Economics
The Open-Access, Open-Assessment E-Journal

Discussion Paper
No. 2010-18 | July 12, 2010 | http://www.economics-ejournal.org/economics/discussionpapers/2010-18

On the Economic Crisis and the Crisis of Economics

Victor A. Beker
University of Belgrano and University of Buenos Aires

Abstract The outburst of the 2008 global economic crisis sparked a myriad of criticism on mainstream neoclassical economic theory, held responsible for not even have considered the possibility of the kind of collapse that the subprime mortgage meltdown unleashed. In this paper, it is argued that what happened was a case of malpractice by hundreds of economists in banks and rating agencies who created and certified as almost risk-free securities assets that were actually highly risky as the events after 2007 overwhelmingly showed. Such a massive case of malpractice denounces deep failures in the regulatory system. The deregulation movement that took place during the 1980s and 1990s was inspired by an almost religious belief in the power of market forces to solve any economic problem. Neoclassical economics can be blamed for creating the ideological climate which stimulated the deregulation movement in the U.S.A during those decades. After discussing some aspects of economics methodology, arguing the need to approach the economy as an interactive complex system, and discussing the use of mathematics in economics, it is argued that the main object of economists´ efforts should be economic illness rather than economic health. Finally, a list of 15 guidelines is sketched out for improving the methodological approach as well the contents of economic analysis.

JEL B41, A11
Keywords Economic crisis; malpractice; bounded rationality; complexity; economic anomalies; microfoundations; regulation

Correspondence victor.beker@ub.edu.ar

The author would like to thank William J. Baumol and John Barkley Rosser Jr. for comments on an earlier version of this paper. Of course, only the author is responsible for the arguments here.

"So in summary, Your Majesty, the failure to foresee the timing, extent and severity of the crisis and to head it off, while it had many causes, was principally a failure of the collective imagination of many bright people, both in this country and internationally, to understand the risks to the system as a whole."
Letter to the Queen of England by the British Academy. July 2009

Introduction

The outburst of the 2008 global economic crisis sparked a myriad of criticism on mainstream neoclassical[1] economic theory, held responsible for not even have considered the possibility of the kind of collapse that the subprime mortgage meltdown unleashed.

If we follow Joan Robinson (1972), this was the third main crisis economic theory faces. She associated the first one with the great slump of the ´1930s and the second one with the 1971 dollar crisis.

Conspicuous among the contemporary critics, Nobel Prize Paul Krugman blames the profession for its ¨blindness to the very possibility of catastrophic failures in a market economy.¨[2]

In his view, ¨the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth.¨[3]

This led to turn ¨a blind eye to the limitations of human rationality that often lead to bubbles and busts; to the problems of institutions that run amok; to the imperfections of markets -especially financial markets- that can cause the economy's operating system to undergo sudden, unpredictable crashes; and to the dangers created when regulators don't believe in regulation.¨[4]

Behavorial macroeconomists as the Nobel Prize George Akerlof and Robert Shiller (2009) put also the blame on the rationality assumption of mainstream neoclassical economics. Only "if we thought that people were totally rational, and that they acted almost entirely out of economic motives, we too would believe that government should play little role in the regulation of financial markets, and perhaps even in determining the level of aggregate demand."[5]

Herbert Gintis (2009) goes further. Although he coincides in the critique to orthodox economic theory, he argues that ¨there is nothing in economic theory that says that rational individuals interacting on markets will produce either stable or socially efficient outcomes.¨[6] He concludes that there are ¨slim grounds for Akerlof and Shiller to attribute macroeconomic fluctuations wholly to "animal spirits" that would not exist were economic actors "rational."[7]

[1] For a distinction between the concepts of neoclassical, orthodox, heterodox and mainstream economics see Colander et al. (2004).
[2] How Did Economists Get It So Wrong? *New York Times*, September 2, 2009.
[3] Ibid.
[4] Ibid.
[5] Akerlof and Shiller(2009: 173).
[6] Gintis (2009: 4).
[7] Ibid., p. 5.

Gintis vindicates then, as an alternative perspective that deserves consideration, the modeling of the market economy as a complex nonlinear system.

For Colander et al. (2009: 2) the financial crisis revealed a ¨systemic failure of the economics profession¨ because the majority of economists ¨failed to warn policy makers about the threatening system crisis and ignored the work of those who did.¨

Direct from the battle front, Willem Buiter, the chief economist of Citigroup and former member of the Monetary Policy Committee of the Bank of England, says that, in his opinion, macroeconomics research programs tended to be motivated by the internal logic, intellectual sunk capital and aesthetic puzzles of established research programs rather than by a powerful desire to understand how the economy works -let alone how the economy works during times of stress and financial instability. So the economics profession was caught unprepared when the crisis struck[8].

Jon Elster (2009), a political scientist, offers what he calls ¨outsider criticism¨ of economic theory.

He argues that the problem with economics and other social sciences is ¨excessive ambitions.¨ Economists look for a level of precision and robustness which cannot be warranted in social sciences.

Two conditions are crucial for mainstream neoclassical economics: determinate prediction and rational behavior. If the theory is indeterminate or the agents are irrational no explanation will be forthcoming. Elster argues why more often than not these conditions do not hold.

Indeterminacy stems from the difficulty for agents to assess numerical probabilities to the possible outcome of actions.

Rationality faces the restriction of agents´ capacities. Economic agents are supposed to make the calculations that occupy many pages of mathematical appendixes in leading journals. Elster discards the ¨as if¨ rationality argument arguing that it is based on the assumption that the economic agent is able to spend absurdly large amounts of time searching for a good rule.

He observes that economists make assumptions for the sake of simplicity without telling the reader how many of the conclusions can be expected to hold in the non-simplistic case.

His conclusion is that much work in economics and political science is devoid of empirical, aesthetic or mathematical interest. Many articles published by eminent economists are nothing more than a piece of science fiction. So, lots of economic students waste their time studying useless theories.

Some of these criticisms have a long standing in economics like the lack of realism of the assumptions[9] or the argument that people do not behave as the theory says they will or should.

Although he vindicates behavioral economics as an alternative to neoclassical thought, he admits that its drawback is that there are relatively few applications of behavioral economics outside the laboratory.

[8] ¨The unfortunate uselessness of most ´state of the art´ academic monetary economics.¨ See http://www.voxeu.org/index.php?q=node/3210

[9] I have already dealt with this argument in Beker (2005: 17). We will come back on this later on.

He maintains that a flaw economics suffers from is the belief that social science only can become a science on the model of the natural sciences. In spite of this belief, he objects, none of the many mainstream economists who received the Nobel Prize got it for confirmed empirical predictions.

The opposite happens in physics, he comments. For example, string theory is today the dominant paradigm in most physics departments of the major research universities. However, it has not been awarded a single Nobel Prize mainly because it has not still generated confirmed predictions that are not also consequences of rival theories.

Elster´s observation coincides with what Hausman (1992, p.222) has called *methodological schizophrenia*, referring to the fact that in economics methodological pronouncements and practice often do not coincide.

Elster proposes to replace the aim of prediction with that of retrodiction --explaining the past-, which he considers is a perfectly respectable intellectual enterprise. He maintains that the past can be falsified no less than predictions about the future.

This also means that we should cease looking for economic laws. In laboratory experiments it is sometimes possible to isolate sufficient conditions for a specific causal mechanism to be triggered. Then, one can appeal to laws and offer predictions. But outside the laboratory, where these conditions rarely obtain, retrodiction and appeal to mechanisms instead of laws is the best we can do, according to Elster.

Mechanisms are, in his definition, causal patterns that are triggered under generally unknown conditions or with indeterminate consequences. One example he provides is the case when a King enacts repressive measures; they may make citizens less likely or more likely to rebel. The net effect is in general unpredictable but once the result is observed we can explain the mechanism behind it: fear dominated hate or vice versa. What usually we cannot say *ex ante* is which will be the dominant effect.

Elster´s conclusion is that economists should have, instead of excessive ambitions, humble but attainable aspirations.

Having summarized some of the main criticism to orthodox economic theory, let us now, first of all, have a look at what responsibility could be attributed to it in the recent economic crisis.

What is economics guilty of?

The core of the recent financial market crisis has been the discovery that many securities were actually far riskier than what people originally thought they were.

The process of securitization allowed trillions of dollars of risky assets –subprime mortgages in the first place- to be transformed into securities which were widely considered to be safe.

Coval, Jurek and Stafford (2009) show how modest imprecision in the parameter estimates can lead to variation in the default risk of the structured finance securities that is sufficient, for example, to cause a security rated AAA to default with reasonable likelihood.

The essence of structured finance is the pooling of economic assets like loans, bonds, and mortgages, and the subsequent issuance of a prioritized capital structure of claims against these collateral pools. Although the argument was that this was a way to diversifying risks, the truth is that the resulting securities were subject to highly correlated risks.

A key factor in determining if an asset is relatively safe is the extent to which defaults are correlated across the underlying assets. The lower the default correlation, the more improbable is that all assets default simultaneously. But the securities backed by large asset pools are strongly affected by the performance of the economy as a whole. So, they have far less chance of surviving a severe economic downturn than, for instance, traditional corporate securities of equal rating.

What has economics to do with all this?

First of all, there was a reckless use of economic models to evaluate risks. The nature of structured finance makes that even minute errors at the level of the underlying securities that would be insufficient to alter a security's rating can dramatically alter the ratings of the structured finance securities.[10]

Securitization itself increased the distance between the originator of the loan and the party that bears the default risk inherent in the loan.[11] This lowered the incentives of lenders to collect information about borrowers.[12] This worsened the quality of loans issued.

On the other hand, substantial lending to subprime borrowers was a recent phenomenon and historical data on defaults and delinquencies of this sector of the mortgage market was scarce. So, the possibility for errors in the assessment of the default correlations, the default probabilities, and the ensuing recovery rates for these securities was significant. Such errors were magnified by the process of re-securitization, leading to the devastating losses the securities market experienced.[13]

However, no special warning accompanied evaluations made on such weak and fragile basis. ¨The mathematical rigor, elegance and the numerical precision of the various risk-management and asset-pricing tools have a tendency to "hide" the weaknesses of these models and their underlying assumptions, which are necessary to guarantee the models' values to those who have not developed them.¨[14]

As Colander et al.(2009) put it: "economists, as all other social scientists, have an ethical responsibility to communicate the limitations of the models and the potential misuse of their research."

As we can see, this has more to do with economists than with economics. It seems to be a typical case of malpractice. Of course, an extended malpractice by hundreds of economists in banks and rating agencies who created and certified as almost risk-free securities assets that were actually highly risky as the events after 2007 overwhelmingly showed.

Such a massive case of malpractice denounces deep failures in the regulatory system. Many economic tools were misused or used without having been duly subject to previous testing. It is like massively using a new vaccine without having tested it according to the regulations of the FDA.

There were some isolated voices who tried to alert the perils of the huge changes which took place in the financial industry. Perhaps the most striking one was Rajan´s (2005) with his prescient analysis of how the developments observed in financial markets could

[10] See Coval, Jurek and Stafford (2009: 9).
[11] Rajan et al. (2010: 506)
[12] Ibid.
[13] Ibid., p.15.
[14] Schneider and Kirchgässner (2009).

degenerate into a crisis. Unfortunately, his was an almost unique voice and was not much listened. No economic journal published his paper and at the SSRN site only collected 93 downloads, which made it rank 96,914th at the SSRN download ranking.

On the other hand, the financial market is clearly characterized by asymmetry of information and externalities. Both are reasons for regulatory measures.

Investors do not have access to the amount and quality of information the issuers of assets have. That is why rating agencies come on scene to provide them with accurate evaluation of the risks. The problem is that the rating is paid by the issuer, not the investor. This raises a conflict of interest, as was exposed by the high credit ratings given to actually highly risky assets.

A second argument in favor of regulating the financial system is externalities. The impact the banking system has on the rest of the economy hardly can be underrated. The impact of a banks´ bankruptcy goes far beyond the losses its shareholders can suffer.

However, the 1980 Depository Institutions Deregulation and Monetary Control Act deeply deregulated financial activities in U.S.A.. Additionally, the final repeal of Glass-Steagall in 1999 lifted restrictions on the sort of investments the banks can make. This opened the door to the development of many instruments of ¨creative¨ financing, through which the repackaging of risks to create supposedly ¨safe¨ assets took place, as well as it made possible the vast involvement of banks in the subprime mortgage market.

The deregulation movement that took place during the 1980s and 1990s was inspired by an almost religious belief in the power of market forces to solve any economic problem. And it is true that mainstream neoclassical economics nourished that belief.

In this respect, neoclassical economics can be blamed for creating the ideological climate which stimulated the deregulation movement in the U.S.A during the 1980s and 1990s.

The belief that market forces would solve potential problems was behind the financial deregulation which proved to be a fatal flaw of the financial system in the United States.

On the contrary, a highly regulated financial system, as the Indian one, mainly remained out of the crisis. Very strict rules hampered the creation of toxic assets of the sort that proliferated in U.S.A.

Stringent rules governing leverage and capital ratios in Canada have been accounted for Canada's impressive performance during the crisis.

In this respect, Nobel Prize Paul Krugman seems to be right when he blames the profession –dominated by the neoclassical school in the 1980s and the 1990s- for its blindness to the very possibility of catastrophic failures in a market economy.

Is neoclassical economics innocent?

Of course, it is always possible to argue that the ideas that are criticized are not the true ideas of mainstream economics, as Levine does in his answer to Krugman.[15]

First of all we have to take into consideration that the scholars that have had great influence on policy makers around the world are those from the neoclassical school of thought. Their ideas dominated the economic policy since 1980.

[15] See http://www.huffingtonpost.com/david-k-levine/an-open-letter-to-paul-kr_b_289768.html

Levine argues that Krugman is shooting at an inexistent target. His clock would be 30 years late, according to him.

In this respect he advices to have a look, for example, at Timothy Kehoe and Ed Prescott (2007), Great Depressions of the 20th Century**.**

Kehoe and Prescott start their book stating: "The general equilibrium growth model is the workhorse of modern economics. It is the accepted paradigm for studying most macroeconomic phenomena, including business cycles, tax policy, monetary policy, and growth.¨

The authors´ point of departure is to assume flexible prices and perfect foresight. But if prices are fully flexible and people have perfect foresight the main reasons for a downwards adjustment in quantities are *a priori* excluded. Then, not surprisingly the conclusion is that the main reason for a depression should be found in exogenous TFP shocks. In the assumptions already lies the answer. These are the usual assumptions of neoclassical economics. Moreover, as Michael Woodford says in his blurb for the volume, it shows ¨how neoclassical theory can be applied…¨; so it is a typical neoclassical contribution with new analytical instruments but the same ideas we could find 30 or 50 years ago. It is just old wine in new bottles. In this respect it seems that it is neoclassical economics whose clock is late.

What economists do know.

However, the answer to the last economic crisis has proven that economists are better prepared than in 1930 to face this sort of challenge.

Of course, the measures taken were far away from what the orthodoxy recommends. A massive bailout of banks and corporations saved them from collapse and saved lots of jobs in the American economy. Countercyclical fiscal policy played a key role in fighting recession. The level of State intervention in the economy has reached unparallel levels in the American history.

We learned in the ´1930s that we cannot wait and see until the market solves the gigantic disequilibria in the financial markets.

As the crisis unfolded, it quickly became apparent that another Great Depression would only be averted by rapid and concerted policy action around the world. Fortunately, policymakers pulled together to respond to this profound economic calamity. A range of bold actions were taken—easing monetary conditions, adopting a fiscal stimulus, and cooperating on cross-border financial problems. International lending reached unprecedented levels.

Once again, this whole package was far away from the orthodox thinking. Moreover, it was unthinkable some years ago to imagine the IMF Managing Director paying an enthusiastic tribute to… John M. Keynes´s ideas![16]

What sort of science economics is

Economics is not an exact science. However, it is true that many economists act as if it were and try to convince society that it is.

Let us start analyzing the main characteristics of economics as a social science.

[16] See Economic Policy Challenges in the Post-Crisis Period. Speech at Inaugural Conference at the Institute for New Economic Thinking by IMF Managing Director Dominique Strauss-Kahn Cambridge, UK, April 10, 2010

I have dealt elsewhere with some methodological issues in economics.[17] Let me make a summary of the main conclusions I arrived at there.

As Blaug (1992, p.243) points out, "mainstream neoclassical economists … preach the importance of submitting theories to empirical tests, but they rarely live up to their declared methodological canons. Analytical elegance, economy of theoretical means, and the widest possible scope obtained by ever more heroic simplification have been too often prized above predictability and significance for policy questions."

In fact, in economics there is, broadly speaking, nothing like a crucial experiment. For example, given a certain econometric result, in many cases it is enough to just include another variable, or to slightly modify the model assumptions or the estimation method to get different, and even opposite, results. There are many examples in the economic literature in this respect. No matter how sophisticated the economic tools are and how detailed the set of data one deals with very few robust relationships can be obtained.

Although potentially falsifiable, most statements in economics are only imperfectly testable. Precisely, the main characteristic that distinguishes it from, for instance, natural sciences, is that theories, in most cases, cannot in practice be falsified.

That is why, as Hausman (1992) states, economists trust more in the implications deduced from the theory's axioms than in the negative results which may emerge from empirical testing. It is very rare to see a theory disregarded because of an apparent disconfirmation.

Since economists are typically dealing with complex phenomena in which many simplifications are required and in which many interferences may appear, it does not seem rational to surrender a credible hypothesis because of predictive failure. When facing an apparent disconfirmation, economists rely on what Hausman (1992, 207) calls the "weak-link principle": when a false conclusion depends on a number of uncertain premises, attribute the mistake to the most uncertain of the premises.

What role plays, then, empirical research? As a matter of fact, most empirical results in economics are more useful to illustrate theories than to test their validity[18].

This is the attitude that the whole profession implicitly has towards empirical results; they are mainly viewed as a way of illustrating that a theory *may* be true.[19] For example, no journal –be it orthodox or heterodox- induces the authors of an empirical paper –or its critics- to test the hypotheses included in it by using new data some time after publication.

Of course, as Colander et al. (2009, p.11) propose ¨the goal should be to put theoretical models to scientific test (as the naïve believer in positive science would expect).¨

If this were always possible, much less difficulties the economists would have to face. But the problem is precisely that ¨in economics there is, broadly speaking, nothing like a crucial experiment. For example, given a certain econometric result, in many cases it is enough to just include another variable, or to slightly modify the model assumptions

[17] Beker (2005).

[18] Hicks maintained that because economics theories can neither verified nor falsified economics is a discipline, not a science.

[19] Mayer (1993: 148)

or the estimation method to get different, and even opposite, results.¨ (Beker, 2005, p. 4).

Colander (2008) himself gives an example which shows this lack of robustness in empirical results. He mentions the DSGE model analysis in Ireland (2004) and the discussion of that paper in Juselius and Franchi (2007).

Juselius and Franchi replicated the results in Ireland and tested the assumptions underlying the model used by this author. Essentially all of them were rejected. Even more seriously, when the model was reformulated using an alternative approach, the conclusions were reversed.

Given the fact that, in general, economic theories cannot be falsified they accumulate and remain available inside a big toolbox to be used according to the case under analysis and the analyst's expertise.

So, it seems very difficult to find some yardstick which may allow making a distinction between ¨right¨ and ¨wrong´ economic theories.

However, what one does notice is an overwhelming predominance in the literature of papers dealing with ¨well behaved¨ models. Most of the scholars´ effort is devoted to study ¨health¨ and very little to analyze ¨illness¨ in economics.

But, of course, it is economic illness which causes concern to society.

There is a lot of effort devoted to show why, most of the time, the economy works smoothly, and very little effort to the analysis of why, from time to time, the economic mechanism breaks down and –more important- what is needed to fix it.

But these failures in the economic mechanism have huge economic and social costs.

This little effort devoted to the study of economic failures reflects in the poor attention paid to curing economic illness. As O. Blanchard et al. (2010, p.9) recognize ¨there is a lot we do not know about the effects of fiscal policy, about the optimal composition of fiscal packages, about the use of spending increases versus tax decreases, and the factors that underlie the sustainability of public debts.¨

Thousands of pages have been written to show the benefits of global financial integration and very few to draw attention to the risks it involved.[20]

So, it seems that priorities in the economic theory agenda are misplaced. Studying economic pathologies and how to cure them should be more encouraged while fewer resources should be devoted to merely show why an economy is in good health.

Let us now make a review of the main issues at stake in the discussion between orthodox economic theory and its critics: rationality, economic agents and collective behavior, the use of mathematics in economics, the hard core of economic studies and the nature of economics.

The assumption of rationality

Economic agents make decisions and we have to make some assumption about how these decisions are taken.

It seems a reasonable assumption to postulate that people are rational, i.e. they use the adequate means to obtain their goals.

[20] See Stiglitz (2010).

To assume that people are rational does not necessary mean we postulate they always act rationally in the real world. It shows what the real world would be *if* people were absolutely rational in their decisions.

It is a benchmark against which to compare real world behavior.

In any case, the deviations from the benchmark explain why in the real world there are behaviors which depart from the ones forecasted by economic theory.

However, the problem arises when economists disregard any seemingly non-rational behavior as if rationality were not a theoretical assumption but a condition that necessarily holds in the real world.

¨Animal spirits¨, herd behavior, are examples of types of behavior observed in real life which cannot be disregarded just arguing that they are incompatible with the rationality assumption.

In any case, they are precisely the proof that people in the real world not always behave as the rational assumption predicts.

Moreover, in many cases, rational decisions at the individual level result in irrational ones at the aggregate as when everybody tries to leave a cinema during a fire.

The interaction among multiple agents is the source of many unexpected results in the economy. This interaction may give way to a collective behavior which is quite different from the one expected from simply scaling up the behavior of the individual agents.

So, we have here two issues to deal with: non-rational behavior and collective behavior.

Let us start with the first one.

Bounded rationality

Nobel Prize Herbert Simon (1955,1991) introduced the concept of bounded rationality in economics. He addressed one of the difficulties mentioned by Elster: the limitations in the cognitive capacity of the economic agent to process all the necessary information to arrive at an optimal decision. So, he proposed to assume that economic agents are not optimizers, that they are satisfiers. Once the agent arrives at a satisfactory situation or result s(he) will not seek to make any changes to it. This idea runs at variance with the traditional view in economics (unbounded rationality) that there is no satiation level which could place an upper limit on a maximization process. It also means to venture into what Sims (1980) –reflecting a widely extended thought of traditional economists- characterized as the wilderness of irrational expectations and bounded rationality.

Akerlof and Yellen (1985) show how a fraction of boundedly rational agents in an economy, though suffering utility or profit losses which are second order small, may cause first order effects on market outcomes. They called near-rational this kind of bounded rational behavior.

Broadly speaking, bounded rationality models are more descriptive than predictive. In many cases, the bounded rationality assumption does not lead to a defined outcome as, for instance, market equilibrium. In most cases, the answer is may be, depending on the exact conditions.

As in path dependence models, initial conditions and chance events may dictate the outcome.

Indeterminacy of results is something the economic profession abhors. Although psychology and economics provide wide ranging evidence that bounded rationality is important to *describe* actual economic behavior, unbounded rationality provides determinate outcomes. Determinacy is more appreciated by economists than accuracy[21].

An outstanding example of this has been the approach to the issue of increasing returns. Although Adam Smith already in 1778 put a great emphasis on increasing returns as an explanation for specialization, this assumption had the entrance forbidden to the economic paradise because it was considered that assuming increasing returns could lead to the "wreckage of the greater part of general equilibrium theory."[22] Only in the 1980s some economists like Paul Krugman dared assume increasing returns in international trade theory, industrialization, and growth theory, simply assuming away the problems that multiple equilibria and increasing returns raise.

A somewhat related issue is the trade off between explanation and prediction.

The traditional point of view in science has been that its main purpose is *explanation*. Nagel (1961, p. 4), for instance, maintains that it is the desire of getting an explanation what lies in the origin of science.

However, the accent placed by Friedman (1953) on *prediction* has led most economists to consider prediction –instead of explanation- as the distinctive characteristic of scientific thought.

There are many examples of respectable theories which predict without providing an explanation. Newton's theory of gravity is a distinguished case. It tells us how bodies attract each other but it fails to identify the mechanism responsible of the motion of bodies.

There are no less distinguished cases of theories which explain without making any prediction. Darwinian theory has been identified as a typical case of an explicative theory which provides no prediction at all.

So, we have explanatory theories and predictive ones. If a theory explains, it helps to understand a phenomenon. If, additionally, it predicts, it is twice as useful.

The essence of scientific activity is to find an answer to the multiple questions that mankind faces. When an answer is not available, prediction is a good *second best*, but it is never a first best[23].

The bias towards prediction made economists to prefer models with quite unrealistic assumptions which explain nothing of the real world to others which do explain but do not predict.

Emphasis on formal aspects -deductive rigor, economy and elegance of expression- are also considered more important than the usefulness of the results. ¨The final aim is to provide the aesthetic pleasure of a beautiful theorem, to solve academic exercises that we have constructed because they are soluble by existing analytical techniques, and not to provide substantive insights into observable behavior."(Blaug, 1994, p.131)

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[21] Some economists argue that teaching economics imposes on the profession the need for clear cut results. Students need easy, simple recipes. In this respect, Colander prevents that, in some way, teaching may turn into cheating.

[22] Hicks (1939: 84).

[23] In this respect it is worth while mentioning that the predictions made by the Ptolemaic astronomy are as good as those made by the Copernican one.

The rational-agent models excel in elegance and precision. However, as Nobel Prize Kahneman (2003:1449) remarked, this is just another way of saying they are psychologically unrealistic. It is true that, by definition, every model implies a certain degree of unrealism in the assumptions –it is a simplification of the real world-; the limits of the portion of the world to which the model applies are conditioned by those assumptions. So, the model´s assumptions limit the portion of the real world to which it may be applied.

For example, Galileo's law of falling bodies contains the assumption that bodies fall in the vacuum. If we use a cannon ball in an experiment we will find that it behaves according to that law. But if we use a feather falling in the atmosphere, we will find out that the law apparently fails: the lack of vacuum makes the law inapplicable in this case while it is irrelevant in the cannonball case.

Something similar happens in economics: a model built under the assumption of a closed economy is of little use in an open economy. In the same way, models built using the assumption of perfect competition are not relevant for highly concentrated markets.

So, more important than to discuss how near or how far from reality are these or those assumptions is to establish in each case the *limits* of applicability of any economic model or theory. This implies the identification of the assumptions on which the conclusions are sensitively dependent.

On this basis, bounded or unbounded rationality are assumptions which may be reasonable to use, as with other model ingredients, according to the context of the problem we have to address.

This is quite different from maintaining that unbounded rationality is the sole assumption compatible with economic analysis.

Nevertheless, the idea of bounded rationality has not been convincing to most economists. It is not that economists think people are unbounded rational: clearly they are not. The argument has been that they act as if they were unbounded rational. Learning would allow them to reach optima thorough practice. If so, what is the benefit the bounded rationality assumption brings to economic theory, they ask.

However, the learning argument only applies to repetitive activities, as everyday consumption or production. But when the issue has to do with, for instance, investing in a new financial instrument, learning may imply having the experience of undergoing a financial crisis before arriving at solid conclusions. Fortunately, financial crisis do not happen every day. So, unbounded rationality seems to be an extremely unrealistic assumption in this case.

The behavioral economics contribution

The departure point for behavioral economics has been the fact that people do not behave as the neoclassical theory says they do.
Behavioral economists argue that this happens because neoclassical economists ignore important variables which affect behavior. These new variables are typically shown to affect decisions in experimental settings. However, the difficulty is that most of these new variables may be unobservable or even difficult to define in economic settings with economic data[24].

[24] See Pesendorfer (2006).

The typical contribution starts with a demonstration of a failure of some common economic assumption (usually in some experiment) and proceeds to provide a psychological explanation for that failure.

In this respect, the main contribution of behavioral economics has been to put in evidence the failures of the standard model of individual behavior and provide an explanation for them. Unfortunately, it has been very difficult to apply its contributions outside the laboratory.

Behavioral economics provides psychologically-based explanations for different failures found when using the standard rational choice model in economics. For instance, one of the first contributions was Kahneman and Tversky´s development of prospect theory to address the failures of expected utility theory.

They showed that when analyzing choice under uncertainty it is not enough to know the lotteries an agent is choosing over. Rather, one must know more about the subject's situation at the time s(he) makes her/his choice.

A large majority of individuals behave as risk takers when confronted by a problem presented in terms of loss while they behave as risk averse when the same problem is presented in terms of gain. This behavioral inconsistency is called the 'framing effect' and demonstrates that the representation (framing) of a problem may be crucial in ordering the preferences.

Numerous experiments have confirmed this framing effect.

So, prospect theory distinguishes between gains and losses from a situation-specific reference point. This allows explaining, for instance, why agents are less likely to sell assets that have incurred losses than assets that have incurred gains. However, when prospect theory is applied to economic settings, it is often impossible to identify the reference point.

Prospect theory is part of behavioral economics. As a matter of fact, behavioral economics does not rest on a unified theory; rather, it consists of a bunch of theories.

In a very comprehensive survey, Stefano DellaVigna (2009) summarizes a list of papers that document aspects of behavior that deviate from the forecasts of the traditional economic theory in different steps of the decision-making process. He groups these deviations in three categories: nonstandard preferences, incorrect beliefs and systematic biases in decision making. The novelty is that the papers surveyed by DellaVigna present evidence in market settings context of these behaviors that were previously detected in laboratory experiments.

DellaVigna also discusses the usual objection: why do not market forces eliminate non-standard behavior. Among other reasons, he mentions the fact that many important decisions are taken seldom, with limited scope for feedback and sorting. In other cases, such as in financial markets, feedback is noisy.

He also rejects the aggregation argument which asserts that the biases at the individual level should not affect aggregate market outcomes. In this respect he mentions the limits to arbitrage argument presented by DeLong et al.(1990) and the fact that, in most settings, there is no incentive to eliminate biases; so, the effect of nonstandard behavior aggregates linearly. Finally, he refers to papers on behavioral industrial organization which indicate that the nonstandard features, far from having no impact, can have a disproportionate effect on market outcomes.

Collective behavior

Although economics main concern is with aggregates, there has predominated in the discipline an atomistic approach. If you want to know what consumers do, you model the individual consumer behavior and assume it represents the behavior of the typical consumer. The same applies to producers: the theory of the firm is the basis for the aggregate supply function. Moreover, it has been proposed that the actual economy can be read as if it were acting out the maximization of the utility function of a single, immortal representative agent. This excludes *per se* any possibility of coordination failure. But many problems in the economy arise precisely from coordination failures and heterogeneous behavior by economic agents.

Economics deals with the study of the economic system. Why not starting by studying the economy as a system?

As we have said above, the interaction among multiple agents may give way to a collective behavior which may be quite different from the one expected from simply scaling up the behavior of the individual agents.

¨How individual agents decide what to do may not matter very much. What happens as a result of their actions may depend much more on the interaction structure through which they act –who interacts with whom, according to what rules¨.[25]

As Philip Ball (2005) argues in his book Critical Mass, winner of the Adventis Prize for Science Books, physics has developed tools, methods and ideas to study systems whose components parts have a capacity to act collectively. So, they seem especially accurate to analyze collective behavior in economics.

The first requisite for this is to change the departing point in economics. It should be not the individual but the economic aggregates. These aggregates are the result of the behavior of many agents, all interacting with one another at once. So, collective behavior and not individual behavior should be the departing point of economic analysis.

Orthodox economics demands for microfoundations as a necessary condition in macroeconomics. But, for instance, thermodynamics and chemistry do not claim for a micro theory. All biological creatures are made up of particles. This does not mean that the natural place to start in building biology is to start with particle physics. Botanists study certain characteristics of the behavior of plants without knowing the exact biochemical mechanism behind them. Zoologists study anthills without having to resort to the individual behavior of ants. It is well known that relativity theory (macrophysics) and quantum mechanics (micro-physics) are mutually inconsistent. Why should economics demand what harder sciences do not?

An interactive complex system

The economic system is a supremely interactive one. Economic agents influence one another directly. A rush to buy or sell a particular asset can prompt others to do the same. Crashes are an example of stampede phenomena in which individuals act simultaneously in a herd-like and sometimes panic-stricken manner.

Although ever since Veblen it is well known that consumption choices may be affected by consumption choices of others, the only reference to this has been Leibenstein´s

[25] Arthur et al. (1997: 9).

(1950) analysis of the so called bandwagon, congestion and snob effects, which in any case have remained as a sort of footnote to the theory of demand, when mentioned. This, in spite of the fact that fashion and trends play an increasing role in consumers´ demand.

In general, microeconomic models usually ignore interaction and consider individuals as isolated entities which take decisions independently one from the other.

In fact, a basic assumption of the general equilibrium theory is that the only interaction among economic agents is through the price system. Assuming that the preferences and hence the choices of one individual are influenced by others introduces an important element of uncertainty which conspires against the possibility of arriving at a stable price equilibrium. On the other hand, a basic tenet of traditional mainstream economics has been that aggregate behavior must be derived from underlying rational microfoundations[26]. So, agents´ interactions are discarded at the micro level and, at the same time, to be acceptable, macro models are supposed to be derived from this sort of micro models.

The feedback that one's decisions have on others´ expectations and behavior is usually ignored.

However, already in the ´1930s, Keynes likened asset markets to beauty contests, where people have to guess which of the participants would get the most votes. In the same way, investors in asset markets try to guess which asset will be favored by other investors´ preferences in order to invest in it, independently of other factors.

This sort of conduct may pave the way to a herd-like behavior. Episodes of collective mania are well known in economic history since the tulip mania in seventeenth century Holland -where tulip prices ballooned absurdly- to the recent subprime mortgage market crisis.

Yet, as Ball (2005, p.175) mentions, ¨irrational does not mean unpredictable¨. On the contrary, he cites physics-based mathematical models of pedestrian movement applied to predict the behavior of a panic-stricken crowd. This sort of models of pedestrian motion aimed at planning urban systems might be used to better understand herd-like behavior by economic agents.[27]

Since the end of the eighties, multi-disciplinary research as done at the Santa Fe Institute has stimulated a lot of work on interacting agents in economics and finance. Models of interacting particle systems in physics served as examples of how local interaction at the micro level may explain structure at the macro level.

In order to take account of the difference of behavior among economic agents in the financial markets an increasing number of structural heterogeneous agent models have been introduced in the economics and finance literature. Financial markets are viewed as complex adaptive systems consisting of many boundedly rational, heterogeneous agents interacting through simple investment strategies, constantly learning from each other as new information becomes available and adapting their behavior accordingly over time.

For instance, Brock and Hommes (1997) consider a market with an endogenous evolutionary selection of expectations rules. Agents choose between a set of different

[26] As stated above, this is something in no other science is required.
[27] See, for instance, M. Batty (2005).

forecasting rules and tend to switch to forecasting strategies that have performed well in the recent past. In Brock and Hommes (1998) this evolutionary selection of strategies is applied to a standard asset pricing model. Agents choose between fundamentalists' and chartists' investment strategies. When the sensitivity to differences in past performance of the strategies is high, evolutionary selection of strategies destabilizes the system and leads to complicated, possibly chaotic asset price fluctuations around the benchmark rational expectations fundamental price. The fluctuations are characterized by an irregular switching between a quiet phase with asset prices close to the fundamental and a more turbulent phase with asset prices following (temporary) trends or bubbles. In contrast with Friedman's argument –that irrational agents will be driven out of the market by rational agents- chartists may on average earn (short run) profits equal or even higher than (short run) profits of fundamentalists.

On the same line of analysis, Honggang Li and Barkley Rosser Jr. (2001) studied the behavior of a model of asset market dynamics with two types of traders: fundamentalists and noise traders. Complex dynamics and greater volatility are seen to emerge as certain parameters in the system are varied.

Brock et al. (2009) extend the asset pricing model with heterogeneous beliefs of Brock and Hommes (1997, 1998) by adding contingent claims or Arrow securities and investigate how these hedging instruments affect market stability. A fairly robust result is that if there are a sufficient number of traders who extrapolate trends, then increasing the number of hedging instruments may well increase the volatility of the markets and lower the welfare generated by the market.

However, as Rosser (2010) points out, it would seem that rather than an unambiguous increase in variance, what may be happening is a reduction of variance coinciding with an increase in kurtosis, a fattening of the "fat tails." Such an outcome might well be derivable from the Brock et al. model if there is a sufficiently nonlinear responsiveness of the movement in and out of being trend extrapolaters, which would be consistent with more general results found in Brock and Hommes (1997), where increases in the willingness to change strategies tends to destabilize and complexify dynamics.

Although speculative bubbles have been observed in laboratory experiments by Nobel prize-winning Smith et al. (1988) and Hommes et al. (2005), it remains a topic for future research the estimation of interacting agent models on actual financial data.

Another promising line of economic modeling is Agent-based Computational Economics (ACE), the computational study of economic processes modelled as dynamic systems of interacting agents.[28] An ACE macroeconomic model might include structural agents (e.g. a spatial world), institutional agents (e.g. a legal system, corporations, markets), and cognitive agents (e.g. entrepreneurs, consumers, stock brokers, and government policy makers). ACE models implemented on modern computational platforms can include millions of heterogeneous interacting agents. Such models seem to be well suited for analyzing an economy in extreme situations, e.g., for evaluating the probability of a financial crash and recommending appropriate recovery policies.

Fat tails

[28] See LeBaron and Tesfatsion (2008).

It is well known since the famous contribution of Mandelbrot (1963) that many economic and financial time series have fat tails, i.e. that the probability of extreme events is higher than if the data-generating process were normal.

However, the usual practice among orthodox economists has been to assume –implicitly or explicitly- a normal distribution. For example, the well-known Black-Scholes model, extended by Merton, aimed at option pricing, assumes normality in the distribution of events.

As Merton and Scholes themselves learned the hard way in 1998, one year after getting the Nobel Prize precisely for their theory of options pricing[29], small probability events do happen in the real world[30]. So, they deserve more consideration by economists.

T. Kaizoji (2004) presents a model with heterogeneous agents (fundamentalists, chartists and noise traders) where, if the nonlinearity of the excess demand is sufficiently strong, a speculative bubble is observed. Fundamentalists are driven out of the market and a fat tail distribution of market returns appears. However, the model appears to be too simple to mimic all characteristics of real return series.

Extreme Value Theory, used initially in the geology and flood control literature and more recently in finance, may be a useful instrument although, perhaps, predicting extreme events will always be a very difficult thing to do. But this does not mean economists should ignore them. This means that economists should be attentive to the possibility of unusual events and always take into account the worst scenario possible.

On the use of mathematics in economics.

One of the critics to traditional economics has been its ab(use) of mathematics. So did Krugman in the quotation included at the beginning of this article. A web petition in support of those words had over 1300 signatures in 2009, most of them from qualified academics. According to Lawson (2009, p. 130), ¨the project of mathematical modelling in modern economics has a long history of failure.¨

This is an issue which has been broadly discussed in the 1940'sand 1950's but which periodically reappears.

It has been argued that economics suffers from physics-envy. However, although physics provides tools to deal with complex systems as the economy is, most of them have been only marginally used in economics.

The truth is that what mainstream economics may be found guilty of is not of physics-envy but of mathematics-envy.

Economists have taken physics as the model for science. Physicists use two basic tools: laboratory experiments and mathematics. But as laboratory experiments have a very limited application in economics, this leaves mathematics as the main tool for economists to try to mimic physics. So, economists hugely borrowed the mathematical instruments used by physicists.

They did to such an extent that, for instance, for the philosopher of science Alexander Rosenberg (1992) economics is not an empirical science at all; for him, it is a branch of applied mathematics.

[29] Fisher Black had died in 1995.

[30] In 1998 the hedge fund Long-Term Capital Management went on the brink of bankruptcy after losing $4.6 billion in less than four months, leading to a massive bailout by other major banks and investment houses. Merton and Scholes were members of its board of directors.

The general equilibrium theorist and Nobel Prize winner Gerard Debreu (1991, p. 5) admits that *the use of mathematics imposes certain restrictions on economic theory*. The very choice of the questions to which the economist tries to find answers is influenced by her mathematical background. Economics may become secondary, if not marginal, in that judgment. Mathematics is a demanding master: it ceaselessly asks for weaker assumptions, for stronger conclusions, for greater generality.

Mathematical models must be manageable and easy to handle. This however requires drastic omissions and simplifications, often at the expense of the models' ability to capture relevant phenomena. So, in many cases economists conclude with models which exclude everything which is of interest for policy making.

Mathematics is a language, as Samuelson reminded economists, popularizing Gibbs´s sentence. It is no less but no more than *a* language. There is no reason to assert that it is *the* language of economics.

Of course, the advantage of mathematization is that it prevents logical mistakes. Given the difficulties for experimenting in economics, economic theory is strongly dependent on logical reasoning. In physics, factual observations and experimental results provide a constant check on its theoretical constructions; this allows employing occasionally some reasoning which violates knowingly the canons of mathematical deduction. This is not acceptable in economic theory where internal consistency is the only guarantee of rigor.

But is logical rigor necessarily equivalent to using mathematical language? In this respect, we must remember that the most influential texts in economics have been *non-mathematical*. For example, Friedman and Schwartz (1963) made more in favour of the monetary approach than many sophisticated econometric models, not to mention, in the opposite stream, Keynes' s General Theory.

So, it is difficult to share Cochrane´s (2009) condemnation of the literary style of exposition in economics as an almost deadly sin.

The broad use of mathematics in economics has more to do with the aim of providing the aesthetic pleasure of a beautiful theorem than to provide substantive insights into observable behavior.

The more impressive the use of quantitative techniques or methods, the more likely that a paper will be accepted by the editorial board of academic journals. Unfortunately, this premium on quantification has had serious adverse consequences, including a misallocation of research efforts in economics.

One must bear in mind that mathematics is just a tool to guarantee logical consistency. If logical consistency can be assured without mathematics, what is the point of using it?

On the other hand, if it allows arriving at conclusions not reachable with only logical reasoning, why not use it?

As a matter of fact, one can be dogmatic with blackboard diagrams and open-minded with reams of equations.

In general, less mathematics has the advantage that lowers the barrier to critical thinking, but simply getting rid of it would imply disregarding an important tool for economic analysis.

There are some economic problems which require a mathematical approach to assure a rigorous treatment while there are others which can only be approached using a literary style.

So, one should conclude that neither the use nor the non-use of mathematics in economics can be a necessary condition for judging its scientific standards.

Studying economic anomalies

After discussing how to study the economy, the second issue is what to study. The natural answer is: economic problems, of course. This may sound rather obvious but most of the orthodox economists´ efforts are devoted to show the non-existence of economic problems. The bulk of their papers are aimed at showing how the market solves by itself any potential conflict or difficulty. If so, there is no economic problem to work on.

As stated before, it should be economic illness rather than economic health the main object of economists´ efforts.

The 1930 crisis inspired the main contribution by Lord Keynes to economic analysis. His ideas paved the way for a huge improvement in economic policy. As a paradoxical by-product of this improvement, many economists announced that economic fluctuations and crises were no longer a subject to be studied by economists but only by historians.

'The economy of the 1990s suggested to [a new] generation of students that the business cycle was no longer of practical importance' (Mankiw (2006, p.37).

Several writers dubbed "the Great Moderation" the remarkable decline in the variability of economic variables which took place during the last part of the 20th century. However, the validity of this concept as a permanent shift has been questioned by the economic and financial crisis that started in 2007. There have been also some previous signals as the 1987 stock market crash, the1998 financial crisis triggered by the failure of the Long Term Capital Management or the bursting of the dot-com bubble in 2000, but the limited effects of them were considered an argument in favor of the theory that crises were only something of the past.

Moreover, problems like poverty, unemployment and slow growth have been present even during the so called Great Moderation although they deserved only a marginal consideration by mainstream economists.

In order to elaborate a new order of priorities for the agenda of economic research it is important to identify the problems to which that research should be addressed.

Economic fluctuations, financial crises and financial regulation, poverty, unemployment and sustainable growth seem to be the undisputable candidates.

Is there a unique economic theory or a collection of economic theories?

Economics has borrowed from physics the concept of symmetry.

However, as we have seen above, there is an asymmetric behavior when people have to face either a loss or a gain.

There are other asymmetries in economics.

For example, a long time ago I claimed[31] that the asymmetry in the price level behavior –flexible upwards, inflexible downwards- gives birth to an asymmetry in economic theories. Monetarism fits well with facts for upwards movements: normally, the main effect of an increase in monetary supply is an increase in prices. However, a liquidity

[31] Beker (1986).

contraction has little effect on prices but generates a fall in real output and in the level of employment, confirming Keynesianism claims.

Something similar happens in general in the discussion between orthodox and heterodox economics. Both contain some grain of truth but not the whole truth.

Orthodox economists represent the economy as a stable equilibrium system resembling the planetary one. The concept of equilibrium plays a key role in traditional economics.

This approach is useful in normal, stable times, when what happened yesterday is the best guide to what will happen tomorrow.

However, it is incapable of dealing with unstable, turbulent, chaotic times.

Heterodox contributions shed much more light on what happens during these exceptional although crucial periods in which a good part of the economy is reshaped and provide powerful insights towards what policies to follow in those extraordinary circumstances. However, they remain as theories mainly suitable for those periods of instability and crisis.

Thus heterodoxy and orthodoxy are both a one-way street. The first is useful only when the economy is in trouble; the second, when it is stable.

The challenge is to arrive at a unified theory valid both for normal and abnormal times.

In this respect, the complexity approach with its use of non-linear models offer the advantage that the *same* model allows to describe stable as well as unstable and even chaotic behaviors.

However, one should bear in mind that up to now there is not a unified theory in physics. Moreover, as we stated before, general relativity theory and quantum mechanics are mutually incompatible.

So, perhaps, as Elster suggests, one should be less ambitious with economic theory.

Compared with the present situation it would be a big leap forward if we could devote a greater effort to research on subjects as financial crises and financial regulation, economic fluctuations, poverty, unemployment and sustainable growth.

It would be even more important to convince the whole profession that there is nothing like ¨the¨ economic theory and instilling in every economist a sense of respect for those theories and models s(he) does not share or like. Instead of disqualifying rival theories it would be better to react looking at them for worthwhile elements.

Instead of a unique economic theory there is a collection of economic theories -our collective diversified intellectual portfolio-, some of them in competition with each other, and it is the applied economist who has to choose among them the appropriate tool to use in each case as the carpenter chooses the proper instrument from her(his) toolbox according to the task s(he) has to do.

What help does s(he) has in choosing among competing economic theories? The main one comes from experience.

Although refutation does not usually come through the empirical tests learnt in the statistics and econometrics courses it does come through what I have called “big social

experiments."[32] They are the "big events" alluded by Tobin (1996) which discredit ideas and replace them with new ones.

The Great Depression in the 1930s, for instance, discredited the idea that full employment of resources could be automatically reached. Today, no reasonable economist in the United States would cast doubts about the role of the Federal Reserve and its monetary policy in stabilizing the economic cycle.

In the same way, for many years the role of monetary policy in inflationary processes was discussed. Moreover, even non-monetary inflation theories were developed. But the processes of high inflation of the 1970s and the cases of hyperinflation, like the Argentinean one in the late 80s, left no doubts on the necessary existence of a *monetary* component in these processes and on the need to resort to the monetary policy to control them.

The 1987 stock market crash persuaded more economists to put aside efficient market theory than any econometric result.

Finally, if something we have learnt from the recent financial crisis is that financial markets are too important a matter in economic life to be left unregulated or bad regulated.

Which way forward?

Identifying the flaws in economic theory is easier than defining a way to get rid of them. But some guidelines can be sketched which have to do with: 1) the methodological approach; 2) the contents of economic theory.

1.- *The methodological approach.*

1.1.- First of all, economists should remember that the main purpose of science is explanation. If a theory explains, it helps to understand a phenomenon. If, additionally, it predicts, it is twice as useful. When an answer is not available, prediction is a good second best, but it is never a first best.

1.2.-The choice of the questions to which the economists try to find answers should be dictated by economics –theoretical and applied- and not by the possibilities of mathematically modelling the answers.

The usefulness of the results should be considered more important than formal aspects such as analytical elegance or economy of theoretical means.

Mathematics is just a tool to guarantee logical consistency. But logical consistency may also be warranted without the use of mathematics, depending on the sort of problem one wants to solve. The method should be subordinated to the problem, not the other way around.

Economists should bear in mind that the most influential texts in economics have been non-mathematical.

1.3.- Accuracy should not be sacrificed at the altar of tractability or determinacy.

1.4.- The departing point in economics should be not the individual but the economic aggregates. Microfoundations are not a necessary condition for macroeconomics.

1.5.- There is nothing like ¨the¨ economic theory. There is a collection of economic theories, some of them in competition with each other. The process of natural selection

[32] Beker (2005).

defines which survive and which do not. "Big social experiments¨ discredit some ideas and replace them with new ones.

1.6.- Economists should be more open-minded in their discussion of the research results from other economists. There should be a sense of respect for those theories and models one does not share or like. Dissenters should not be treated as those boring old aunts always having something to grumble about at family parties. Instead of disqualifying rival theories it would be better to react looking at them for worthwhile elements.

1.7.- This also implies that editorial boards of leading journals need to be willing to review submitted research papers that are less conventional, less mathematical or more critical about the received theory, and insist on a serious discussion of other empirical results on the same topic. Journals should also be less closed-shop-like in terms of specific nationalities, universities, and research centers.

1.8.- Journals should encourage authors of empirical papers –or its critics- to test the hypotheses included in them by using new data some time after publication in order to verify the robustness of the results.

1.9.- It is the applied economist who has to choose from the economists´ portfolio the appropriate tool to use in each case. This is the art of economics, to use the concept introduced by John Neville Keynes.

2.- *The contents.*

2.1.- In this respect, it is not just an issue of changing the answers. What should be changed first are the questions or, at least, the priority given to them.

For instance, the contemporary economy has been transformed by the forces of technology and entrepreneurship. However, after Schumpeter, little attention has been paid to the economic explanation of the forces behind these changes.[33]

2.2.- Economic illness rather than economic health should be the main object of economists´ efforts.

2.3.-The main problems to which research should be addressed are economic fluctuations, financial crises and financial regulation, poverty, unemployment and sustainable growth.

2.4.- Economic research should pay more attention to institutional aspects and inter-agent heterogeneity, as well as inherent conflicts of interest between agents on different sides of the market, as recommended some years ago by Hendry (2004).

Researchers should pay attention to issues concerning the coordination of actors and the possibility of coordination failures.

The global financial crisis has revealed severe dysfunctional institutions that need to be adapted, revised, or even abolished. Risks turned out to be strongly mispriced, while new financial institutions and instruments posed a threat to both financial stability and the efficient operation of financial sector functions.

2.5.-The financial crisis has underscored the need to reform the regulatory and supervisory architecture of the financial system. The importance of this undertaking, and doing it properly, cannot be overstated.

[33] Baumol (2002) and Baumol et al. (2007) are two of some few exceptions to this assertion.

It is urgent to address the broad-based problems of the financial system - chiefly, to eliminate the incentives for the risky bets that necessitated government bailouts.

The role of rating agencies has to be redefined: at least, their fees can no longer be paid by the issuer of the securities they are supposed to qualify. To set up a public credit ratings agency would be a second step towards correcting the present perverse incentive system facing private agencies.

2.6.- On the other hand, we need also an updated theory of economic regulation which should answer both the public concern about the powers of the regulators as well as the problem of regulatory capture –when regulatory bodies become advocates for the industry they are supposed to be regulating,

Given that there is no 'regulator's regulatory body' in existence, effective regulation should ensure that regulators fulfil their duties by aligning their incentives with the public interest.

There must be also external bodies to which regulators are accountable. Although discretion is needed for powerful decision-makers, the challenge is to provide an appropriate level of control over those decision-makers.

Conclusions

The outburst of the 2008 global economic crisis sparked a myriad of criticism on mainstream neoclassical economic theory, held responsible for not even have considered the possibility of the kind of collapse that the subprime mortgage meltdown unleashed.

However, an analysis on the causes of the recent financial crisis shows that it was, first of all, a case of massive malpractice. Such a massive case of malpractice denounces deep failures in the regulatory system.

The deregulation movement that took place during the 1980s and 1990s was inspired by an almost religious belief in the power of market forces to solve any economic problem. Mainstream neoclassical economics bears the responsibility of having nourished that belief.

Although identifying the flaws in economic theory is easier than defining a way to get rid of them, 15 guidelines are sketched out for improving the methodological approach as well the contents of economic analysis.

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