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Economic Coordination and Dynamics: Some Elements of an Alternative "Evolutionary" Paradigm

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Economic Coordination and Dynamics: Some Elements of an Alternative "Evolutionary" Paradigm

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Abstract

The paper, largely based on the introduction to Dosi (2012), elaborates on the main interpretative ingredients, methodology and challenges ahead of the evolutionary research program in economics.

Telegraphically, such a perspective attempts to understand a wide set of economic phenomena – ranging from microeconomic behaviours to the features of industrial structures and dynamics, all the way to the properties of aggregate growth and development - as outcomes of far-from-equilibrium interactions among heterogeneous agents, characterized by endogenous preferences, most often "boundedly rational" but always capable of learning, adapting and innovating with respect to their understandings of the world in which they operate, the technologies they master, their organizational forms and their behavioural repertoires.

And on methodological grounds, far from disdaining formal modelling and statistical analysis, the research program is, however, largely *inductive*, taking very seriously indeed empirical regularities at all levels of observation as discipline for the modelling assumptions.

Together, the paper places such interpretative perspective against some fundamental questions addressed by the economic discipline in general and against the answers to such questions that contemporary theory has to offer. Such questions fundamentally concern first, the drivers of dynamics and, second, the conditions of coordination among interacting agents.

Keywords

Economic Evolution, Coordination, Dynamics, Bounded Rationality, Agent-based Macro Models, Innovation

Coordination and change, or, where Walras, Schumpeter, and Samuelson got it at least partly wrong and Adam Smith, Marx, and Keynes got it fundamentally right

Indeed, one way to look at at any research program is by reference to the basic questions it addresses and the ways it does it.

In my view *the* two basic questions at the core of the whole economic discipline since its inception regard, first, *the drivers and patterns of change of the capitalistic machine of production and innovation* and, second, the *mechanisms of (imperfect) coordination among a multitude of self-seeking economic agents* often characterized by conflicting interests[1].

Of course, of crucial importance are the answers which diverse theories offer to the two questions, but equally important are the relations purported by each theory between the two questions themselves.

Interestingly, Adam Smith begins his *Wealth of Nations* with a detailed analysis of the drivers of change – in particular the positive feedback between division of labour, mechanisation, productivity growth and demand growth -. Conversely, issues of coordination are discussed much later, building on such a dynamic background: in fact, apologists of unbridled free-market eager to recruit Adam Smith among their supporters, are not generally aware of the fact that A. Smith talks of the (in)famous "invisible hand" much later, *only once*, in his whole work (Book IV, chapter 2, of the *Wealth of Nations*).

Somewhat similarly, Karl Marx builds upon a long discussion on the relationships between a theory of production and labour relations – centred around the theory of value, capital accumulation and technological progress -. "Coordination", if we can call it that way (or, more rudimentary, a theory of sectoral relative prices and their dynamics) comes much later, taking of course for granted the intrinsic dynamic nature of capitalists' interactions.

From a quite different angle, Keynes too never dreamt of separating "what keeps the system together" from "what keeps it going": in fact, the properties of shorter term coordination – as revealed prominently by involuntary unemployment – were derived from the properties of capital accumulation and the "animal spirits" driving it.

In fact, the current dominant theoretical creed is very much on the analytical opposite. It builds on the separation between "coordination" and "dynamics". In fact, notwithstanding the fundamental Schumpeterian contribution to the understanding of technological innovation as the driver of long-term change, even Schumpeter turned out to subscribe to this "separation epistemology", building as a point of departure not on Adam Smith and other classics but upon the Walrasian approach to coordination.

[1] We already raised this issue in Dosi and Orsenigo (1988) to which we still refer for more details.

Many readers are well aware of the basic Schumpeterian story, at least of "Schumpeter Mark I", vintage 1912. One starts with a *Walrasian* state of (equilibrium) *circular flow*. Nowadays we would say that one begins with a *general equilibrium*, of course grounded on well specified *fundamentals* in terms of technologies, endowment and preferences. Then comes a "shock": in the Schumpeterian story, the entrepreneurial innovator introduces an *unexpected* innovation, in turn yielding disequilibrium profits for the innovator himself (yes, most likely for Schumpeter, a *him*self!), changing relative prices, "creative destruction", etc. Thereafter, the economic system adapts via technological imitation and diffusion of the innovations. This is the "transient", until the system converges to a new (equilibrium) *circular flow* characterized by a new ensemble of fundamentals of the economy.

In the original work of Schumpeter there are of course many important historical qualifications and nuances. However, the story is in principle in tune with the young Samuelson's formalization of the idea in terms of the *correspondence principle* (in fact Paul Samuelson was a PhD student of his).

If S(0) is the vector of *equilibrium* state variables of the system at time zero and S(1) stands for the values of the same variable in the new equilibrium state of the "circular flow", why not comparing S(1) and S(0) as the most elegant an parsimonious way of doing "dynamics"? Naturally, one may think of a whole sequence of such equilibrium states. And, here we are, we have an *equilibrium theory of growth nested upon a narrative of (transient) creative destructions*, temporary disequilibria, etc[2].

Indeed, this is more or less one of the three tenets of the intellectual compromise on which the economic discipline has run for long time after WW II, with a rough division of labour between (i) "microfounded (GE) models; (ii) "short run" macroeconomics; and (iii) growth theories.

The "coordination research program", as known, soon culminated into the Arrow-Debreu-McKenzie *General Equilibrium* model, indeed an elegant and institutionally very parsimonious demonstration of the *possibility* of equilibrium coordination amongst decentralized agents.

In fact, subsequent, basically *negative*, results have shown the general impossibility of moving from *existence theorems* to that sort of "implicit dynamics" captured by proofs of global or local stability - loosely speaking, the ability of the system, when "scrambled", to get back to its equilibrium state. Quite the contrary, even empirically far-fetched processes such as *tâtonnements* (with the omniscient Walrasian auctioneer proclaiming equilibrium transaction when he sees them) in general do not converge.

Even more powerfully, some of the founding fathers of GE themselves have shown that "existence" does not bear any implication in terms of the shape of excess demand functions (this is what the Sonnenschein-Mantel-Debreu theorem implies). Putting it shortly, in general *forget even local stability*!

Conversely, any careful look at the toll requirements which *sheer existence* entails – in terms of information and rationality – highlights the extent to which GE is a beautiful but extremely fragile creature, certainly unable to withhold the weight of any account of the dynamics of the economy as a whole

[2] To be fair, Schumpeter especially for the '30s onward, grew increasingly unhappy of such separation between "shocks" and subsequent adjustments to equilibrium: witness its reluctance to accept the related distinction in the emerging econometrics field between (exogenous) impulses and subsequent propagation, suggested by his friend Ragnar Frisch. (Thanks to Francisco Louça for pointing it out to me: much more on the point in Louça, 2001).

and even less so to offer any serious microfoundation to transforming economies undergoing various forms of innovation.

In fact, – even forgetting search, innovation and all that – it is quite ill-founded to claim that standard GE models can be an account, no matter how utterly stylized, paraphrasing Adam Smith, of "why the butcher offers meat day after day more or less at the same price" mainly motivated by self-interest. If the conditions – in term of rationality, characteristics of the exchanges, etc. - required in reality were even vaguely as stringent as those required in GE models, probably no one would ever offer meat or whatever else !

[Concerning all the foregoing points, my friend Alan Kirman has insightfully discussed in various works the achievements, limitations and dead ends of GE analysis. Kirman (2010) offers an overall assessment together with alternative proposals on how to tackle economic coordination. See also Stiglitz (2011) and below.]

In any case, that was the "micro".

Then there were basically two "macros". One were (equilibrium) growth theories which largely lived until the end of the '70s a life of their own. While it is the case that e.g. models à la Solow invoked maximizing behaviours in order to establish equilibrium input intensities, no claim was made that such allocations were the work of any "representative agent" in turn taken to be the "synthetic" (??) version of some underlying General Equilibrium[3]. By the same token, the distinction between *positive* (that is purportedly descriptive) and *normative* models before Lucas and companions was clear to the practitioners. Finally, in the good and in the bad, technological change was kept separate from the mechanisms of resource allocation: the famous "Solow residual" was, as well known, the statistical counterpart of the drift in growth models with an exogenous technological change.

Together, in some land between purported GE "microfoundations" and equilibrium growth theories, lived for at least three decades a macroeconomics sufficiently "Keynesian" in spirit and quite neoclassical in terms of tools. It was the early "neo-Keynesianism" – pioneered by Hicks, and shortly thereafter by Modigliani, Patinkin and a few other American "Keynesians" – which Joan Robinson contemptuously defined as "bastard Keynesians". It is the *short-term* macro which students used to learn up to the '80s, with IS-LM curves – meant to capture the aggregate relations between money supply and money demand, interest rates, savings and investments -, Phillips curves on the labour market, and a few other curves. In fact, the "curves" were (are) a precarious compromise between the notion that the economy is supposed to be some sort of equilibrium – albeit of a short-term nature – and the notion of a more "fundamental" equilibrium or equilibrium path to which the economy is bound to tend in the longer run. Needless to say, forget any formal demonstration of the proposition, for example in terms of "slow" and "fast" variables: hand-waving in economics is one the most robust conventions!

IS-LM curves and the like, I straightforwardly admit, were a major pain in my early studies and still are. The quick "Keynesian synthesis" presented by Hicks (1939) and Modigliani (1944) had been offered as a seemingly sensible and parsimonious account of Keynes' *General Theory* – cutting out all the detours and qualifications. In fact they were the most rudimentary "general equilibrium" translations with an implicit representative agent and various sort

[3] For a devastating critique of the notion of "representative agent", cf. Kirman (1989) and (1992)

of "frictions" added up. (However it took almost half a century for the American macro mainstream to further sterilize, re-formulate, refine it and baptise the monster "Stochastic Dynamic General Equilibrium"; see below).

"New Classic (??)" Talibanism and beyond

What happened next?

Well, in my view, everything which could get worse got worse and more. (For a much more detailed reconstruction, which I largely share, of what happened to the theory, intertwined with the reconstruction of the actual policy dynamics which led to the 2008 crisis, see Cassidy, 2009).

First, "new classic economics" (even if the reference to the classics cannot be more far away from the truth) fully abolished the distinction between the normative and positive (i.e. descriptive) domains – between models *à la* Ramsey *vs.* models *à la* Harrod, Domar, Solow, etc. (notwithstanding the differences amongst the latter ones).

In fact, the striking paradox for theorists who are in good part market talibans is that one starts with a model which is essentially of a benign, forward-looking, *central planner*, and only at the end, by way, again, of an abundant dose of hand-waving, one claims that the solution of whatever intertemporal optimization problem is in fact supported by a decentralized market equilibrium.

I have already mentioned it: things could be much easier for this approach if one could legitimately "summarize" a genuine "general equilibrium" (that is with many agents, heterogeneous at least in their endowments and preferences) into some "representative agent". But the fact is that *one cannot*. By doing that nonetheless, one simply *assumes away as solved by construction the coordination problem*. Notwithstanding the name, there is very little of "general equilibrium" in the Dynamic Stochastic General Equilibrium (DSGE) models, and earlier antecedents (all that irrespectively of the trust in the ability of GE to capture the essentials of the coordination hurdle in market economies, which in my case is very low indeed).

Second, but relatedly, the last three decades has seen the disappearance of the distinction between "long-term" and "short-term" - with the latter as the locus where all "frictions", "liquidity traps", Phillips curves, some (temporary!) real effects of fiscal and monetary policies, etc. would all hazardously survive. Why would a representative agent able to solve sophisticated inter-temporal optimization problems from here to infinity display frictions and distortions in the short-run ? We all know the outrageously silly propositions, sold as major discoveries, associated with infamous "rational expectation revolution" concerning the ineffectiveness of fiscal and monetary policies and the general properties of markets to yield Pareto first-best allocations [In this respect, of course, it is easier for that to happen if "the market" is the representative agent: coordination failures and allocation failures would involve serious episodes of schizophrenia by that agent itself!][4].

[4] To be precise, "rational expectations" alone are not sufficient to guarantee "neutrality" of monetary and fiscal policies, but one looses the straightforward Keynesian relations such as these implied by IS-LM curves and the like.

Personally, I believe that in other times, nearly the entire profession would have reacted to such a "revolution" as Bob Solow once did when asked why he did not take the "new classics" seriously. Let me extensively quote him - one of the great few, with a great mind and a great intellectually integrity. When interviewed early on about the supposed "new classics", Bob replied

"Suppose someone sits down where you are sitting right now and announces to me he is Napoleon Bonaparte. The last thing I want to do with him is to get involved in a technical discussion of cavalry tactics at the battle of Austerlitz. If I do that, I am tacitly drawn in the game that he is Napoleon. Now, Bob Lucas and Tom Sargent like nothing better than to get drawn in technical discussions, because then you have tacitly gone along with their fundamental assumptions; your attention is attracted away from the basic weakness of the whole story. Since I find that fundamental framework ludicrous, I respond by treating it as ludicrous – that is, by laughing at it- so as not to fall in the trap of taking it seriously and passing on matters of technique" (Solow in Klamer, 1983, p. 146)

The reasons why the profession, and even worse, the world at large took these "Napoleons" seriously, I think, have basically to do with a *Zeitgeist* where the hegemonic politics was that epitomized by Ronald Reagan and Margaret Thatcher, and their system of beliefs on the "magic of the market place" *et similia*. And, crucially, it was a *Zeitgeist* which was largely politically bipartisan. Compare the subsequent Blair and Clinton administrations who sometimes did things which even the former would not dare considering - including, among many reckless measures, the abolition of the Glass-Steagall Act, a measure which contributed to fuel the greatest crisis over the last 80 years.

Or think of the disasters produced for decades around the world by the IMF, inspired by the so-called *Washington Consensus* – as such another creed on the magic of markets, the evil of governments and the miraculous effects of blood, sweat and tears.

The point I want to make is that the changes in the hegemonic (macro) theory should be primarily interpreted in terms of the political economy of power relations among social and political groups, with little to write home about "advancements" in the theory itself … On the contrary !

I must end this sub-section with a cautionary *caveat*. I often hear the objection to the foregoing telegraphic, inevitably rough, account, that new macro theory, in the form of the newest generation of DSGE models, takes on board various forms of "imperfections", "frictions", "inertias" (cf. Blanchard, 2009, and Woodford,2009, for bold claims on the DSGE – "New Keynesian Synthesis"). True, we are now in the late-Ptolemaic phase of the theory: add epicycles at full steam without any empirical discipline and you will get some greater possibilities of *calibration* of the model ("calibration" is the new game in town, often not too short of voodoo: see also below[5]). Of course, in the epicycles frenzy one is never touched by the sense of ridiculous in assuming that the mythical representative agent at the same time is extremely sophisticated when thinking about future allocations but falls into backward looking habits when deciding about consumption or, when having to change prices, is tangled by "menu costs"! (Caballero, 2011, offers a thorough picture of this surreal state of affairs).

[5] Strictly speaking, DSGE are typically estimated through a Bayesian procedure, which actually boils down to a calibration of the parameters over intervals

What about innovation dynamics?

I have argued that even the coordination issue has been written out of the agenda by *assuming* it as basically solved by construction. But what about change? What about the *Unbound Prometheus*[6] of capitalist search, discovery and indeed destruction?

Very simply, in the DSGE workhorse, there is no Prometheus: "innovations" come as exogenous shocks upon the aggregate production function, with the same mythical agent (or in recent more sophisticated versions the representative household (?!)) optimally adjusting its consumption and investment plans . End of the story.

However, the last 30 years, have seen also the emergence of *new growth theories,* bringing – as compared to the original Solow model - some significant advancements and, in my view, equally significant drawbacks.

The big plus is the endogenization of technological change: innovation is endogenized into economic dynamics as either a learning externality or as the outcome of purposeful expensive efforts by profit maximising agents. However, in the latter case the endogenization comes at what I consider the major price (although many others would deem it as a major achievement) of reducing innovative activities to an *equilibrium* outcome of optimal intertemporal allocation of resources. Hence by doing that, one loses also the genuine Schumpeterian notion of innovation as a disequilibrium phenomenon – *at least as a transient*. In fact, putting it another way, innovative activities undertaken by private actors are ultimately reduced to yet another instance of optimal intertemporal resource allocation, with or without (probabilizable) uncertainty.

Let me be a bit hard on my friend Philippe Aghion and colleagues: certainly they masterly endogenized innovation, but was it necessary to do that by squeezing Mandeville, Smith, Marx, Schumpeter – that is dynamics - into Lionel Robbins ("...economics as the science of allocation of scarce resources to alternative uses ...")? If anything, innovation and knowledge accumulation are precisely the domains where the dismal principles of scarcity and conservation are massively violated: one can systematically get more out of less, while dynamic increasing returns are the general rule.

... And, incidentally, there was, and is, a major crisis ...

All the foregoing discussion takes the bird-eye view of the theory. But, as someone might remember, there has been a major financial and real crisis, and that crisis continues to be largely there at the time of the writing of this essay and will remain there for quite a while in the foreseeable future.

Indeed its very arrival and its sheer size are as near as one can get in social sciences to a falsifying "crucial experiment": as the "Dahlem Manifesto" puts it the crisis highlights a *systemic failure of the economic profession* (Colander et al., 2009[7])

Of course one cannot demand economists to predict precise dates or modes of occurrence of any crisis, but what is astonishing is that the mainstream paradigm briefly sketched out above is unable to allow the *very possibility* of a crisis .

I cite again from the "Manifesto":

[6] I borrow the term from Landes (1969)

[7] Partly overlapping considerations about the current state of macroeconomics are in Stiglitz (2011).

The implicit view behind standard models is that markets and economies are inherently stable and that they only temporarily get off track. The majority of economists thus failed to warn policy makers about the threatening system crisis and ignored the work of those who did. … The confinement of macroeconomics to models of stable states that are perturbed by limited external shocks and that neglect the intrinsic recurrent boom-and-bust dynamics of our economic system is remarkable. … The failure [of the economic discipline] has deep methodological roots. The often heard definition of economics – that it concerned with the 'allocation of resources' – is short-sighted and misleading. It reduces economics to the study of optimal decisions in well-specified choice problems. Such research generally loses track of the inherent dynamics of economic systems and the instability that accompanies its complex dynamics. (Colander et al., 2009, pp. 2-3)

Can the mainstream paradigm be saved by appropriate modifications?

I do not think it can, precisely because its massive interpretative failure is connected to its core building blocks(forward looking rationality, equilibrium, etc.).

The alternative interpretative venue: the economy as a complex evolving system

I take indeed some pride in having worked at an alternative research program well before the crisis itself (cf. also Dosi, 2000, and its introduction), shared with the broad community of evolutionary economists – at least I hope we do! –, which stands in most respects *at the opposite* to the state-of-the-art sketched above.

Indeed it starts by acknowledging that the object of study is the *economy as a complex evolving system* (this is also the name of a series of conferences and books sponsored by the Santa Fe Institute, rich of interesting insights, even if what was subsequently delivered was somewhat less than the promises: cf. Anderson, Arrow and Pines, 1988; and Arthur, Durlauf and Lane, 1997) .

Notice that here I want to start with the most minimalist notion of "complexity", capturing at the very least the fact that the economy is composed by multiple interacting actors – hence the illegitimacy of its "antropomorphization" -. (On "complex dynamics" see Kirman, 2010, near the spirit of these notes, indeed an important source of inspiration, and Rosser, 2011).

Moreover "evolution" entails that any assumption of "given the fundamentals" (including technologies and preferences) in most circumstances implies a significant rape to the object of study.

Of course in the analysis of a complex evolving economy one has to go well beyond the Schumpeter/Samuelson separation between coordination, on the one hand, and change,on the other. The (imperfect) coordinating features of the system are fundamentally shaped by its evolving nature. This is what I jokingly call the "bicycle theorem". It is easier to stand up on a bicycle when you cycle, while only a few *virtuosos* are able not to fall while standing still. That is, out of the metaphor, the relatively orderly properties (which are there often but not always !) of capitalist economies derive from its being in motion . This is the relative order of "restless capitalism", as Stan Metcalfe put it (cf. Metcalfe 1998, and Metcalfe and Ramlogan, 2006).

So for example, prices move roughly in line with the average costs of production which in turn depend on the underlying (technology-specific and sector-specific) rates of process innovation.

Demand patterns are shaped by the ensuing prices and, possibly even more importantly by the "trajectories" in product innovation.

Gross and net labour demand are affected by the double nature of technical progress as a "labour saver" and as a "demand creator".

Among many others, these are all features of *imperfect coordination* and *relative order* in the distributional properties of whatever statistics on economic variables, stemming precisely from the fact that the system is changing all the time in its process and product technologies, consumption patterns, organizational forms .

In fact the evolutionary paradigm, at least the way I see it, precisely addresses the properties of such endogenously changing multi-agent systems . Let me spell out a few general features of such interpretation.

Methodology

"*Dynamics first!*"

The emphasis on dynamics and change is indeed quite in tune with a more general methodological prescription common to the whole evolutionary research program, which my friend and mentor Sid Winter sums up in the imperative *dynamics first!* .

Such methodological imperative demands that the explanation for why something exists, or why a variable takes the value it does, ought to rest on a process account of how it became what it is. Loosely speaking, that amounts to the theoretical imperative: provide the process story either by formally writing down some dynamical system, or telling a good qualitative historical reconstruction (or, when possible, *both*).

Putting it in terms of negative prescriptions: be extremely wary of any interpretation of what is observed that runs just in terms of ex-post equilibrium rationalizations ("it has to be like that, given rationality").

By the same token, notwithstanding a very widespread practice in the economic profession, never take as a good "explanation" either an existence theorem or a purely functionalist claim (entity *x* exists because it performs function *y* …).

Moreover, note, in this perspective, Milton Friedman's old "as … if" interpretation of the properties of equilibrium behaviours (Friedman, 1953) should be taken as a (daring, and indeed most often wrong!) conjecture on the limit properties of some unspecified dynamics; and so should be notions such as that of evolutionary stable strategies (ESS) as originally put forward in biology by John Maynard Smith (1976), (although the dynamical intuitions are more understandable in the biological rather than in the social case).

In fact, only under very special circumstances observed phenomena can be interpreted as the outcome of behaviours "as … if" the latter were maximizing ones, on the assumption that those which were not had been driven away by some selective process. Indeed, in any serious scientific discipline, the reading of Winter (1964) critique would have put an end to any "as..if .." claims. However, our poor quasi- theological discipline is deaf to both theoretical

argumentation and empirical evidence when it comes to defend its epistemological core …

Realism

Realism is a virtue and in certain respects a necessity.

Theories are necessarily abstract and admit less of reality than they omit: indeed "the map is never the territory " as Kay, 2011 puts it . At the same time, there are some broad features of reality that the are omitted at the theorist's peril – in the sense that the conclusions are unreliable guides to the interpretation of reality, though perhaps instructive regarding important mechanisms or otherwise useful. [The broadest point that I cannot pursue here as much as I would like to is that, in a word, the "prediction-centred" justification of running modelling practices - "it does not matter the assumption you make, what counts is the quality of your prediction" - is basically epistemological trash…]

Some substantive building blocks

Given these general epistemological prescriptions (admittedly not obvious ones or even generally accepted amongst economists), the following substantive building blocks give shape to a full-fledged evolutionary research program (much more on the substantive part of evolutionary research program is in Nelson and Winter, 1982; Dosi et al.,1988; Metcalfe, 1998; Dopfer, 2005; Dosi and Nelson, 1994, Coriat and Dosi, 1998,, Dosi and Winter, 2002, upon which I partly draw in this section; and the introduction to Dosi, 2000).

Microfoundations

Theories ought to be *micro-founded*, in the sense that they ought to be grounded explicitly (though perhaps indirectly) in a plausible account of what agents do and why they do it[8]. (Note that the proposition does *not* imply, however, that agents' objectives are in general achieved or their expectations fulfilled. In other words, only a massive misunderstanding has "microfoundations" equivalent to rational expectations).

"Bounded rationality", broadest sense

Among the fundamental micro features is the fact that agents have at best imperfect understanding of the environment they live in, and, even more so, of what the future will deliver. I must say in this respect that I rather dislike the word "bounded rationality" as it seems to hint at a full "Olympic" rationality whose distance from actual behaviours measures also how much "bounded" is "bounded". On the contrary, it happens that in changing complex environments, such an Olympic "perfect" rationality might not be definable even in principle. We discuss the issue at length in Dosi, Marengo and Fagiolo (2005).

[8] Note, however, that quite a few 'aggregate' (i.e. non-microfounded) dynamic models are nonetheless consistent with an evolutionary interpretations (some of them are surveyed in Silverberg and Verspagen, 2005; and in Coriat and Dosi, 1998). The point is also discussed in the introduction to Dosi (2000).

There, we adopt a very expansive notion of "bounded rationality", related most obviously to limitations in (i) access to information; (ii) memory; and (iii) computational abilities; but also more fundamentally to (iv) intrinsically imperfect *representations* of the environment in which agents operate; (v) ubiquitous limitations in the agents' abilities to master physical and "social" technologies[9]; and (vi) fuzziness, possibly incoherence, and instability in very perception of one's own preferences.

Heterogeneity

Straightforwardly, imperfect understanding and imperfect, path-dependent, learning entails persistent heterogeneity among agents. Of course agents are heterogeneous in (i) their preferences and endowments – a property well acknowledged also by standard models in their full General Equilibrium version (but hardly so by most current macro models!). However, they are heterogeneous also with respect to (ii) the *models-of-the-world* they hold, even when facing identical information; (iii) their technological repertoires; and (iv) (possibly) their learning processes (in fact we still know very little on learning patterns, both at the levels of individuals and, even more so, of organizations).

Capturing heterogeneity is crucial to the representation of aggregate dynamics: to repeat,the lack of it contributes significantly to the pitiful state of contemporary macroeconomics.

Persistent innovative opportunities

The knowledge margin is always active: agents are always capable of discovering new technologies, new ways of organizing, and new behavioural patterns. Allowing for the *immanent possibility of novelty* in the system is a major theoretical and modelling challenge that cannot safely be ignored. In this respect, evolutionary-inspired students of technological and organizational change have contributed to open up a whole new field of analysis addressing the structure and dynamics of technological knowledge: we review the state-of-the-art in Dosi and Nelson (2010).

Interactions, coordination and selection

While (imperfect) adaptation and persistent discovery generate variety, collective interactions within and outside markets operate, first, as mechanisms of *information exchange and coordination*, and second, as *selection mechanisms*, generating differential growth (and possibly also survival) of different entities that are the 'carriers' of diverse technologies, routines, strategies, etc. Indeed, crucial issues here regard (i) the coordinating power of whatever "invisible (or visible) hand" of decentralized interactions; (ii) the drivers, powers and efficiency of selection mechanisms and (iii) the interactions between the foregoing two processes. [Obviously, under a "dynamic first" rule, demonstrations of existence of a purported equilibrium, followed, again, by some "hand-waving theorem" based on pub-like anecdotes and assertions such as "the system must get there after all" do not count as serious points …]

[9] On the latter notion related to nature of institutions and behaviours therein cf. Nelson and Sampat (2001).

Aggregate regularities as emergent properties

As a result of all this, collective aggregate phenomena (e.g. regularities at different levels of aggregation, in the growth processes, in industrial structures and dynamics, etc.) ought to be generally captured theoretically as *emergent properties*. the collective and largely unintentional outcome of *far-from-equilibrium micro interactions* and heterogeneous learning.

Putting it another way, they are the relatively orderly properties of processes of *self-organization* (what Stan Metcalfe calls "self-transforming market order") without however any equilibrium connotation attached to it, neither in term of market clearing of all markets, nor in terms of fulfilment of the underlying expectations of the individual agents.

Note also that such properties often have a *metastable* nature, in the sense that while persisting on a time scale longer than the processes generating them, might ultimately disappear with probability one[10].

Organizational forms

A similar style of representation and interpretation should apply to the emergence and self-maintenance of *organizational forms* and institutions: they are partly the result of directed (purposeful) actions by the agents but also, partly, the unintentional outcome of collective interactions and the interplay of agents learning. I will come back to the organization domain in a short while.

Co-evolutionary dynamics

The relation of the "higher level" regularities manifested in institutions, rules and organizational forms to "lower level" evolutionary processes is a complex one of co-evolution across *levels of analysis* and *time scales* -- and ought to be properly understood and possibly modelled as such. While the former are emergent phenomena of the latter, they may be considered as relatively invariant structures which constrain and shape the latter on shorter time scales. Modelling approaches that take these higher levels quasi-invariants as given have the same provisional legitimacy granted more generally to models that exclude, in the imperative spirit of dividing the difficulties, significant forms of novelty.

This is the "grand program", as Sid Winter - with whom I wrote down the foregoing list of paradigmatic building blocks-, and I, see it. It is impossible to review in this essay the rapidly growing literature that share parts or all of it . Some discussions and a quick review of the achievements up to a decade ago are in Dosi and Winter (2002), and much more detailed one addressing specifically technological change and industrial dynamics in Dosi and Nelson (2010). Here less ambitiously let me highlight some crucial domains of research within such "grand program", so to speak, from the very micro to the macro.

Microfoundations: Cognition, behaviours and learning in complex evolving environments

[10] On the notions of the "emergence" and "metastability", cf. the suggestive discussion in Lane (1993); see also below.

"Microfoundations" of course stand also for the account of actual behaviours of agents, being they individuals or organizations.

I have repeatedly discussed stories of the type ".. let us start with assuming that agents max (something) and build some theory from there…", with all the paraphernalia of very dubious epistemological claims such as "this is just a useful yardstick", "this is the outcome of an 'as … if' process even if I am unable to formally write it down", etc. One of such discussions of mine is indeed in the introduction to Dosi (2000). And of course major rebuttals are in Winter (1964), Nelson and Winter (1982), and many works in Herbert Simon (1957) and (1969). The defence of "start from max (…)" in fact in my view is too pathetically near the old story of the drunkard looking for his keys under a street lamp because that was the only lit-up place even if he remembered that he lost the keys somewhere else.

Incidentally, this was not originally the case. Savage (1954) in his classic *Foundations of Statistics* was extremely cautious: any reasonable representation of behaviours in terms of max (something) had to be limited to *small worlds* (and even that was an *upper bound*). The notion did not have to do with the current network-related meaning but rather to a much more down-to-earth restriction that the *possible states-of-the-world* upon which agents were taking decisions had to finite, well-known to everyone, and everyone could somehow come up with probability distributions over them. On that, Savage was extremely clear and utterly humble.

We know what happened since then: the wide spreading of the description of behaviours in terms of increasingly sophisticated maximizing assumptions without any empirical or theoretical discipline, the only constraint being "how much math I can learn and sell on intertemporal maximization".

On the contrary, in most economic circumstances, featuring change and innovation, maximizing rationality cannot be characterized *even in principle*, let alone being an attribute of actual behaviours.

But then what do people and organization do?

In my view, in order to answer the question concerning behaviours and learning, we must heavily borrow from cognitive and social psychology. And too bad if what we are getting from these discipline is a far cry from the behavioural assumptions of mainstream economics.

Indeed, I fully share Kahneman's view that

Psychological theories […] cannot match the elegance and precision of formal normative models of belief and choice, but this is another way of saying that rational models are psychologically unrealistic […] Psychology offers integrative concepts and mid-level generalizations, which gain credibility from their ability to explain ostensibly different phenomena in diverse domains" (Kahneman, 2003, p. 1449)

Recently, a lot of progress has been made in several directions. Let me just mention two.

One is *neuroeconomics*. The way i see it as a fruitful venue of research is not related to the reductionist flavour that some of the exercises convey (… " map greediness in this part of the brain and generosity in that other part …"). Rather, because some such *proximate* mapping does appear indeed to be possible, neuroeconomics helps in identifying and taxonomizing multiple drivers and processes underlying evaluations and decisions (for thorough reviews of the field, Rangel et al, 2008, and Camerer, 2007).

A second venue of progress have been the exploration and refinement of the conjecture that humans operate on the grounds of two distinct systems of cognition (and as a consequence also of action elicitation). As from Kahneman (2003), call them *System 1* (driven by intuition – fast, parallel, automatic, effortless, associative, slow-learning, emotional) and *System 2* (driven by reasoning – slow, serial, controlled, effortful, rule-governed, flexible, neutral) (Kahneman, 2003, p. 1451; see also the early Schneider and Shriffin, 1977a and 1977b).

Loosely speaking, most contemporary development are somewhat *Simonesque* in spirit, although they move much further away from any notion of rationality (even of the *procedural* kind) than Herb Simon would have been ready to go. In fact, the variegated body of research bordering economics, psychology and cognitive studies, are increasingly filling in a "model of man" which shall ultimately include also (i) cognitive foundations of both "System 1" and "System 2" based on imperfect and evolving *categorizations* and *mental models*; (ii) ubiquitous valuation and decision *heuristics*¸(ii) *context-dependence* and *social-embeddedness* of both interpretative models and decision rules¸(iv) evolving (and possibly inconsistent) goals and preferences. (We attempt to formalize such dynamics with respect to demand patterns and explore their economic implications in Aversi et al.,1999)

The "transparency of the world" and the assumption that "there cannot be money left on the table": a digression

I keep repeating it every possible occasion and I have already mentioned it above. Economics is the only discipline which assumes that the economic world is *structurally transparent* to the agents who populate it. They might have imperfect information – in the sense of some noise on the signals they receive form the environment – and also incomplete information – basically meaning that I might not precisely know whether the agent in front of me is Saint Therese of Calcutta or Al Capone – but for the rest everyone knows the *true* structure of the world, what causes what, how it works and will always work.

While the very existence of all scientific disciplines is motivated by the search for *causes* and processes – from the movements of the planets, the drivers of biological evolution, the causes of particular diseases, all the way to the working of our brain. And even all of us in everyday life operate as "naive scientists" asking, say, what causes the firm where I work to give a pay rise or not, or asking whether the changed attitude of my wife is because she got a lover ...

This does not happen in economics.

Indeed, the dominant theorizing mode implies that "the map *is* the territory" in some ontological sense. It is a bit as if one started from the assumption that physical bodies know Newton laws, particles know Boltzmann equation and bees knew the dynamics governing the beehive.

What is the "true" model then? Needless to say, it looks very much like the model that any particular economist developed in his Ph.D.! And the majority of the profession is so convinced by this utterly naive (one could say outrageous) ontology that an inordinate amount of scientific energies has gone (with the usual confusion between the descriptive and normative domains) into the thread of instruments by which agents try to channel and control the stochasticity of the relevant variables of an otherwise perfectly understood

world: witness the bulimic amount of work on options, Black and Scholes, derivatives, etc. [In that, not "stochasticity" in general, but most often the Gaussian one which they could more or less master!]

It should come as no surprise, then, that many economists were so surprised by the arrival of the big financial crisis. *At last* (!) they noticed that the variables of interests generally are not not normally distributed, but fatter-tailed - there are a lot of "big events" and "black swans" are relatively frequent. Indeed what is actually most striking is that there was (is) a whole community religiously believing in statistical normality!

However the acknowledged of fat-tailedness – as welcome as it is – just scratches the surface of the foregoing, much deeper, issue: as the world is everything but transparent, agents operate on the grounds of different, sometimes wildly different, models of the same world and, ultimately the observed variables are the outcomes of their interactions.

All this has a lot to do also with the "no money left on the table" assumption.

This is yet another *mantra* that all graduate students in respectable U.S. universities soon have to learn. It is a sort of non-arbitrage hand-waving theorem stating the presumption that if there is an opportunity, someone sooner or later will grab it (Chari's witness to the U.S. House of Representatives *after the crisis*, says it all with moving faith (Chari, 2010)). The "no money left" creed however withers away as soon as one acknowledges the intrinsic opaqueness of causes and processes, so that opportunities if they are there, they are hard to see, and can be detected with some spectacles and not with others.

Was the financial crisis due to the fact that out there there was an outrageous number of exploitable idiots and not enough clever exploiters? Not at all. On the contrary, a major driver has been an endogenous evolution of cognitive models and behavioral patterns fostering the "survival of the reckless" as Jacobides and Winter (2010), put it, ultimately driving the system toward the abyss.

Organizations: behaviours and learning patterns

An isomorphic question concerns *organizations*. What do they actually do? And how do they change their behaviours and their internal functioning? [that is how do they learn, if they do it at all?]

Again, one familiar answer is that firms max (something) plausibly profits, subject to a technological constraint (their "production function") and conditional on the information they access. In this case, as Herb Simon argued long ago, one does not have any need to open-up the "organizational blackbox". It is sufficient to know what the firm maximizes, the production function and the information set to be able to account for what the firm will do without, so to speak, looking into its belly.

To be fair, also mainstream theory has moved a long way away from such a blackboxing. The acknowledgement of the trivial fact that organizations are made up by more than one people, possibly with interests not perfectly aligned, calls for the opening up of the box because what the organization does and ultimately its performance does depend on the intra-organizational relations among its members. This is what *Agency theories* have been basically doing, in fact much more than Transaction Cost Economics (TCE), whose

primary and most natural focus has been the Coasian question of the boundaries between organizations and markets.

However, the agency-inspired opening-up of the box had very little to do with any inquiry of how organizations *actually behave*, and even less so of their *actual internal set-ups*. Rather, the intellectual industry has been to offer a rapidly expanding menu of models of firms as microcosms composed by asymmetrically informed, self-seeking, sophisticatedly rational individuals linked up by equilibrium contracts. What the members of the organization do and ultimately its overall performance depend on the characteristics of such contracts together with conditions that are partly *lato sensu* "technological" and partly "social" – including the distribution of information, the degrees of observability of efforts and outputs, etc. -.

In essence, the *virtuoso* exercise in this domain is to substitute the maximizing "organizational blackbox" with an ensemble of many, even more sophisticated, contractually linked, *individual blackboxes.*

And here and throughout the magic word is *incentives*.

Contracts – whether of the formal, legally enforceable kind or of the "relational", informal one – entail an incentive structure, and, given the contract, these mini boxes will fire out the optimal response (optimal for each of them of course, even if generally not first-best for the organization as a whole). Incentives, as crisply reviewed by Bob Gibbons (see Gibbons, 2010), one of the best in the trade, may be directly *economic* ("have a price attached") or *political* (in terms of lobbying, influence, collusion, etc.). Indeed, the prescription "find the incentive structure able to account for behaviour *x*" is as core to mainstream Ph.D. teaching in microeconomics, as it is "find the DSGE model and the appropriate calibrations to account for statistics *y*" in macroeconomics.

Well, not surprisingly, the perspective outlined in this essay entails advancements in the *opposite* direction.

As we argue in Marengo and Dosi (2005) and elsewhere (cf. the chapters on organization in Dosi, 2000), we may clear the way by just assuming, to begin with, a *weak incentive compatibility*, simply standing for the assumption that – at least in economic organizations – no one will be required to heroically undertake actions which benefit the organization while massively damaging the person undertaking them. Full stop.

Granted that, our perspective offers, as *first order* account, a view of organizations as *complex problem-solving institutional arrangement* – where, as we have repeated endless times, "problem-solving" stands for production problems (how to build a car ..) and search problems (finding the vaccine for malaria …) which are typically *complex* also in the technical sense that (i) they might not be perfectly decomposable (so that whatever "solution " to a sub-problem thereof influences other sub-problems as well), and, (ii) several classes of such problems might be computationally "hard", so that the full exploration of the problem-solving tree might take a time more than polynomial (indeed exponential) in the problem's arguments (On problem-solving in general, see the classic Simon, 1969 and 1983; a germane discussion of ours is in Dosi and Egidi, 1991).

Note that problem-complexity, decomposability (or not) and their mapping into different *intra- (*and *inter*-) organizational division of labour in principle has nothing to do with issues of incentive governance (even if it influences the latter), but rather impinges on the characteristics of organizational knowledge

and its distribution. In turn, that has a lot to do with the characteristics of *organizational routines* (on the notion, growing out of the seminal Nelson and Winter, 1982, see among others Cohen et al., 1996; Becker et al., 2005; Becker, 2005, and the literature reviewed here) and, relatedly, of *organizational memory* (indeed, we are currently working on its formalization : cf. Dosi et al, 2011a).

Come as it may, there is an emerging *knowledge-based, capability-based*, theory of the firm, based on a *procedural* view of distributed organizational knowledge, which one begins also to formally investigate[11].

Yes, I admit, there is the risk of be taken as too much of a "technological determinist", in the sense that such capability-centred theories derive crucial characteristics of organizational set-ups from stringent knowledge-related requirements. I am ready to run that risk : I have spent a good deal of my academic life, at least since Dosi (1982), investigating the nature and dynamics of technical knowledge, and I do stand comfortable by the notion that such knowledge entails quite hard constraints either to the fine tuning of incentives – as the smartest agency theorists currently address – or to various sorts of "political negotiations" and "social constructions" - as a few post-modernist streams of thought vocally argue. My never forgotten friend and mentor, Keith Pavitt, used to ironically reply to the latter that "no one wants to fly the Atlantic in a socially constructed airplane"! . By the same token I would never fly either on an airplane that is the outcome of a bundle of optimal contracts!

In the *capability view* of the firm, capabilities are large chunks of interrelated routines and "other quasi-genetic traits of the firm …" (Winter in Cohen et al., 1996) - inertial, path-dependent, quite opaque to environmental feedbacks – : in the short-term, *state-variables* as opposed to *control-variables* as Winter (1988) put it. And they are resilient, *primarily because they are learned, knowledge-rich, responses to external or intra-organizational signals* grounded on cognitive and habit-related factors. Their *nature* is indeed far from being a decision either derived from some argmax (.., ..) subject to some constraints. By the same token, I do not believe for a second that "routines are equilibria" in some game-theoretic space (indeed, I do not believe that Bob Gibbons, suggesting it in 2006, and 2010, is too hot about it it either!).

Granted that, broadly defined motivations and rewards are indeed important in shaping behaviours.

This is what we try to start formally exploring in Dosi, Levinthal and Marengo (2003). There we present a general model of organizational problem-solving in which we explore the relationship between problem complexity, decentralization of tasks and reward schemes. When facing complex problems which require the coordination of large numbers of interdependent elements, organizations face a decomposition problem which has both cognitive dimensions and reward dimensions. The former relate to the decomposition and allocation of the process of generation of new solutions: since the search space is too vast to be searched extensively, organizations employ heuristics for reducing it. The decomposition heuristic takes the form of a division of cognitive labour and determines which solutions are generated and become

[11] For a more detailed review of the whole *genre* of formal models trying to grasp procedural knowledge, routines, and their dynamics, cf. Dosi et al. (2011b).Important contributions in this domain include Levinthal (1997), Gavetti and Levinthal (2000), Ethiraj and Levithal (2004), Siggelkow and Rivkin (2005).

candidates for selection. The reward dimensions basically shapes the selection environment which chooses over alternative solutions.

Consider this just as an initial inroad within a broader agenda whereby one begins to bring also *power*, yes, *incentives* and *conflict over the distribution of net output* into a story that so far – for good reasons, as I have argued above – begins with knowledge, its distribution within and across organizations and its patterns of accumulation.

Problem-solving procedures and the theory of production

As I have already emphasized, I believe that the "primitive" levels of description of technologies are in terms of, first, the nature of problem-solving knowledge, and, second, the actual production and search *procedures* implemented by organizations (business firms, but also non-profit organizations).

This is "where the action is". And the characterization of these procedures is also where ultimately the *theory of production* rests (likeminded arguments are in Winter, 1982; 2005; and 2006). However, procedures are obviously linked with several sorts of material and immaterial inputs (ranging from raw materials to machines all the way to software and services) and finally yield some outputs which, again, can be goods or services.

But, how do procedures map into this lower dimensional space of inputs and outputs?

Certainly, there are candidates to any empirically founded theory of production which have to be ruled out. They prominently include standard production functions in their familiar version on the continuum (and, even more unlikely, homogeneous, degree one, etc.) one, but also in the discrete version of Activity Analysis coming together with the axiomatics of divisibility, additivity, convexity, topological closure of the production possibility set, etc. Indeed, many of these assumptions are far from innocent – and a few are far from plausible: Winter's works on the production theory bear many precious hints (in addition to those cited above, cf. Winter, 2008, and on replication of techniques, Winter and Szulanski, 2001). Also in this domain I believe that one should be very cautious about any form of axiomatics (one day some historians of economic thought will document the damages that the related, French and non-French, topological formalism brought to economics, one of the early monuments to it being Debreu's *Theory of Value*).

On a much more phenomenological ground, it seems to me much more reasonable to characterize firms with the same "industry" - no matter how precisely defined in terms of output – as distributions of fixed ("Leontief") coefficients in the short term, with longer term dynamics shaped by both idiosyncratic learning and environmental selection (more on it below[12]).

[12] I easily admit that this stand on "axiomatics", *on production theory,* is quite radical and even Sid Winter is unwilling to go that far. So, in a comment on a previous draft of this Introduction, Sid wrote to me that he "would argue that the familiar axioms have a reasonable claim to being a good approximation to some parts of economic reality … [depending] strongly on the scale of the phenomena [one] is talking about , e.g. whether [one is] talking about capacity utilization levels of an industry or in a single plant", while of course acknowledging that the "key shortcoming of the traditional apparatus is its built-in distancing from technological change". Of course I fully share the last part of the comment. Concerning the former part, I do think that a theory of production is *in primis* a *micro* theory, but it is there where the distance between the standard axiomatics and the empirical evidence is particularly high (I trust Sid agrees on that). And, in my view, taking on board

In fact, as I shall recall below, also the interpretation of aggregate growth crucially builds upon an account - at the level of firms and sectors - of the *nature and dynamics of technological knowledge*, its sources, modes of access and mechanisms of economic exploitation (We discuss all this at length in Dosi and Nelson, 2010).

Demand patterns and market dynamics

So far I have basically discussed what goes on in the head of individual economic agents and the somewhat more metaphorical head of organizations – that is the microeconomics of the supply side. But what about demand and market interaction?

The proposal here is to interpret the demand profiles of multiple socially adaptive, but possibly innovative, agents – characterized by lexicographic (that is hierarchically ordered) preferences, obviously a budget constraint, and some inclination to reduce cognitive dissonance ("... how much I would like to get good z, but I cannot afford it, therefore let me convince myself that I do not like it that much after all ..."... the fox and the grapes …): for a preliminary formalization, cf Aversi et al (1999).

The topic is indeed near one of the cores of economic analysis. In fact most economists and even undergraduate trainees, when asked what economics is about, would put very high on the list the answer: "it has to do with supply and demand … if prices increase demand falls, and symmetrically if prices increase supply augments ...".

Let me leave supply on the side for a moment, as it has to do with the foregoing issues concerning the theory of production. Rather let me focus on demand. After second thoughts, what is the *demand curve* about?

It could be two things.

First, it could be a "psychological proposition" about agent-specific *notional* but indeed, by assumption, clear and coherent preferences. After all this is what in many models agents deliver to the mythical "Walrasian auctioneer". To the same effect in the General Equilibrium philosophy no one explains its preferences to anyone else but still behind the existence of an equilibrium there are well-behaved individual demand functions [That notwithstanding, I have already mentioned the dramatic pitfalls of aggregation: well-behaved (downward sloping) individual demand do not translate into isomorphic aggregate excess demand functions].

Note that in any case this first interpretation of "demand" involves an ensemble of counterfactual thought-experiments (one for each individual)

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the standard axiomatics is also misleading in that it tends to induce an appreciation of, say, "industry level production technology" as an allocative problem across micro techniques. Yes, most often at this level it is easy to describe an industry *ex post* in terms of convex, divisible, etc. sets of microtechniques even when one abhors any choice-theoretic description of what goes on behind them (an example is in Hildenbrand, 1981. which I find very insightful). These sets, however, are very interesting objects in their own right for the description and analysis of the moving distributions of industries techniques (indeed I use them with my collaborators), but in my view, they do not offer foundations to any theory of production - *except under Central Planning*- if by the latter we mean a theory interpreting why the micro coefficients are what they empirically are. Let the discussion unfold. In any case, as the reader has certainly noticed, I hold a sort of methodological prescription which even my mentor and co-author Sid possibly shares to a lower degree: *build the least decision-theoretic, or worse, game-theoretic, model possible.* On some major misunderstandings on the relationships between axiomatics and modelling see below.

while of course at each time one just observes an ensemble of points (i.e. combinations of price(s)/demanded quantities).

Alternatively, second, demand functions could be understood as representing a *notional* aggregate relation in any one market, given distributions of micro preferences, not necessarily well behaved or even coherent (in this perspective, Hildebrand, 1994, investigates the statistical conditions under which such aggregate relations are "well-behaved").

In general, the actual relations prices/quantities in any one market depend on (i) the way markets are organized and on (ii) the different "ecologies" of decision rules and behaviours. Both determine *how markets work*.

What do we know about it?

In a striking paradox, relatively little: economists, who tend to use the word "market" in every other sentence, have mostly kept away from investigating their actual working, maybe fearful that by looking at them some of the "magic" would disappear!

Notable exceptions here are the works of Alan Kirman and colleagues on the fish and other markets: see among other Kirman and Vignes (1991) and the forthcoming *Journal of Economic Behavior and Organization* Special Issue (Sapio, Kirman and Dosi, 2011).

All these studies reveal, first, robust behavioural patterns quite at odd with optimizing behaviours (at least in their most naïve versions).

Second, they vividly illustrate the already mentioned lack of isomorphism between individual behaviours – including the price/quantity profiles of individual buyers – and the aggregate price/quantity patterns of the market. The latter have to be properly understood as emergent outcomes of the interaction of multiple, heterogeneous, rule-governed, budget-constrained agents.

Third, all studies abundantly support the proposition that the institutional architecture of the markets (e.g. whether based on pairwise interactions vs. auctions of different types) influences the revealed outcomes – in terms of price levels and changes,dispersion, volatility etc.- even when holding unchanged the characteristics of the object traded and the ecology of behavioural rules of market participants.

Still, most of work in this area awaits to be done.

Industrial Evolution

Contemporary economic analysis is largely subject to a rather bizarre schizophrenic syndrome. On the one hand, as already discussed above, over the last 30 years or so, macro theories have tried to squeeze the interpretation of aggregate dynamics down to some sort of decision-theoretic framework in which the mythical "representative agent" was doing all the action. Whatever the statistical properties of the time series, being it productivity and GDP growth, fluctuations, employment, investment, it had to be explained as the equilibrium outcome of some sophisticated intertemporal maximization exercise by such an agent. Conversely, on the micro side largely the opposite has happened. Empirical analyses drawing upon an increasing ensemble of micro longitudinal data sets have powerfully highlighted the ubiquitous, large and persistent heterogeneity in all dimensions of business firms' characteristics and dynamics one cared to look at.

Thanks to massive infusions of micro-data (at plant and firm level levels) into economic investigation over the last 20 years, one has begun to identify a few robust statistical properties characterizing industrial structures, their changes, and performance indicators such as corporate growth and profitability. (See also Dosi and Nelson, 2010; and Bottazzi et al 2010).

In brief, the "stylized facts" include:

i. Highly right-skewed firm *size* distributions (which at the level of the whole manufacturing industry are unimodal and closely resembling a Power Law[13] but significantly depart from it at higher degrees of sectoral disaggregation).

ii. Phenomenological description of firms growth as a multiplicative stochastic process independent from size ("Gibrat Law") yield a fairly good first-order description of the observed dynamics. However, significant deviations from the simplest benchmark process concern (a) small firms; (b) the general (negative) dependence of growth rates upon age; and (c) the (negative) scaling of growth *variances* with size itself.

iii. The distributions of *growth rates* themselves are fat-tailed, as such a sign of some underlying correlating mechanism which would not have been there if growth events were small and independent.

iv. At any level of disaggregation one observes widespread *differences in productivity* (no matter whether measured as labour of "total factor productivity", whatever that means) across firms and across plants. Such differences are highly persistent over time.

v. Equally widespread *differences in profitability*, again at all available levels of disaggregation. And, again, profitability differentials are persistent in time.

vi. Finally, the number of innovators within each industrial sector is a small fraction of the whole population of firms, even in technologically leading countries.

Given this evidence, one is bound to ask what drives persistent asymmetries in performances and heterogeneity in corporate characteristics.

In brief, as we argue in Dosi and Nelson (2010), the single most important factor in accounting for persistently heterogeneous performances rests indeed upon equally heterogeneous organizational capabilities – idiosyncratic, difficult to imitate, often only incrementally changing over time.

Granted that, a set of tricky and difficult questions regards the relationships between corporate characteristics, performances and their dynamics. There are here both empirical challenges and tangled theoretical issues. For example, can one rationalize such relationships in terms of some underlying general equilibrium, albeit of a rather weird kind? What would that add to our interpretation of the evidence? Or, conversely, should one understand it as far from equilibrium evolutionary dynamics? In any case, what drives such processes? What is the balance in it between idiosyncratic and mistake-ridden innovation, learning, adaptation, on the one hand, and environmental selection among competing firms, but also products, technologies, patterns of organization, behavioural rules, on the other?

An archetypical evolutionary story about the relationship between firm specific characteristics and performances runs roughly like the following.

Different productivities, organizational setups, propensities to innovate, and corporate strategies make up the distinct corporate identities which in turn should influence firms' performances. More productive firms are able to charge

[13] A Power Law is a relationship of the type $Pr(X>x) = ax^{-b}$, accounting for the probability that a random variable X is greater than x, with a and b as constant, and b often empirically found to be near one.

lower prices for the same quality goods and thus increase their market shares; more innovative firms are able to sell products which are "better" in some dimensions, likewise increasing their shares in differentiated industries; and, finally, more efficient and more profitable firms are able to grow more because they are able to invest more given far less than perfect capital markets.

On the theory ground, the formal account of the same story is in terms of some explicit Fisher-Price or whatever *replicator dynamics*, such as in Silverberg et al. (1988) and Metcalfe (1998) among many others, or in terms of some implicit efficiency-related replication as implied by a Nelson-Winter type investment dynamics.[14]

But how does this story fares against the evidence?

Let me consider first the impact of different productivities upon profitability, growth, and survival probabilities. Mainly North American evidence, mostly at plant level, does suggest increasing output shares in high-productivity plants and decreasing shares of output in low-productivity ones as drivers in the growth of average sectoral productivities, even if the process of displacement of lower efficiency plants is rather slow .

In complementary efforts, a growing number of scholars has indeed began doing precisely what we could call evolutionary accounting, even if most do not call it that way. The fundamental evolutionary idea is that productivity distributions change as a result of (i) learning by incumbent entities, (ii)differential growth (i.e. a form of selection) of incumbent entities themselves, (iii) death (indeed, a different and more radical form of selection), (iv) and entry of new entities.

Favoured by the availability of micro longitudinal panel data, an emerging line of research (cf. Olley and Pakes, 1996; Foster et al., 2001; Bottazzi et al,2010 ; and the discussion in Bartelsman and Doms, 2000), investigates the properties of such decompositions, identifying the contribution to productivity growth of (1) firm-specific changes holding shares constant (sometimes called the *within* component); (2) the changes in the shares themselves, holding initial firm productivity levels constant (also known as the *between* component); (3) some interaction term; plus, to repeat,(4) exit ; and (5) entry.

Of course, there is a considerable variation in the evidence depending on countries, industries, and methods of analysis. However, some patterns emerge.

First, the *within* component generally is significantly larger than the *between* one: putting it another way improvement of productivity by existing firms dominates upon selection across firms as a mode of industry advancement at least concerning productivity (both labour and total factor productivities). This emerges both from the foregoing evolutionary accounting exercises and from estimates of the relationship between efficiency and subsequent growth. Moreover firms identified as more productive tend also to be more profitable than other firms. The impact on growth is, instead, much less clear-cut. The

[14] A replicator dynamics relates the variation of the frequency of traits (or individuals carrying such traits) in a whatever population to the relative "fitness" of the traits themselves. In biology, the classic formalization is from Fisher (1930), which several works by Stan Metcalfe build upon and refine. Gerry Silverberg and a few others (including several works of mine and collaborators), broadly follow the same formalization pattern allowing for a dynamics in the "fitness" and their distributions across micro entities. In Nelson-Winter style modelling, no explicit "replicator equation" is there but in their models the relation from *relative production efficiency to relative profitabilities to relative investment possibilities* plays the same role.

evidence analysed by Bottazzi et al (2010) suggests a weak relationship between relative (labour) productivities and growth: more efficient firms do *not* seem to grow much more. Further, when some positive relation between efficiency and growth appears, this is almost exclusively due to the impact of few outliers (the very best and the very worst). And, this holds in both the short and the medium term.

Second, relative efficiencies do influence survival probabilities, and it may well turn out that selective mechanisms across the population of firms operate much more effectively in the medium/long term at this level rather than in terms of varying shares over the total industry output. (Here the challenge is primarily empirical/statistical, as it is relatively hard to find reliable data on genuine "death" of firms, linkable with their past performances).

Third, all the evidence I am familiar with strikingly shows little or no link between profitability and firm growth of incumbents . However, other pieces of evidence suggest also systematic effects of profitability upon survival probabilities (cf. the discussion in Bartelsman and Doms, 2000; Foster et al., 2008).

The implications of all the foregoing empirical regularities identified so far are far-reaching. Certainly, the recurrent evidence at all levels of observation of interfirm heterogeneity and its persistence over time is well in tune with an evolutionary notion of idiosyncratic learning, innovation (or lack of it), and adaptation. Heterogeneous firms compete with each other and, given (possibly firm-specific or location-specific) input and output prices, obtain different returns. Putting it in a different language, they obtain different quasi-rents or, conversely, losses above/below the notional pure competition profit rates. Many firms enter, a roughly equivalent number of firms exits. In all that, the evidence increasingly reveals a rich structure in the processes of learning, competition, and growth.

Moreover, as mentioned, various mechanisms of correlation, together with the sunkness and indivisibilities of many technological events and investment decisions, yield a rather structured process of change in most variable of interest, - including size, productivity, profitability - also revealed by the fat-tailedness of the respective growth rates.

At the same time, market selection among firms - the other central mechanism at work together with firm-specific learning in evolutionary interpretations of economic change - does not seem to be particularly powerful, at least on the yearly or multi-yearly time scale at which statistics are reported. Diverse degrees of efficiencies seem to yield primarily relatively persistent profitability differentials. That is, contemporary markets do not appear to be too effective selectors delivering rewards and punishments in terms of relative sizes or shares, no matter how measured, according to differential efficiencies. Moreover, the absence of any strong relationship between profitability and growth militates against the naively Schumpeterian notion that profits feed growth (by plausibly feeding investments).

Selection among different variants of a technology, different vintages of equipment, different lines of production does occur and is a major driver of industrial dynamics. However, it seems to occur to a good extent *within* firms, driven by the implementation of better processes of production and the abandonment of older less productive ones.

In turn the apparent "selection weakness" might be rooted into multiple reasons – from sheer statistical to genuinely interpretative ones.

First, one measures productivity - supposedly an underlying driver of differential selection - very imperfectly: one ought to disentangle the price component of value added (and thus the price effects upon competitiveness) from physical efficiency to which productivity strictly speaking refers, but only very rarely one is able to do it. This applies to homogeneous products and even more so when products differ in their characteristics and performances. As in modern industries most often product innovation and product differentiation are a fundamental competitive dimension one should explicitly account for their impact of the latter upon revealed selection processes.

Second, but relatedly, the notion of sharp boundaries between industries and generalized competition within them is too heroic to hold. It is more fruitful in many industries to think of different sub-market of different sizes as the locus of competition (see Sutton, 1998). The characteristics and size of such submarkets offer also different constraints and opportunities for corporate growth. Ferrari and Fiat operate in different sub-markets, face different growth opportunities, and do not compete with each other. However, the example is interesting also in another respect: Fiat can grow, as it actually happened, by acquiring Ferrari.

Third, a growing microevidence highlights the intertwining between technological and organizational factors as determinants of Schumpeterian competition: Bresnahan et al. (2011) illustrate the point in the case of IBM and Microsoft facing the introduction of the PC and the browser, respectively. Both firms, the work shows, faced organizational diseconomies precisely in the corporate activities where they were stronger, due to the mismatching between trajectories of technological change, internal organizational set-ups and market requirements.

Fourth, the links between efficiency and innovation, on the one hand, and corporate growth, on the other, are in any case mediated by large degrees of behaviour freedom, in terms, for example, of propensities to invest, export, expand abroad; pricing strategies; and patterns of diversification.

Come as it may, the evidence on the apparent weakness of selection processes requires that evolutionary theories re-think their account of the *selection landscapes* – that is the space over which competitive interactions are represented – possibly increasing the number of arguments (e.g. not only production efficiencies and prices but also product characteristics) and maybe allowing for non linear effects (so that for example competitive forces might bite hard just in favour of the very "best" and against the very "worse"...). Indeed, important challenges ahead for the theory.

History and Evolution

The evolution of technologies, firms, industries, institutions and whole economics unfold as a historical process entailing varying degrees of *path-dependence* and *irreversibility*. Path-dependence stands for the influence that initial conditions and subsequent events along the historical process bear on long-term outcomes. And the (related) notion of "irreversibility" stands for the varying measures of *lock-in* of the process itself and the difficulties in, so to speak, "run it backward". As Davidson (2011) emphasizes in the "Kay dabate", *non-ergodicity* is the general rule in economic processes.

Indeed, while sheer intuition would suggest that *history matters* at all domains of socio-economic change, it is sad to acknowledge that history does

not matter for most contemporary economic theory and econometric practice, and when it does it is via initial, possibly persistent, but exogenous conditions of otherwise invariant processes (the hunt for "instruments" in current econometrics of "comparative political economy" is a good case to the point).

In fact, path dependence might emerge at both individual and system levels. One could think of systems composed of path-dependent agents which are however collectively *ergodic* at least in the long term – even if my guess is that this is empirically pretty unlikely , or alternatively systems composed of agents with history-free behaviours displaying nonetheless aggregate path dependence. [My priors are that in many circumstances one is going to find some path dependence at both levels. My friend Paul David has done an excellent job in showing the emergence of path-dependent phenomena even under microeconomic assumptions of rational enough and "flexible" agents: see for example David (1988), (1993) and (2005)]

Path-dependence has many sources, including the dynamic increasing returns associated with the accumulation of technological knowledge, network externalities, complementarities in the adoption of innovation and agglomeration economies. Moreover, I fully agree with David (1994): corporate organization and institutions, largely rule-governed entities, are as such "carriers of history".

Finally, path-dependency is going to appear whenever the "selection landscapes" (see also the brief discussion above) – over which technologies, organizational traits, behaviours, etc. evolve – are *rugged*, with multiple peaks. In this case, the process is likely to get stuck in one of the multiple local maxima.

Path-dependent world always entail the possibility that technological and institutional evolution "gets it wrong" - in the sense that convergence is to the dominance of a technology or an organizational form which is "inferior" to other ones available in some form from the start, which however the collective did not reinforced.

In this domain, works by Brian Arthur, Paul David, Yuri Kaniovski, Robin Cowan and also myself – among others – is just scratching the surface of some grand interpretative questions. The first - paraphrasing Stephen Jay Gould - concerns what "would be conserved if the tape of history could be run twice", that is, what are the aspects of socio-economic evolution which can be thought as relatively invariant and those that are specific to a particular sample path, to a particular history.

A second grand question regards what one could call the permanent *tension between freedom and necessity* in human affairs, i.e. the degrees of tightness of the "grip of history" on what people and organizations can and/or "choose" to do.

And, relatedly, third, what are the factors which are able to *de-lock* from particular technologies, organizational set-ups, institutional arrangements?

Again, it is sad to observe that not much research has gone into all this: in my view, another lethal consequence of the trivialised "vision of man" (and the isomorphic "vision of society") in which forward looking agents, living in a causally transparent world maximize statistical expectations of something. In the world I have in mind, on the contrary there is much less transparency, much less"shadow of the future upon the present", as Gibbons (2010) put it, and even when behaviours are actually grounded upon expectations, the latter bear the mark of the evolution of collective beliefs, and also of collective

illusions and sheer blind madness, as the current financial crises vividly illustrates.

To repeat, I strongly believe that path-dependency is ubiquitous in human affairs (and not only there), from the very micro of individuals to the levels of institutions and macroeconomics. In turn, this is intimately related to what economists – mostly with some uneasiness – acknowledge as instances of potential *multiple equilibria* (if equilibria are more than one, historical circumstances are the likely candidates to explain which one is chosen) and *hysteresis* – as path-dependence has been called for a while in macroeconomics by a brave minority largely reduced to publication silence since. (An insightful example is Cooper and John, 1988, unfortunately with not much follow-up).

Toward a soundly microfounded evolutionary macroeconomics: some contributions to the Grand Project, and a few challenges ahead

The "Grand Evolutionary Project" as I see it explicitly builds upon the foregoing properties of agents' behaviors, of the patterns of innovative search, and of competitive interactions, trying to address, so to speak "head on", the interpretation of macroeconomic dynamics.

Macroeconomic dynamics is generated in the class of models that I am advocating here via simple aggregation of individual behaviours. Typically, non-linearities induced by heterogeneity and far-from-equilibrium interactions induce a co-evolution between aggregate variables (employment, output, etc.). In that, the statistical properties exhibited by aggregate variables might then be interpreted as emergent properties grounded on persistent micro disequilibria. The observed stable relations amongst those same aggregate variables might emerge out of turbulent, disequilibrium, microeconomic interactions.

Let me illustrate the *genre* with a self-reference to Dosi, Fagiolo and Roventini (2010) where we further study an agent-based model that bridges Keynesian theories of demand generation and Schumpeterian theories of technology-fuelled economic growth. Agents always face opportunities of innovations and imitation, which they they try to tap with expensive search efforts, under conditions of genuine uncertainty (so they unable to form any accurate expectations on the relation between search investment and probabilities of successful outcomes). Hence (endogenous) technological shocks (the innovations themselves) are unpredictable and idiosyncratic.

The model builds on evolutionary roots, and is also in tune with "good New Keynesian", insights (cf. for example Stiglitz, 1994a). It tries to explore the feedbacks between the factors influencing aggregate demand and those driving technological change. By doing that it begins to offer a unified framework jointly accounting for long-term dynamics and higher frequencies fluctuations.

The model is certainly well in tune with the growing literature on agent-based computational economics (see Tesfatsion and Judd, 2006; LeBaron and Tesfatsion, 2008), clearly meeting evolutionary but also Solow's (2008) pleads for microheterogeneity: a multiplicity of agents interact without any ex ante commitment to the reciprocal consistency of their actions[15].

Furthermore, the model—alike most evolutionary ABMs—is ''structural'' in the sense that it explicitly builds on a representation of what agents do, how they adjust, etc. In that, our commitment is to ''phenomenologically'' describe microbehaviors as close as one can get to available microevidence. Akerlof's (2002) advocacy of a ''behavioral microeconomics'', we believe, builds on that notion. In fact, this is our first fundamental disciplining device.[16]

[15] For germane ABMs, Delli Gatti et al (2005), (2010) and (2011), Russo et al (2007), Dawid et al (2008) and (2011), Ashraf, Gershman and Howitt (2011), and with both some Keynesian and Schumpeterian elements see, Verspagen (2002), Saviotti and Pyka (2008), Ciarli el at (2010) and the discussion in Silverberg and Verspagen (2005).

[16] With respect to the debate on modeling - cf also Michael Woodford's reply to John Kay – the model is an easy illustration of the straightforward fact that models can be models *without* being derived from anyaxiomatics. The identification of modelling with derivation from first principles is epistemologically wrong, *in economics but also in all other disciplines, also in physics.* Even in the latter, classical mechanics stands out as an exception: models in most other sub-fields are *phenomenological*. Conversely, theology look much more like

A second, complementary discipline involves the ability of the model to jointly account for an ensemble of stylized facts regarding both ‘‘micro/meso’’ phenomena with genuinely macro “stylized facts”. In the case of the mentioned model they include (i) endogenous growth, (ii) persistent fluctuations, (iii) recurrent involuntary unemployment,(iv) pro-cyclical consumption,investment, productivity, employment and changes in inventories; (v) fat-tailed distributions of aggregate growth rates; together with (persistent asymmetries in productivity across firms, (vi) “spiky” investment patterns; (vii) skewed firm size distributions ; (viii) fat-tailed firm growth rates.

We employ the model to investigate the properties of macroeconomic dynamics and the impact of public polices on supply, demand and the “fundamentals" of the economy.

We find that the complementarities between factors influencing aggregate demand and drivers of technological change affect both “short-run" fluctuations and long-term growth patterns. From a normative point of view, simulations show a corresponding complementarity between “Keynesian” and “Schumpeterian” policies. I consider this a major result, with far-reaching implications both in terms of theory and policies.

Both types of policies seem to be necessary to put the economy into a long-run sustained growth path. Schumpeterian policies *potentially* foster economic path, but they do not appear to be able alone to actually yield such sustained long-run growth. In a broad parameter region, “fundamental” (indeed, endogenously generated) changes in technology are unable to fully propagate in terms of demand generation and ultimately output growth. By the same token, demand shocks (in the simplest case, induced by government fiscal policies) bear persistent effects upon output levels, rates of growth, and rates of innovations. Hence, Keynesian policies not only have a strong impact on output volatility and unemployment but seem to be also a necessary condition for long-run economic growth.

In fact, our results suggest that the matching or mismatching between innovative exploration of new technologies and the conditions of demand generation appear to yield two distinct “regimes” or “phases” of growth (or absence thereof), also characterized by different short-run fluctuations and unemployment levels. Even when Keynesian policies allow for a sustained growth, their tuning affects the amplitude of fluctuations and the long-term levels of unemployment and output. Symmetrically, fluctuations and unemployment rates are also affected by “Schumpeterian policies", holding constant macro demand management rules.

As I see it, the model is a very encouraging template to be modified and refined in order to explore further domains of economic analysis. As such, however, it represents already in my view an important advancement vis-à-vis a whole first generation of evolutionary models pioneered by Nelson and Winter, which – I keep repeating to often less than enthusiastic evolutionary audiences – contain far too much Schumpeter and far too little Keynes.

Take the Nelson-Winter model(s). Together with their path-breaking merits in formalizing endogenous uncertainty-ridden technological search, they are, from the macroeconomic point of view, *equilibrium models*: the labour market clears and so does the product market. A central reference of them is Solow's growth model and the related quest is for much more reasonable (indeed, evolutionary!) foundations to the macro patterns of growth Solow identified. In

mainstream economics, with its derivation of propositions from unfalsifiable axioms!

that, however, they fall short of Keynesian economics, which – as Paul Krugman puts it, and I fully agree – is "essentially about the refutation of Say's Law, about the possibility of a general shortfall in demand". And in that view one finds "it easiest to think about demand failures in terms of quasi-equilibria models in which some things, including wages and the state of long-term expectations in Keynes' sense, are held fixed, while other adjust toward a conditional equilibrium of sorts" (Krugman, 2011, p. 3).

Indeed, as Kaldor (1983) sharply points out in his 50-years assessment of the *General Theory,* generic multiplicity of *non-Say quasi-equilibria* is the rule

Let me refine a bit on this,citing again Kaldor:

The originality in Keynes's conception of effective demand lies in the division of demand into two components, an endogenous component and an exogenous component. It is the endogenous component which reflects production, for much the same reasons as those given by Ricardo, Mill or Say – the difference is only that in a money economy (i.e. in an economy where things are not directly exchanged, but only through the intermediation of money) aggregate demand can be a function of aggregate supply (both measured in money terms) without being equal to it - the one can be some fraction of the other.

To make the two equal requires the addition of the exogenous component (which could be one of a number of things, of which capital expenditure - "investment" - is only one) the value of which is extraneously determined. Given the relationship between aggregate output and the endogenous demand generated by it (where the latter can be assumed to be a monotonic function of the former), there is only one level of output at which output (or employment) is in "equilibrium" - that particular level at which the amount of exogenous demand is just equal to the difference between the value of output and the value of the endogenous demand generated by it. If the relationship between output and endogenous demand (which Keynes called "the propensity to consume") is taken as given, It is the value of exogenous demand which determines what total production and employment will be. A rise in exogenous demand, for whatever reasons, will cause an increase in production which will be some multiple of the former, since the increase in production thus caused will cause a consequential increase in endogenous demand, by a "multiplier" process. How large this secondary increase will be will depend on a lot of things such as the retribution of the additional output between wages and profits, and the change in productivity (or in costs per unit of output) associated with the increase in production, etc. […] A capitalist economy … is not "self-adjusting" in the sense that an increase in potential output will automatically induce a corresponding growth of actual output. This will only be the case if exogenous demand expands at the same time to the required degree; and as this cannot be taken for granted, the maintenance of full employment in a growing economy requires a deliberate policy of demand management.

[…]

Keynes was no student of Walras. However, there was enough in Marshall (particularly in Book v, the short period theory of value) to raise the same kind of qualms – why don't all markets behave in such a way to compel the full utilisation of resources? Marshall's own theory suggested that saving provide the supply of "loanable funds" which, given an efficient capital market which equates supply and demand, governs the amount of capital expenditure

incurred. This amounts to a denial of the whole idea of an exogenous source of demand – the latter notion presupposes that the supply and demand for savings are brought into equality by changes in income and employment and not by the "price" of savings in the capital market, which is the rate of interest. In order to explain why the market for loans is not "market-clearing" in the same sense as other markets, Keynes introduced the liquidity-preference theory of interest – which, as is evident from his own later writings, was added more or less as an afterthought. (Kaldor, 1983, 172-175).

And, if I may, it was a bad, empirically far-fetched, and theoretically misleading idea. The one which allowed the Patinkin, Modigliani, etc of this world to step in and show that Keynes' model was after all a DSGE with frictions … (The paradox being that the neoclassical Vatican took so long to realize !) In fact, the advocacy is to proceed on the opposite route and analyse, together, the properties of *endogenous fluctuations* and *multiple non-Say growth paths*, conditional on different mechanisms of demand formation.

Relatedly, all the above bears a crucial link with macroeconomic contributions and in particular unemployment rates.

To quote Keynes (1943) as cited in Kaldor (1983):

… unemployment is not a mere accidental blemish in a private enterprise economy. On the contrary, it is a part of the essential mechanism of the system and has a definite function to fulfil. The first function of unemployment […] is that it maintains the authority of masters over men. The master has usually been in a position to say: "If you do not want the job, there are plenty of others who do". When the men say "If you do not want to employ me there are plenty of others who will" the situation is radically changed.

There are several further challenges for analysis broadly within an "evolutionary/Keynesian" perspective.

Financial dynamics and transmission mechanisms with the real economy

Finance is not a "veil" just rapping up real dynamics . At last, after the latest crisis, a rapidly growing ensemble of models takes seriously the fact that financial dynamics might systematically depart from some "fundamental process" (whatever that means …).

I think that major advances have been made in the understanding of correlating mechanisms of whatever origin on financial markets - with network theories helping a lot on the formal side -, together with the acknowledgement of cognitive and behavioural correlations.

But also in this domain there is a long way to go.

There are obviously theories that one should rule out: at this point I think the decency should prevent any respectable scholar to talk about "market efficiency" *et similia*.

Granted that, among decent candidates to the interpretation of financial market dynamics, I see a divide demanding to be fruitfully bridged.

On the one side, a set of investigations – going under the heading of "evolutionary finance"- seriously takes on board some form of inter-agent heterogeneity (at very least in term of risk aversions) and focuses on the properties of markets as *selection environments*. In that wealth variations play the role of a "replicator process" (c.f. above): see among others Levy et al (2010), Blume and Easley (1992 and 2010), Anufriev and Bottazzi (2010) and Anufriev and Dindo (2010).

Almost symmetrically, a variegated ensemble of analyses address as primarily e*xpectation variety* and *expectation dynamics*, in ways disjointed to varying degrees from "fundamentals"*.*

This ensemble includes, first, a good deal of behavioral finance.

It includes also more radical departures from any "fundamentalist" anchor*,* correspondingly accounting for phenomena such as imperfect, heterogeneous learning; imitation; herd behavior; "beauty contests" and "market reflexivity"; and changing knowledge frames.

This ensemble includes prominently Frydman and Goldberg (2011), building their interpretation on cognitively rather sophisticated agents but fully taking on board an assumption of *lack of transparency* similar to the one discussed above*.*

Although coming from a quite different tradition they happen to share with most scholars of the economics of innovation and of organizational studies the ideas that knowledge is *not* sheer information and cognitive maps are *not* isomorphic to the territory they try to represent.

To the opposite end, within the same ensemble one finds much simpler "strip down" models of social imitation, mimetism, herd or contrarian behaviors, etc. sometimes amenable to formal treatment. The models sketched out in Kirman (2010) are a good example[17].

Note that I am not advocating here an sort of meta-model unifying the two forgoing ensembles. However, more systematic links between the two would help a lot in understanding the *irresistible evolutionary drive toward the financial abyss,* as we call in an ongoing research project.

Finally, another major challenge concerns the coupling of financial markets with the real economy.

Putting it another way, we are still relatively far from a coherent merging between Minsky-type financial processes (Minsky, 1982, and 1986), on the one hand, and explicit accounts of decentralized evolving economies, on the other.

Policy experiments

Another major challenge ahead concerns "policy experiments", including those regarding monetary and fiscal policy prescriptions.

For example, were one to trust that the model mentioned earlier captures something important of the real world, what would happen if on the top of it one would put a Central Banker applying some sort of Taylor's Rule – linking purportedly, money supply, interest rates and inflation rates - ? Needless to say, there is nothing in our artificially created world implying any such relation. Want my guess: if you do not build it in, you do not get it out. In my view it is at par with other mythical objects such as the so-called "Ricardian equivalence"[18] or in the Middle Ages, the Unicorn, the miraculous properties *Mandragora roots*, and so on.

[17] Here I want, however, to mention an old model by Marengo and Tordjman (1996) with ecologies of forex trading strategies evolving over endogenous landscapes in absence of any fundamental.

[18] In fact, I think that the famous "intertemporal inconsistency of policies" belongs to the same *genre*. Indeed, one is led to think, and worse teaches students in textbooks, that maybe up to the '70 people were so stupid to adapt to fiscal and monetary policies, but thereafter, possibly rationally anticipating the "rational expectation revolution" of Lucas, Sargent et al. they stopped doing it. A sudden outburst of forward looking intelligence by the general people? Or an outburst of novel cleverer (and more "perverse") cooking of the data? You have my guess.

Are we doomed to live forever with unbridled globalization?

We live an international economy which is – fortunately or unfortunately - "globalized", Agent-Based, evolutionary, models ought to offer at least some pale images of it. Bad luck that most of models are closed economy ones. Of course the latter are bound to be a necessary first approximation.

To be fair, quite a few works within the "institutionalist/evolutionary family" address the issue, but it also holds that there seem little urgency within such a family to offer reasonable formal accounts of interacting, technologically and organizationally asymmetric, economies.

Yes, there are a lot of insightful hints, but I think the whole community is committing a major intellectual and policy crime not to link up with the admirable policy battle by Paul Krugman on the whole macro side, and even the more tamed point put forward by usually cautious Dan Rodrick on the inner incompatibility between globalization, national sovereignty and democracy (Rodrick, 2011).

I have emphasized a lot in the foregoing comments *formal* theories. This is not because I consider other forms of analysis less important. On the contrary: other approaches – from history grounded "appreciative" (qualitative) theorizing, to bottom-up statistical analyses all the way to case studies – are at least equally important complements, and sometimes more conflicting indeed. The point is however that a good deal of the interpretation of economic phenomena and an overwhelming part of the policy debate is informed by *theory* – indeed, as the reader who got to the current page of this introduction well understands – in my view a *very bad theory*. And with very *pernicious policy* implications.

Take the diagnosis of the current crisis. Let me leave aside the *pasdarans* who believe that it was just the outcome of an aggregate supply shock (and therefore presumably there is no voluntary unemployment or if there is, it is just due to "adjustment frictions"?!?!). They belong to Bob Solow's Napoleons' cited earlier. Even neglecting them, a good deal of the profession, after the initial surprise, is rapidly converging back to the propositions and policy advocacies derived from their old theoretical spectacles. And so one gets also the rosary of too familiar advices: "in order to increase employment, labour market in general and wage setting in particular have to be made more flexible ..." (as if unemployment were not a consequence of a worldwide aggregate demand); "the priority now is to balance the budget because only then growth will start again ..." (as if there were the slightest evidence of a crowding out between private investment and public expenditure, even after all the econometric cooking); "one should stop pumping liquidity into the economy because this will fuel a like in long-term interest rates and inflation ..." (when in fact, net of imported inflation of primary commodities, OECD countries are in the middle of a price *deflation*); etc.

Indeed, establishing a sound *theoretical* alternative is probably a necessary, even if not sufficient, condition for an alternative menu of policies.

In a short-hand, I would call it a program of *innovation-centred, environment-friendly, heavily redistributive, Keynesianism*. The ambition I trust shared by many is to "better understand the world in order to contribute to make it better". Needless to say, there is an enormous gap between elements of an alternative understanding of how the economic system works

(or doesn't) and a coherent ensemble of policy prescriptions. Of course, filling this gap is bound to be a huge collective enterprise. Let me just end this essay by flagging its urgency, in a historical moment when the scourge of misleading orthodoxies – much alike the early '30s – carries its sinister impact on the management of a crisis that they contributed to generate in the first place.

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