GLOBAL MACRO ECONOMONITOR

Rethinking (Macro)Economics

Roberto Tamborini | Apr 18, 2011 11:08AM

Bashing economists has become a fashionable sport today. Healthily, self-criticism and rethinking also come from inside the profession. RGE has recently posted three authoritative reflections, by Olivier Blanchard, Joe Stiglitz and Michael Spence, about the state of the discipline taking stock of the hard lesson imparted by the crisis in the face of the policy challenges ahead. This welcomed attitude should not remain a moment's penance in the wait for the next upswing in the business cycle that will push the sorrow days of the crisis far back in the memory. A wide-ranging and long-lasting correction is needed of major faults in the way in which economics has been thought, taught and practiced for some decades.

The economic profession's responsibilities in the face of the crisis are not limited to the fact that the so-called "mainstream" theories were not good enough for policy advice, and should be corrected here and there. They concern two deeper related levels and tasks:

- revise critically the *foundations* of the existing body of knowledge
- rethink the deontoloy of the profession with respect to society and, even more importantly, with respect to teaching to new generations.

Let me add some considerations on each point.

1. **Foundations.** H. M. the Queen of England, visiting the London School of Economics in 2008, wondered how was it possible that the crisis was not foreseen and policy-makers were not forewarned. Yet this is the most apparent symptom, not the cause of the disease of the discipline. And the disease is not just a matter of forecast techniques. The true disease to be attacked is a genetic one.

Professor Stiglitz (here and elsewhere) points out that "*the attempt to incorporate micro-foundations [into macroeconomic models] was laudable; it was important that they be the right micro-foundations*". Today's "mainstream" methodological framework, the so-called dynamic stochastic general equilibrium (DSGE) model, is a representation of a *simple* (i.e. not *complex*) economic system animated by perfectly informed, best forecasting, optimizing "representative agent(s)". The usual argument in favour of rooting macro-theory in *this* micro-theory is that this makes the macro-level responses to policy interventions fully controllable. The result is an artifact that can be regulated by means of "optimal (stochastic) control" (OSC) instruments, promising to transform policy-making from art into science (Clarida *et al*., 1999). OSC comes from engineering and is based on the presumption, and conveys the idea, that the system's behaviour can be fully known and controlled up to "user-friendly" random shocks. Think of the control system that assists a missile launch unit, the pilot of the Shuttle or the driver of a Formula 1 car (which, by the way, sometimes do go out of control).

Practitioners of the DSGE-OSC apparatus entertain an intermittent attitude towards the issue of prediction and control. On the one hand, most subscribe to the popular version of Friedman's instrumentalism (whatever model is good insofar as it gives good predictions of the data, net of normally distributed errors, of course). Nowadays, as a response to the allegation of not being able to predict the crisis, the argument often heard is that these models never promised accurate predictions of economic crises. In fact, these are the "exogenous shocks" part of the story, and as such they are removed out of the reach of scientific explanation (Lucas, 2009). Another oft-heard disclaimer is that seismologists cannot predict earthquakes' occurrence with precision, which does not make

seismology a useless science. The same could be said for meteorologists and long-term weather forecasts. These *boutades* beg the key point. Seismologists, meteorologists, etc., do have a "mainstream" theory of earthquakes and hurricanes, and they do know why they cannot predict and control these phenomena accurately: because nobody can know and control all the conditions of the underlying *complex dynamic processes*. However, reportable improvements in short-term weather forecasts, or in geo-dynamic simulations, testify that hard work on these methods and instruments does pay off. By contrast, "mainstream" economics does not even have a theory of economic disasters. These are simply not existent in a system which is, by construction, always in equilibrium (for the layman: nobody ever makes forecast errors that are so large and one-sided that they can generate bankruptcy or bankruptcy chains). Let me only add a telling quotation from one of the architects of modern macroeconomic theory, namley Robert J. Lucas:

"*The problem is that the new theories, the theories embedded in general equilibrium dynamics […] don't let us think about the US experience in the 1930s or about financial crises and their consequences […] We may be disillusioned with the Keynesian apparatus for thinking about these things, but it doesn't mean that this replacement apparatus can do it either*" Lucas (2004, p. 23).

In the 1990s a string of these models with some "financial frictions" were produced, notably by the present Chairman of the Federal Reserve. Even those were eventually removed from what then became the workhorse model for macro-policy, the "New Keynesian" DSGE model where the only "friction" is sticky good prices. Replugging "financial frictions" into this machinery will not overcome its intrinsic limits. The DSGE-OSC technique, like all techniques, implies a "view of world"; and it no longer works, actually it is dangerously misleading, if the economic system happens to be closer to a complex dynamical system than to a controllable human artifact. To appreciate the implications of this paradigmatic mis-fitness, I only wish to recall one of the earliest and most authoritative critics of the advent of the OSC approach to economic policy, Friedrich von Hayek. Hayek was one of the path-breaking thinkers in the theory of complex systems, first in his neuro-physiology studies and then in economics. His Nobel Memorial Lecture (1974) - just to mention one among his many influential writings - was entitled "The Pretence of Knowledge" and contained an outright rejection of the OSC approach precisely on the grounds of the complexity paradigm.

"*The failure of the economists to guide policy more successfully is closely connected with their propensity to imitate as closely as possible the procedures of the brilliantly successful physical sciences* […] *Unlike the position that exists in the physical sciences, in economics and other disciplines that deal with essentially complex phenomena, the aspects of the events to be accounted for about which we can get quantitative data are necessarily limited and may not include the important ones* […] *Complexity here means that the character of the structures showing it depends not only on the properties of the individual elements of which they are composed, and the relative frequency with which they occur, but on the manner in which the individual elements are connected with each other.* […] *A theory of essentially complex phenomena must refer to a large number of particular facts; and to derive a prediction from it, or to test it, we have to ascertain all these particular facts* […] *The real difficulty, to the solution of which science has little to contribute* […] *consists in the ascertainment of the particular facts*" (Hayek, 1974, pp. 3-4, 6-7)

I do not mean that the "mainstream" apparatus is entirely useless. It is elegant, it is manageable, it may be thought of as an ingenious bypass of the limits indicated by Hayek. Maybe we get good flight instructions for fine weather, and maybe this is better than nothing. But then we should stop pretending too much of this apparatus, in particular that it can transform economic policy into science for all seasons. Economists should eventually come to terms with the hard fact pointed out by Hayek and corroborated by subsequent theoretical and empirical research, namely that the deeper we penetrate into the micro-structure, the more we find the shifting sands of heterogeneity, bounded rationality, and all sorts of behavioural vagaries. Delving into individual behaviour and microfoundations has turned

out to be very much like a one-way journey with no return ticket towards the meso- or macro-surface. Thus, the "rigorous microfoundations" claimed by the "mainstream" now appear not to be serious scientifically, whereas the serious microfoundations discovered by scientific investigation of human behaviour are hardly susceptible to *simple* aggregative procedures.

2. **Deontology.** There must be something special in thinking about disasters that repels "mainstream" economists while so much attracts seismologists, meteorologists and many other natural scientists (I suspect that ideology may play some role). A strategy to save the profession's reputation that I find particularly disturbing is: *embrassons nous*! Everyone is now eager to give credit to the scholars who have contributed to the study of micro market failures up to macro systemic failures, or to the few of them who did forewarn that some disruptive forces were mounting under the dazzling surface of the Great Moderation. Some of these scholars and ideas were even crowned with a Nobel Prize! True, this is evidence that the discipline as a whole cannot be blamed. However, the fact remains, and remains to be explained, that these ideas did not penetrate into (better: at some point they were actively espunged from) the inner circles that dictated the agenda of macroeconomic research, the requirements for academic careers, and the recipes to be cooked for policy-makers. "Economics" is not a homogeneous entity. "Economists" are not a homogenous class either. Economists and schools of thought bear different responsibilities in the face of the crisis.

When I try to understand why the large majority of macroeconomists deliberately go on building models that are unable to reproduce economic crises in any meaningful sense of the word, or why exploration of new theories and techniques in that direction is banned from leading journals and discouraged among young research trainees, I can only imagine two answers: inertial "normal research" (in Kuhn's and Lakatos's sense), and ideology (in Schumpeter's sense). They may reinforce each other as they eventually protect the free market ideal from even the remotest handle offered to marxists, radicals, pro-government high-tax high-spending invaders, and professional doomsayers of all faiths. In so doing, however, the discipline has been pushed to the opposite extreme of an almost empty Panglossian fiction for the delight of precisely those "obscure forces".

The reform of the economic profession and deontology should be profound. Economic education and research training, the journal system and the career selection mechanisms should recreate appettite for risk-taking in the search of new ideas instead of rewarding conformistic, riskless "normal research". Fear of type-II errors (rejecting a good idea/paper) should rebalance the current obsession with type-I errors (accepting a bad idea/paper). The self-constructed image of the modern economist as social engineer (Mankiw, 2006) should be left behind. Predictions and prescriptions of those who study, and to a greater extent of those who live in, a complex system are necessarily *conditional*, conditional on available knowledge of the topology of the structure -which is necessarily incomplete- on the control of initial conditions -which may be very limited- on the reproducibility of "experiments" -which cannot be taken for granted. Hence economists should also be ready to accompany their recipes with a clear statement of their limits and potential damage if mistaken, as one can find in medicine dosage instructions. On the other hand, will institutional and political authorities be willing to pay pecuniary and non-pecuniary rewards to technicians falling short of the technical certainties of an engineer?

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