drawn from them.

The Failure of Economic Theory in the Crisis of 2008

The notion of financial fragility implies that a given system might be more or less susceptible to producing crises. For instance, it seems clear that financial innovations prior to 2008 made the system more fragile. Apparently, the existing linkages within the worldwide, highly connected financial markets generated spillovers from the U.S. subprime problem to other layers of the financial system. Many financial innovations had the effect of creating links between formerly unconnected players.

All in all, the degree of connectivity of the system probably increased enormously over the last decades. As is well known from network theory in the natural sciences, a more highly connected system might be more efficient in coping with certain tasks (perhaps by distributing risk components), but will often also be more vulnerable to shocks and systemic failure.

A systematic analysis of network vulnerability has been undertaken in the computer-science and operations-research literature (e.g., Criado et al. 2005). Such research has, however, been largely absent from financial economics. The introduction of new derivatives was rather seen through the lens of general-equilibrium models: More contingent claims help to achieve higher efficiency. Unfortunately, the claimed efficiency gains through derivatives are merely a theoretical implication of a highly stylized model and, therefore, should be seen as a *hypothesis* to be tested, not a fact that is assumed. Since there is hardly any supporting empirical evidence (or even analysis of this question), the claimed real-world efficiency gain from derivatives is unjustified. Meanwhile, the possibility of negative effects was neglected—with real-world consequences. Specifically, the idea that the system was made less risky by the development of more derivatives may have led financial actors to take positions with extreme degrees of leverage. (The leverage of financial institutions rose to unprecedented levels prior to the crisis, partly by evading Basel capital regulations through structured investment vehicles [Acharya and Schnabl 2009]). The interplay between leverage, connectivity, and systemic risk needs to be investigated at the aggregate level.

The economics profession also has to re-investigate the informational role of financial prices and financial contracts. While trading in stock and bond markets is usually interpreted as, at least in part, transmitting information, this information transmission seems to have broken down in the case of structured financial products. It seems that securitization rather led to a loss of information by anonymous intermediation (often multiple) between borrowers and lenders. In this way, the informational component was outsourced to rating agencies and typically, the buyer of a tranche in a collateralized debt obligation would not have spent any effort himself on information acquisition concerning his far-off counterparts. This centralized information processing by the rating agencies, in contrast to the dispersed processing in traditional credit relationships, might have led to a severe loss of information. Standard loan-default models of the sort on which rating agencies relied failed dramatically in recent years (Rajan et al. 2008).

It should also be noted that the price system itself can, like trading in securities markets, exacerbate problems rather than just neutrally transmitting information (see Hellwig 2008). One of the reasons for the sharp fall in the asset valuations of major banks was not only the loss in the assets on which their derivatives were based, but also the general reaction of the markets to this decline. As markets became aware of the risk involved, all such assets were written down, and in this way a small sector of the market "contaminated" the rest. Large parts of the asset holdings of major banks abruptly lost much of their value. Thus, the price system itself can be destabilizing as expectations change, and the consequences can be severe.

On the macroeconomic level, it would be desirable to develop early-warning schemes that indicate the formation of unsustainable price swings away from historical benchmark levels. Combinations of indicators with time-series techniques could be helpful in detecting deviations of financial or other prices from such levels. Indications of structural change (particularly towards non-stationary trajectories) would be a signature of changes of the behavior of market participants leading to unsustainable fluctuations.

The financial crisis might be characterized as an example of the final stage of the well-known boom-and-bust pattern that has been repeated so often in the course of economic history. There are, nevertheless, some aspects that make this crisis different from its predecessors. First, the preceding boom may have had its origin—at least in large part—in the development of new financial products that opened up new investment possibilities, while most previous crises were the consequence of overinvestment in new physical investment possibilities. Second, the global dimension of the current crisis is presumably due to the increased connectivity of our already highly interconnected financial system. Both aspects have been largely ignored by academic economics. Research on the origin of instabilities, overinvestment, and subsequent slumps has been considered as an exotic side track from the academic research agenda (and the curriculum of most economics programs). This occurred because such research was incompatible with the premise of the rational representative agent, which had come to be thought the only allowable model. That belief made the economics profession blind to the role of interactions and connections between actors (such as the changes in the network structure of the financial industry brought about by deregulation and introduction of new structured products). Indeed, much of the work on contagion and herding behavior (see Banerjee 1992 and Chamley 2002), which is closely connected to the network structure of the economy, has not been incorporated into macroeconomic analysis.

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We believe that economics has been trapped in a suboptimal equilibrium in which much of its research efforts are not directed towards the most pressing social needs. Self-reinforcing feedback effects among economists may have led to the dominance of a paradigm that has no solid methodological basis and whose empirical performance is, to say the least, modest. Defining away the most prevalent economic problems of modern economies and failing to communicate the limitations and assumptions of its popular models, the economics profession bears some responsibility for the financial and economic crisis. It has failed in its duty to provide needed insight into the workings of the economy and markets, and it has been reluctant to emphasize the limitations of its analysis. We believe that the failure even to envisage hypothetically the current problems of the worldwide financial system, and the inability of standard macro and finance models to provide any insight into ongoing events, make a strong case for a major reorientation in these areas and a reconsideration of their basic premises.

NOTES

1. Carmen Reinhart and Kenneth Rogoff (2008) argue that the current financial crisis differs little from a long chain of similar crises in developed and developing countries. We certainly share their view. The problem is that the received body of models in macro finance, to which these authors have prominently contributed, provides no room whatsoever for such recurrent boom and bust cycles. The literature has, therefore, been a major source of the illusory "this time it is different" view that the authors themselves criticize.
2. Indeed, few researchers explored the consequences of a breakdown of their assumptions, even though this was rather likely.
3. The historical emergence of the representative-agent paradigm is a mystery. Ironically, it appeared during the 1970s, after a period of intense discussion of the problem of aggregation in economics (which basically yielded negative results). The representative agent, however, appeared without similar methodological discussion. In the words of Deirdre McCloskey, "It became a rule in

the conversation of some economists because Tom and Bob said so" (personal communication). Today, this convention has become so strong that many young economists wouldn't know of an alternative way to approach macroeconomic issues.

4. The reductionist conceptual approach of the representative agent is also remarkably different from the narrative of the "invisible hand," which also has the flavor of "more is different."
5. It is pretty obvious how the currently popular class of dynamic general-equilibrium models would have to "cope" with the financial crisis. It would be covered either by a dummy variable or interpreted as a very large negative stochastic shock to the economy, i.e., as an event equivalent to a large asteroid strike.
6. Robert Solow (2008, 235) has called it a "rhetorical swindle" that the "macro community has perpetrated on itself, and its students."

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