

Luigi Spaventa

## The economists and the crisis: a note

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### 1.

By now there is little to be added to the narrative of the financial crisis and to the analysis of its proximate and remote causes. A debate on the lessons of the crisis for economics as a discipline and for its practitioners is instead only just beginning. This is the theme of this note, without much pretence to organized thought

In the past year or so bashing the economists has become a fashionable sport. Why didn't you tell us, asked HM the Queen of England when visiting the London School of Economics. An Italian minister said something in Latin which translated into plain English is an injunction to economists to just shut up. Old jokes have been resurrected, sardonic books and articles on the theme written by journalists have come out. Mock trials to the profession have been organized. More seriously, some economists (Daren Acemoglu, Willem Buiter, Paul De Grauwe, Barry Eichengreen, Simon Johnson, Paul Krugman, Roberto Perotti, Pietro Reichlin, Ignazio Visco, Charles Wyplosz and more) have themselves initiated interesting and thoughtful soul searching exercises, mostly in the form of short papers and op ed or blog columns. Recently (and after this piece had almost been completed) *The Economist* (July 18) devoted its main leader and two extensive briefing articles to "What went wrong with economics". The profession's reaction to these serious and less serious provocations has betrayed embarrassment or has been absent, perhaps in the belief that business as usual, as if nothing had happened, is the best reply. Reputation was not helped by the policy debates that have taken place since mid-2008, where disparate and discordant pieces of advice were given, belatedly but always with arrogant certainty.

Neglecting these policy debates, I wish to consider whether economists and/or economics failed, and if so why. Before doing that, let me however dispense with two intellectually vulgar allegations (to which pointed replies have already been given (see Perotti (2009) for instance).

One is that economists did not foresee the crisis in time. Nor do seismologists or oceanologists anticipate the precise time and place of a quake or of a tsunami. Indeed practitioners, be they policy-makers, central bankers or market participants, fared no better, as the reading not only of official reports but also of private sector analyses up to June 2007 can easily document. Some, especially non-economists, claim that they had predicted a crisis for years. Let us draw a distinction between well argued and documented diagnoses based on serious analyses of the system's fault lines (of which there were some, as I shall report) and the recurrent generic utterances of professional doomsayers of the "I told you so" variety: the only clock that is astronomically precise twice a day is a stopped clock

Another vulgar and irrelevant allegation is that economists are unable to understand reality because of the abstraction of their theories and models. Woolly and inconclusive thinking or mere descriptions of events are of little or no help: there is nothing as useless as a map of scale 1:1, as Joan Robinson used to say.

In the rest of this note I shall ask the following questions. First, if not forecasting the crisis, were economists at least aware that the system had set on an unsustainable path possibly leading to one? With some remarkable exceptions they were not (section 2), as also shown by the long time it took many of them to understand that the dislocation that started in June 2007 was a serious matter is further proof. Some have drawn a distinction between economic theory, which in their view provided adequate tools to understand the crisis, and economists who did not use those tools because affected by selective blindness. I shall argue (section 3) that this distinction is unconvincing: the tools available were inadequate, though in the field of macro- rather than in that of microeconomics. The problem then is why macroeconomic modelling, especially in its version for policy consumption, was so patently unfit to accommodate financial phenomena (section 4). My final question is whether the costs of these failures are confined to a reputational damage for the profession: I surmise (section 5) that there have been externalities, insofar as the economists' doctrines and attitudes played contributed to an environment in which the germs of financial dislocation could prosper and grow.

## **2.**

Whether my first question is relevant very much depends on one's view of the nature of this crisis. Mine is that we were not confronted with the effects of some

sudden and unexpected shock, but with the endogenous and eventually unavoidable outcome of developments that had shaped the financial system (and in this sense the term “bubble” may be misleading).

Given this premise, though economists cannot be expected to have provided precise forecasts, it is legitimate to ask if they were aware that the financial system had set on an unsustainable path which could eventually lead to a crisis. Seismologists, though unable to anticipate exactly when and where a quake will happen, can identify the areas at risk, where anti-seismic rules must be followed in construction. We ought to recognize that here the profession as a whole fares poorly: though I am sure I am neglecting many, the list of those who forewarned that risks to systemic stability were growing – a different category from the doomsayers – is embarrassingly short.

The analyses of Shiller and others documented that the seemingly endless rise in house prices was an unsustainable anomaly, a conclusion accepted by most. But, as far as I know and with the exceptions I shall mention presently, the financial consequences of the likely bursting of the house price bubble in the brave new world of securitization were never considered. There was a literature on financial innovations in the field of structured credit products, but the nature of the new business model known as “originate to distribute” and its macro- and microeconomic implications were never properly explored if not to provide an unqualified praise of its benefits.

The literature on the 1997-1998 crises of South-Eastern Asia and Russia, which were different in nature, more local and less systemic, is of little relevance for understanding the current crisis. The LTCM case is more interesting from this point of view: the path opened by the 1997 paper by Shleifer and Vishny on the limits to arbitrage, which provided an uncanny anticipation of the reasons for the LTCM crash a year later, was however not pursued, with the exception of Rajan (2005). There were economists (Bordo, Eichengreen et al. (2001)) worrying that financial crises were growing more frequent and more severe. As for the steady production of models portraying crises as equilibrium outcomes, none of those we find in the excellent 2007 survey by Allen and Gale has to my knowledge been of much use for understanding the crisis that was just beginning.

Some *now* maintain that global macroeconomic imbalances have been the only, or at least the major cause the current crisis. I do not think that this is the case:

though important, they interacted with other factors and are not by themselves a sufficient explanation. Be it as it may, one thing is certain: the vast literature on imbalances was mostly concerned with currency crises. The one possible exception is Bernanke's savings glut (2005), arising from China's low propensity to spend and accounting in his view for low interest rates. Otherwise the major concerns were the destiny of the dollar exchange rate and why the dollar had not yet collapsed: was the dollar running in the vacuum, like Willy coyote, or were we living a new Bretton Woods, or was there something undetected in capital flows? All relevant issues, no doubt, but which had little to do with the growth of credit, leverage and risk exposure that was nurturing the imminent crisis.

Few were aware of the deteriorating macro-financial conditions. Foremost, Raghuram Rajan, in his 2005 paper provided a prescient analysis of how the developments observed in financial markets could easily degenerate into a crisis: he also anticipated banks' contingent commitments left them exposed to systemic risks in spite of the diffusion of the OTD model. Presented at Jackson Hole, Rajan's paper was criticized by Donald Kohn, of the Federal Reserve Board, for being too interventionist and by Larry Summers, who thought "the slightly lead-eyed premise of [the] paper to be largely misguided": it otherwise elicited no academic reactions, perhaps because it lacked a formal presentation. There was then Nouriel Roubini, who started predicting gloom and doom round 2005: his warnings went unheeded. So were those of the economists of the BIS, the one and only official source that (unlike for instance the IMF in its *Stability Reports*) expressed growing worries. In general, dissenters were often treated as those boring old aunts always having something to grumble about are at family parties. A separate strand of literature, associated with the names of Tobias Adrian, Hyun Song Shin, Markus Brunermeier and others dealt with funding and market liquidity, the effects of asset price movements on banks' balance sheets, leveraging and deleveraging cycles and related themes: it did not find at the time a proper place in macroeconomic modelling, but it has later provided some simple but potent conceptual tools for interpreting and understanding the crisis.

Not only were most economists unaware of, or unconcerned with, the tensions that were accumulating in the financial system. Even after the crisis started, in the early summer of 2007, it took many of them a long time to understand that what was going on was a serious matter. In this they were somehow reminiscent of a

philosopher in a famous Italian 19<sup>th</sup> century novel, who, unable to find a place for the plague in the accepted Aristotelian classification, decided that there was no plague (and eventually died of it). (Even here practitioners have done no better: as admitted by the Bank of England in its October 2008 *Financial Stability Report*, “while ...weaknesses had been identified, few predicted that they would lead to such dislocation in the global financial system”).

### 3.

There is a more intriguing question. To save the day, or perhaps to save the discipline if not its practitioners, some draw a distinction between economics and economists. According to Barry Eichengreen (2009),

“It was not that economic theory had nothing to say about the kinds of structural weaknesses and conflicts of interest that paved the way to our current catastrophe”: “the problem [was] a partial and blinkered reading of [the] literature” on the part of economists afflicted by a problem of “cognitive capture” and choosing to stick to mainstream models; “it is in this light that we must understand how it was that the vast majority of the economics profession remained so blissfully silent and indeed unaware of the risk of financial disaster”.

What Eichengreen, and many others (like Perotti (2009) and Reichlin (2009)), have in mind are all those developments of (micro)economic theory that provide obviously useful tools for an understanding of financial markets: agency theory, incentive theory, asymmetric information and its consequences, behavioural economics, models with heterogeneous agents and incomplete markets. Even more relevant, perhaps, is the recent literature on liquidity and leverage referred to above.

True, we can readily identify a case of asymmetric information in the issuance of CDOs or agency and incentive issues in the case of banks and hedge funds. But that is not enough. If the problem was only one of the practitioners’ selective blindness and not one of available tools, it should be possible for economists, now that their eyes are opened, to find a general macroeconomic framework into which all the various bits and pieces of relevant theory could be assembled to obtain an approximate but plausible account of the crisis and of its dynamics.

But does a general scheme or model exist that can accommodate financial asset, banks and financial intermediaries, heterogeneous agents and asymmetric information, agency problems and coordination failures and possibly institutions? Obviously it does not. While microeconomists were busy at work producing useful constructs relevant for an understanding of financial markets, macroeconomists of

different schools spent much time attempting to reconcile the dynamic general equilibrium models of the new classical school with the rigidities and market imperfections of the neo-keynesian tradition. The outcome of the macroeconomists' convergence effort was a generation of Dynamic Stochastic General Equilibrium (DSGE) models: the new workhorses of macroeconomics, as they have been defined, and widely adopted by Central Banks.

Of course DSGE models are impeccably microfounded; but their micro-foundations are hardly compatible with credit cycles and financial dislocations. Prices are sticky and labour and product markets imperfections cause deviations from Pareto optima. But otherwise financial assets are absent or modelled in a primitive fashion, with their prices assumed to reflect all available information under the strong version of the efficient market hypothesis; there are no coordination failures, as rational and forward looking representative agents behave consistently with the model of the aggregate economy; since intertemporal budget constraints always hold there can be no insolvencies; markets, if not always contracts, are complete. Lack of consideration of financial variables is complementary to the linear or linearized nature of these models, which, when shocked by real and monetary disturbances, produce relatively well behaved business cycles converging eventually to a unique equilibrium. All very neat, but of little or no use for understanding why a financial crisis may occur and how it unfolds. Defining those models “a costly waste of time” (Willem Buiter (2009)), which are “spectacularly useless at best, and positively harmful at worst” (Paul Krugman in his 2009 LSE Lectures) may be too harsh; but one does sympathize with Charles Goodhart, whom Buiter quotes as saying that the DSGE approach “excludes everything I am interested in”. Perhaps then the problem is not so much with cognitively captured economists reluctant to use the available theoretical tools, as with the fact that those tools (which however they constructed) are unsuitable to deal with financial phenomena.

#### 4.

Neglect of financial variables, far from being a specific feature of DSGE models, has characterized large part of modern macroeconomic modelling. Andrea Prat of LSE has recently reported that, when scanning a collection of four articles on whether there exists “a core of usable macroeconomics” in a 1997 issue (vol. 87, 4) of the American Economic Review, he was unable to find the words banks or insolvency,

while risk was mentioned once, but in a footnote. As noted by Wyplosz (2009), “most macroeconomists assumed that financial markets were just a side show, which could safely be taken as exogenous or described in a rudimentary way”. There are of course exceptions to this practice: one is Bernanke’s model of the financial accelerator (1999, 2007), which however accounts for the transmission of the effects of a credit cycle to the real economy more than being an explanation of the financial crisis itself and of its dynamics. In general it remains true that a developed financial sector cannot be found in the more readily usable versions of macro-modelling.

It is not easy to understand why this is so. In particular, one wonders why the consideration, or lack thereof, of a financial sector has played no role in the heated disputes between the new classical school and the neo-keynesians: though finance occupies a prominent place, not confined to the ‘beauty contest’ parable, in the vision of the world provided by the *General Theory*, the whole debate has been on rigidities in the goods and labour markets and on the role of policies in an environment where financial variables are absent or irrelevant. Possibly mainstream neo-keynesians (but not some at the fringe, like Hyman Minsky) shared the view, inspired by some version of the Modigliani-Miller theorem and even more by the efficient market hypothesis, that those variables matter little. The results of an excellent specialized literature where those assumptions are abandoned, like those of agency theory, asymmetric information and the like have never found their way into macro-models.

A concurrent explanation of this attitude, at least for the most recent period, may be the macroeconomic environment of the past two decades or so. In spite of recurrent but localized financial and currency crises, issues regarding financial markets fell in the shade cast by the Great Moderation: that exceptional period of world economic history characterized by sustained growth and low inflation, growing role of emerging economies in promoting world demand and world trade, generalized improvement in economic conditions, fall in the volatility of output and inflation, as well as of asset prices. There was a debate on the causes of the Great Moderation and on the role played by the new theoretically inspired model of monetary policy, but the feeling, *à la* Fukuyama, was that the end of economic instability marked “the end of (economic) history”: “the economy of the 1990’s suggested to [a new] generation of students that the business cycle was no longer of practical importance” (Mankiw (2006)) and inspired a “misplaced belief that the same central-bank policies that had reduced the volatility of inflation had magically...also reduced the volatility of

financial markets” (Eichengreen (2009)). Models and research programs naturally adapted to this environment, taking it for granted that there had been a permanent structural break in economic history.

A third plausible explanation is of a practical nature. Allowing not only for financial variables, but, more importantly, for heterogeneous agents, asymmetric information, leverage, banks’ balance sheets and so on in macro-models is more easily said than done. To be useful, models must be manageable and easy to handle; this however requires drastic omissions and simplifications, often, as in our case, at the expense of the models’ ability to capture relevant phenomena. (By way of example, the non-linearities in the behaviour of financial variables are incompatible with linear or linearized models.) This inherent contradiction is documented by the following two quotes that well reflect the prevailing mood *now*. Gorton (2008) writes of this crisis that

“...It will surely be some time before researchers can sort through the events.....the lessons to be learned are likely only going to be known when there is some distance from the events. But, since panics are rare, it may be that we never have the ability to formally test in the way that is acceptable to academic economists. The scholars who studied panics before us...described the events with narratives. Perhaps this is the best we can do ”

while it is Willem Buiter’s view (2009) that

“The Bank [of England] has by now shed the conventional wisdom of the typical macroeconomic training of the past few decades. In its place is an intellectual potpourri of factoids, partial theories, empirical regularities without firm theoretical foundations, hunches, intuitions and half-developed insights”. It is not much, he adds, but perhaps the beginning of wisdom.

Only few months before Michael Woodford (2009) had proudly stated in his address on Convergence in Macroeconomics (typified by DSGE models) at the 2008 AEA meeting that:

“The current moment is one in which prospects are unusually bright for the sort of progress that has lasting consequences, due to the increased possibility of productive dialogue between theory and empirical work, on the one hand, and between theory and practice, on the other.”

Clearly in January 2008 the feeling still was that the ongoing financial crisis was an event of relatively minor importance and one which in any case did not affect the current generation of macroeconomic models.

Does it matter very much outside the profession whether and why economists were unable to anticipate and understand the worst world financial and economic crisis since the great depression? More precisely, were the costs of their failure confined to a reputational damage for the profession or were there social costs as well, as would be the case if the economists' doctrines and attitudes played a part in creating an environment congenial to the eruption of a crisis?

It is debated if and to what extent macroeconomic theory has affected policy. While Woodford, as we have seen, believes in "the increased possibility of productive dialogue...between theory and practice", having especially in mind monetary policy, Mankiw holds that the new macroeconomics has had no influence on policy making. This dispute is however too narrow (also because of the difficulty of finding a place for Alan Greenspan in mainstream models of monetary policy). It is more interesting to look for different and less direct channels for the economists' influence on policy.

A hypothetical committee of inquiry into the economists' responsibilities would probably start from finance theory, which has been under attack on at least three counts (see Wyplosz (2009): not having provided the necessary warnings on the use of the ever more complex products of financial engineering; not having monitored the robustness of the risk assessment and pricing models with respect to the underlying empirical assumptions; having neglected a proper analysis of liquidity risks. But even leaving finance aside, what deserves consideration is the way in which economists have helped create an all pervading *Zeitgeist* that undeniably affected the actions and omissions of policymakers and regulators.

It is by now widely accepted that regulators failed in their jobs in several ways. Having ignored all symptoms of an unsustainable growth of credit and leverage, having assumed until the very eve of the crisis that credit risk was actually being transferred away from banks and that this would stabilize the system, having failed to detect the many ways in which the banks were taking that risk back, while worrying only about the hedge funds' counterparty risk, regulators were caught by the crisis with their eyes wide shut.

Alongside the omissions there were actions. Before the crisis, regulators and policy makers resisted any attempt to broaden the scope of regulation so as to keep pace with financial innovation: if anything they did the opposite. The examples are numerous: the proposal by the chair of the CFTC Brooksley Horn to introduce some regulation for the CDSs was defeated by the joint resistance of Messrs. Greenspan,

Levitt and Rubin; even after Enron, the financial industry successfully opposed a revision of accounting standards imposing more rigorous criteria for the consolidation of off-balance sheet entities; the 2000 Commodity Futures Modernization Act of 2000 relaxed regulatory standards; the SEC, in charge of investment banks, followed a permissive approach, lifting the ceiling to leverage; GSEs (Fannie and Freddie) captured their regulator and were given by Congress full freedom to engage into speculative investment.

Actions and omissions were sometimes the outcome of pathologies present in the system: revolving doors, currying favour with politicians to obtain more power, selective blindness as a result of regulatory capture (Simon (2009a)). More often, however, they were the consequence of the creeds followed by the authorities: that markets could operate their wonders irrespective of institutions; that there were proper incentives for rational and effective self-regulation; that it was in the interest of management to properly assess risk and to avoid excessive exposure; that public regulation should interfere as little as possible with the spontaneous working of markets and should therefore operate with the lightest possible touch, also in view of the competitive growth of the major financial centres; that capital deepening, as measured by the extent of recourse to external financing, would always and unqualifiedly be welfare enhancing, irrespective of whether it originated in the real sector of the economy or was only endogamous to the financial sector. These propositions, if unaccompanied by “ifs” and “buts”, were tainted by ideology. Did the economists bear any responsibility?

Acemoglu (2009) thinks that it was regulators who prevaricated on economists:

when “we mistakenly equated free markets with unregulated markets”, “in our obliviousness to the importance of market-supporting institutions we were in sync with policy makers”: “we let their policies and rhetoric set the agenda for our thinking about the world and, worse, perhaps, even for our policy advice”.

Acemoglu is not flattering to the profession when believing that it can be plagiarized so easily. I surmise instead that economists and economic theory had the initiative, when providing, perhaps unwittingly, the foundations on which the ideology was made to rest. Of course, (almost) no respectable economist would utter propositions like those reported above without warning that their validity depends upon many stringent conditions which rarely hold in real life. Sometimes however this caution is lost in the process of translating the outcome of rigorous research into a product

ready for immediate consumption. As in churches, while clerics are not bound by the simplistic orthodoxy of an elementary catechism and are allowed to express doubts and draw distinctions, the message to the congregation must be clear and unequivocal. Though economists would not provide simple truths in their research work, some were ready to distil them for popular use in op.eds and blogs and few in any case objected to the vulgate version which became popular with the congregation. They should have been suspicious, because theirs was a peculiar congregation, consisting not only of policymakers, central bankers and sundry regulators, but even more of private sector agents in search of arguments justifying financial deregulation for their own private interest. The consumption version of mainstream macro-models was ideal from this perspective: as argued above, there is nothing there suggesting that some regulation may be desirable. This also affected the literature on financial crises. As noted by Allen and Gale (2007), “In the 1930’s the market was the problem and government intervention through regulation...was the solution. Today...the view is that government is the cause of crises and not the solution. Market forces are the solution”, crises being an equilibrium outcome.

Of course, there was plenty of literature leading to different conclusions (including that establishing a positive relation between the quality and effectiveness of regulation and the growth of financial markets): but, to quote again Barry Eichengreen, “the consumers of economic theory...tended to pick and choose those elements of [a] rich literature that best supported their self-serving actions”.

Thus the answer to my initial questions in this section is that economists do indeed bear some responsibility for what has happened, as their doctrines, at least in their vulgate version, often provided an intellectual justification to the unconstrained behaviour of the private sector and to the negligence of regulators.

Also for this reason, it is now time to think afresh. Greater humility, rather than an implausible defence of past rent positions, is necessary to provide new impetus to the discipline. It is to be hoped that the younger and the brighter understand that there has been a problem – otherwise we would not have seen Hyman Minsky become a popular character in the letters of private sector sell-side analysts to their clients.

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