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Interpreting Economic Change: Evolution, Structures and Games

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1. Introduction

A century after Veblen's famous article complaining about the lack of evolutionary foundations of the economics discipline (Veblen, [1898]) the notion of evolution features as a central concept in several current analytical perspectives, although with different interpretations and uses. Sometimes it is barely distinguishable from the very notion of dynamics itself, *cum* some non-linearities and phase transitions. Elsewhere, it takes a meaning much nearer to that in biological theories involving selection, inheritance and variation. In yet other perspectives, it borrows only part of the biological view, significantly modified to suit the distinctive features of socio-economic processes.

It would be a futile exercise to try to review here what is actually meant by evolution in these different theories. Rather, in the following, we shall, first, sketch out what we consider to be the main results achieved so far by a few different approaches that invoke, one way or another, evolutionary explanations of economic phenomena, and second, map some overlaps and some differences among them that hint, in our view, at some major unresolved issues ahead. More specifically, we shall concisely consider (a) models and empirical studies broadly in a *post-Schumpeterian* perspective of the *genre* often ascribed to Nelson and Winter [1982]; (b) *evolutionary game* theories; (c) *organizational ecology* approaches; (d) *artificial economies*; and (e) part of that expanding literature on *adaptive learning* which is based on some evolutionary argument.

To set some sort of benchmark for the discussion below, let us start from what we deem to be a ‘maximalist’ vision of an evolutionary research programme.

2. Evolutionary theories: some building blocks ¹

For the purposes of this work, let us confine ourselves to some basic features of evolutionary theories of *economic* change. First, notwithstanding possible differences in other more substantive hypotheses, evolutionary theories share the methodological imperative “*dynamics first!*”. That is, 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, much better *both*). Or, 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”). Never take as a good ‘explanation’ either an existence theorem or a purely functionalist claim (entity *x* exists because it performs function *y*...). [Note, in this perspective that, Milton Friedman’s old “as...if”

¹ This section draws upon Coriat and Dosi [1998a]. More detailed discussion in germane perspectives are in Hodgson [1993], Dosi and Nelson [1994], Nelson [1995], Silverberg [1988], Silverberg and Verspagen [1995], Metcalfe [1992], Anderson [1988], Witt [1993], Dosi, Marengo and Fagiolo [1996], Lesourne [1991] and Lesourne and Orlean [1998].

interpretation of the properties of equilibrium behaviors (Friedman [1953]) should be taken as a (daring!) 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 (1982), although the dynamical intuitions are more understandable in the latter case.].

Given this general epistemological prescription (admittedly not an obvious one or even generally accepted amongst economists), the following substantive building blocks give shape to a full-fledged evolutionary research programme.

1. *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 typical agents do and why they do it. ²

2. *Realism* is a virtue and in certain respects a necessity. Although theories are necessarily abstract and admit less of reality than they omit, there are some broad features of reality that they 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.

3. Among these features is the fact that Agents have at best *imperfect understanding* of the environment they live in, and, even more so, of what the

² Note, however, that quite a few 'aggregate' (i.e. non-microfounded) dynamic models are nonetheless consistent with an evolutionary interpretation (some of them are surveyed in Silverberg and Verspagen [1995] and in Coriat and Dosi [1995]; see also the survey in a different perspective, by Boldrin [1988]).

future will deliver. Hence, “bounded rationality” is generally assumed, with its specific content varying with context.

4. Also, imperfect understanding and imperfect, path-dependent, learning entails persistent *heterogeneity* among agents, even when facing identical information and identical notional opportunities. Capturing heterogeneity is crucial to the representation of aggregate dynamics; a model without heterogeneity is like a flower garden without color.

5. The knowledge margin is always active: agents are always capable of discovering new technologies and ways of organizing, and adopting new behavioral patterns. Allowing for the *immanent possibility of novelty* in the system is a major theoretical and modeling challenge that cannot safely be ignored.

6. While (imperfect) adaptation and discovery generate variety (often in a seemingly random fashion), collective interactions within and outside markets operate as *selection mechanisms*, generating differential growth (and possibly also survival) of different entities that are the ‘carriers’ of diverse technologies, routines, strategies, etc.

7. As a result of all this, aggregate phenomena (e.g. regularities in the growth process or in industrial structures, etc.) are often captured theoretically as *emergent properties* -- the collective and largely unintentional outcome of *far-from-equilibrium micro interactions* and heterogeneous learning. Such properties often have a meta-stable nature, in the sense that while persisting on a time scale

longer than the processes generating them, they disappear ultimately with probability one.³

8. 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) action by the agents but also, partly, the unintentional outcome of the interplay of agent learning and collective interactions.

9. 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 properly to be modeled 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 short time scales. Modeling approaches that take these higher level 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 programme, as we see it. It is obviously impossible to review here the rapidly- growing literature of contributions that share some or

³ On the notions of the “emergence” and “metastability”, cf. the suggestive discussion in Lane [1993]; see also below.

all of the foregoing theoretical ‘building blocks’.⁴ We review here some of the major lines of research, achievements and distinctive features.

3. Evolutionary lines of research and main results: a brief overview.

Above we called one of the evolutionary-inspired research perspectives “post-Schumpeterian”, lacking a better word. In the following we shall refer to it, with some imperialism, , as “evolutionary theories of economic change” (ETEC), borrowing from the title of Nelson and Winter. ETEC do indeed share most of the foregoing ‘building blocks’, and on that ground aim at the interpretation of economic phenomena at different levels of aggregation - including industrial dynamics and macroeconomic growth.

For the sake of illustration, consider, in a nutshell, the ETEC story on the economic growth process. The story crucially involves a multitude of (heterogeneous) firms searching for more efficient techniques of production and better performing products, and competing in the markets for products, inputs and finance. Differential success in search, together with different behavioral and strategies (concerning e.g. pricing, investment, etc.) determine their differential revealed performances (in terms of e.g. their profitability, market shares, and survival probabilities) and hence their ability to grow. Aggregate growth, in this view, is - in a first approximation - driven by (partly endogenous and

⁴ Note that, given the above broad definition of an ‘evolutionary research programme’, it may well partly cover also contributions of authors who could not call themselves ‘evolutionist’ in any strict sense.

idiosyncratic) technological advances. Thus, the eye of the analyst is naturally drawn to the origins, nature and accessibility of technological *opportunities*; the ease with which firms can imitate each other (affecting *diffusion* and *appropriability* conditions); the ways firms are able to store and augment their knowledge (i.e. affecting *organizational routines* and their relationship to *competences*); and the mechanisms and speed of *market selection*. Finally note that such an evolutionary programme demands a complementary explanation of the key features of the environment faced by firms and individuals, especially the technological opportunities tapped by private agents; the legal framework shaping appropriability conditions; the origins of particular sets of corporate routines; the nature of product market interactions among rivals; the ways wages react to the changes in the demand for labor induced by technical change and growth; etc. In all of these respects, economic institutions play a key role: consider respectively university and government labs, patent laws and courts, trade associations and business schools, antitrust and regulatory authorities, labor laws and unions. It must be admitted, however, that the detailed exploration of these linkages has barely begun; for a discussion of the relationship between institutions and evolutionary processes, see Coriat and Dosi [1998a]).

There is indeed a flourishing family of formal models and historical interpretations constructed along these lines, construing economic growth as an evolutionary process propelled by technical change. After Nelson and Winter

[1982], see among others Dosi, Freeman, Nelson, Silverberg and Soete [1988], Day and Eliasson [1986], Silverberg and Verspagen [1994], Conlisk [1989], Chiaromonte and Dosi [1993], Silverberg and Soete [1993], Fagerberg [1994], and the discussion in Nelson [1995] and Silverberg and Verspagen [1995].

Relatedly, the diffusion of innovation has been analysed, from different angles, as a process highlighting at least some of the evolutionary, path-dependent, features outlined above (cf. among others David [1985] and [1992]; Silverberg et al [1988]; Arthur, Ermoliev and Kaniovski [1987]; Nakicenovic and Grübler [1992]; Metcalfe [1992] and [1995]).

The development of an evolutionary perspective has been deeply intertwined with the historical analysis of the processes by which technical change is generated, ranging from the microeconomic level all the way to 'national systems of innovation' (within an enormous literature, see Freeman [1982]; Dosi [1982]; David [1975], Rosemberg [1976] and [1982]; Basalla [1988]; Mokyr [1990]; Grandstrand [1994]; Vincenti [1990], Nelson [1993]; Lundvall [1992] and the reviews in Dosi [1988] and Freeman [1994]).

Finally, a growing number of industrial case studies and models of industrial change fits quite well the evolutionary conjectures outlined above (again, just as examples, see Pavitt [1984]; Utterback and Suarez [1992]; Klepper [1993]; Malerba and Orsenigo [1994]; Winter [1984]; Dosi et al. [1995]).

The properties of industrial dynamics have been addressed also from a partly overlapping perspective, namely *the ecology of organizational populations* (EOP), a literature stream based in sociology that has emphasized the processes of entry, social legitimization and mortality as drivers of industrial evolution, operating on changing populations of heterogeneous firms characterized by highly inertial organizational and behavioral traits (Hannan and Freeman [1989], Hannan and Carroll [1997], Carroll [1997]).

Notwithstanding the great variety in the objects of analysis, styles and hypotheses, it is still fair to say that most of the foregoing contributions share a distinct “phenomenological” flavour. They typically identify the empirical phenomena to be interpreted and then proceed with that task by telling a micro-founded dynamic story, wherein the assumptions on the microentities (principally the structures and behaviors of firms) and on their interactions draw a lot from empirical generalizations or reasonable conjectures specific to the problem at hand. So, for example, most of the ETEC and EOP works entail “boundedly rational” agents; but, given that, the specification of what agents actually do - whether they are characterized by behavioral routines, and, if so which ones; etc. - is highly context dependent, and often behaviorally quite rich (for example, the Nelson-Winter model(s) specify rules for technological search, investment, pricing, etc.). We shall come back below to some problems involved in this style of analysis.

A related but contrasting methodology inspires work in so-called *Artificial Economies* (AE) (Axtell and Epstein [1996] is an archetypical example) and is common to the broader class of Artificial Worlds (AW) models. In an insightful discussion, Lane characterizes such models as follows:

“Artificial worlds.....are computer-implementable stochastic models, which consist of a set of ‘microlevel entities’ that interact with each other and an ‘environment’ in prescribed ways. The aim of AW modelling is to discover whether (and under what conditions) histories [i.e. sample paths under well specified interaction rules and initial conditions] exhibit interesting *emergent properties*. An emergent property is a feature of a history that (1) can be described in terms of aggregate-level constructs, without reference to the attributes of specific microentities; (2) persists for time periods much greater than the time scale appropriate for describing the underlying micro-interactions; and (3) defies explanation by reduction to the superposition of ‘built in’ micro-properties of the AW”. (Lane [1993] pp. 90-91).

Lane (1993) reviews some examples from different disciplines, including Walter Fontana’s Function-Object Gas (Fontana [1992]) from chemistry ⁵, Christian Lindgren’s Evolutionary Prisoner’s Dilemma (Lindgren [1992]) and a would be Chiaromonte Dosi-Lane-Lippi-Pelkey-Tayler model of economic dynamics, of which a preliminary version was developed in the early 90’s at the Santa Fe Institute.

These and others models -- building upon Kauffman’s models of evolution and self organization: cf. Kauffman [1993]; Kauffman and MacReady [1995];

Levinthal [1997]; Frenken et al. [1999]; Westhoff et al. [1996] -- ambitiously begin to explore some possible *generic* conditions under which aggregate statistical regularities, or organized structures emerge, most often under far-from-equilibrium conditions.⁶

In the simplest cases, these regularities concern emerging patterns of behaviors and/or location in some spatial dimension (cf. Axtell and Epstein [1996], and for a presentation of some results in a language familiar to economists, Krugman [1996]), in others, they refer to properties of stylized ecologies or organizational forms (Kauffman [1993], Frenken et al. [1999], Levinthal [1997]). In terms of the ‘grand’ evolutionary research programme outlined earlier, the AE perspective emphasizes -- and indeed has contributed path-breaking insights into -- the notion of emergence in complex evolving systems. Moreover, it builds upon appropriate evolutionary microfoundations in that agents have, at best, a “local” knowledge of the environment in which they operate, and endogenously learn, or at least adjust, in ways possibly marked by systematic mistakes (from a hypothetical observer’s standpoint).

Growing out of quite different theoretical origins, a wide class of models of *Evolutionary Games* (EG) also share the general assumption of boundedly rational agents and a commitment to some underlying collective dynamics as a basic explanatory process, often driven by a selection mechanism at the

⁵ Further challenging developments are presented in Fontana and Buss [1994]

⁶ The debt to the works of Ilya Prigogine as a fundamental source of inspiration along this line of research ought to be more widely acknowledged: cf. , among the others, Nicolis and Prigogine [1977].

population level. The focal reference is Maynard Smith's notion of an Evolutionary Stable Strategy (ESS) - in fact a refinement of Nash equilibrium - developed with respect to the static properties of population ecologies in biology (Maynard Smith [1982]). Since then, that analytical perspective has been enriched by diverse dynamic developments which have begun to explore the limit properties of selection/adaptation processes - both in deterministic and stochastic settings - and trying also to reflect some abstract mutation-generating mechanism. For comprehensive presentations of the state-of-the-art at various dates, together with a few key advances -- see Hofbauer and Sigmund [1988], D. Friedman [1991], Vega-Redondo [1996], Weibull [1995], Samuelson [1997], Young [1998]).

Unlike the ETEC and EOP approaches, the broad Evolutionary-Game modeling enterprise -- at least within the economic discipline -- has been largely theory-driven, with comparatively low "phenomenological" discipline. Loosely speaking, EG appears to be the symmetric opposite (but sometimes a useful complement) to canonic Game Theory, at least in three respects. First it swings the microfoundations of strategic analysis the opposite way from the increasingly incredible forward-looking rationality postulated by what one could call the "ex-ante consistency view" of strategic interactions. Second, it tries to tackle the multiplicity-of-equilibria issue. This is a general unresolved problem for the "ex-ante" rationalists, but it is particularly acute in the plausible circumstances of repeated strategic interactions that lack any termination point

well perceived by the agents. In that respect, EG theorists attempt to identify some broadly plausible selection mechanism that picks out equilibria. Third, and relatedly, EG's address the issue of the stability of particular equilibrium concepts, in principle allowing for seemingly "irrational" micro perturbations or "mutations".

The foregoing bird-eye appreciation of the diverse perspectives that share some of our evolutionary building blocks might give the (misleading) impression of a comfortable convergence toward an increasingly coherent *weltanschauung* centered on common evolutionary ideas. This, however, is too optimistic a view. A more fruitful dialogue among the perspectives sketched above requires facing quite a few difficult and controversial issues concerning the basic nature of evolutionary dynamics in the socio-economic domain.

Let us briefly flag some of them.

4. Some open questions on structures, learning, games and other interaction patterns ⁷

Small worlds vs. open-ended dynamics. Evolutionary processes, in biology as well as in the social domain, clearly involve the emergence of novelty of various kinds along the evolutionary path. Although novelties are constrained to some extent by past history, there is still leave ample room for unexpected emergent

⁷ For broad discussions along similar lines, cf. Winter [1988], Dosi and Nelson [1994], Nelson [1995], Metcalfe [1992], Coriat and Dosi [1998a], Dosi, Marengo and Fagiolo [1995], Tordjman [1998].

phenomena and surprises. But capturing this in models presents tricky modeling challenges.

We believe that a full-fledged evolutionary dynamics would most likely entail an endogenous explosion of the dimensionality of the state space explored by the system. As the complexity of functions and traits increases in biological evolution from unicellular entities to mammals, so it happens also in the socio-economic arena.

One possible modeling strategy is to allow for a notionally infinite-dimensional space of search ⁸, without being able to fully specify the “law of motion” over that space, except locally. But what might be the analytical objectives in studying such a system? Clearly, there is by intended construction hardly any hope of finding some particular limit state where the system might end up -- such a result would spell the failure of the attempt to capture the persistent emergence of novelty. Under such open-ended dynamics, one might aim at best to find some emergent regularities in the process itself, concerning e.g. the properties of emerging, metastable structures, the temporal patterns of events - such as the “punctuation” of (quasi) equilibria and major structural discontinuities ⁹ -, average dynamic properties such as rates of growth of some variables, etc. This is indeed the philosophy of a good deal of AE’s and is shared by a few ETEC models.

⁸ Or, for computer-implementable models, finite for obvious technical reasons, but with a dimensionality high enough to practically simulate the absence of dimensionality limits.

⁹ E.g. Eldredge and Gould [1972]. See also Casti [1992] and the remarks in Lane [1993].

However, EG's are in this respect located at the opposite extreme: they generally assume some closed and well defined domain of exploration given from the start, allowing a rigorous characterization of the limit states of dynamical systems that represent mechanisms of adaptation and selection over a given fitness landscape.¹⁰

Ultimately, the appropriateness of the theoretical representation depends on the nature of the phenomena to be explained. The "small world" methodology is best suited to the interpretation of those properties of environments where the rates of adjustment to given "fundamentals" - e.g. technologies, organizational forms, etc. (that is to a given evolutionary landscape), is orders of magnitude greater than the rates of change in the fundamentals themselves (and thus in the selective environment). Given the rate of technological and organizational innovation in modern economies, suitable targets would seem to be rare.

Limit vs. 'transient' properties. Given an evolving system, what are the properties one primarily wishes to study? This issue overlaps with the previous point. Whenever one is pretty sure that adaptation is "fast" compared to innovation, one may assume that most empirical observations of the phenomenon at hand are close-to-equilibrium outcomes of the underlying adaptive dynamics -- hence it should be *descriptively* fruitful to study the properties of the limit states of the dynamical system in the model. Conversely,

¹⁰ But also some 'reduced' forms models developed in the ETEC spirit: cf. Winter et al. [1997]

when the foregoing condition does not apply it might be of greater interest to study the *transient* (“disequilibrium”) properties of the system, recognizing that in the reality to which the model relates, the fundamental parameters will be shocked well before the system reaches anything approaching an equilibrium. This indeed is what most ETEC and AE modelers do.¹¹

It seems to us that typical EG methodology too often subscribes cavalierly to the former assumption. And sometimes it does worse than that: it starts from some equilibrium notions which have ‘nice’ properties from the point of view of economic theory (for example because they correspond to microbehaviors that can be supported by standard “rationality” assumptions) and then searches for dynamical processes able to generate them. If the point is to understand reality, this style of intellectual production seems to us to be too roundabout. And, at bottom, it rests on unquestioning faith in the view that economic rationality is the only viable approach to understanding behavior in the specific case – as distinguished from the less doctrinaire view that the rationality assumption is sometimes a fruitful approach.

More to the point, however, a general issue concerns what can we learn from the characterization of the asymptotic properties of the system about its finite-time behaviors. In some cases, empirically interesting propositions may

¹¹ Without, however, any religious commitments to the methodology. So, for example, the ETEC perspective fully draws from those works on innovation diffusion where suggestive empirical implications may be derived from the limit properties of dynamics *cum* heterogeneous agents and increasing returns -- cf., among others, David [1988], Arthur, Ermoliev and Kaniovski [1987], Bassanini and Dosi [1998]).

be derived from the rates-of-convergence properties ¹² since the latter allow to infer, loosely speaking, how quickly the finite time (disequilibrium) observations should be expected to converge to something resembling the limit predictions. However, even assuming a qualitatively correct dynamic model, empirically relevant conclusions on this point cannot be derived from purely analytical studies of convergence; the quantitative magnitudes always matter. It is a point in favor of simulation modeling that it offers the opportunity to explore the quantitative convergence behavior of the model experimentally, drawing on rough evidence or intuitions concerning parameter magnitudes.

Interaction Dynamics and Selection A minimal requirement, common to different *genres* of the evolutionary approach, and shared with a vast class of models of decentralised economies ¹³, are microfoundations involving a large number of interacting agents. This requires the specification of a) the *levels* at which interaction occurs (e.g. does it concern product-market competition? behavioral imitation? mutual technological learning? etc.); b) the *mechanism of interaction* (who can do what to whom, when, and under what types of rules...); and, possibly, c) some *topology* over which agents are distributed, which may shape also the actual interaction process (for example, by making interaction probabilities dependent upon some distance in an appropriately defined space, etc.). As mentioned earlier, complete evolutionary models of economic change

¹² An example concerning the patterns of innovation diffusion is explored in Bassanini and Dosi [1998]; convergence properties in stochastic EG are discussed in Vega-Redondo [1996].

¹³ See the critical surveys in Kirman and Weisbuch [1998] and Fagiolo [1998], and the references therein.

ought to account explicitly for the joint dynamics of learning and selection. Ideally, one should specify the “physics” of interaction in both domains together with the ways they are dynamically coupled.

Actual practice typically remains well short of that ideal. In many models, either learning or selection is bracketed - which is not necessarily a bad intellectual strategy since it allows the exploration of the properties of single dynamic processes, holding the rest constant. So, for example, several models of stochastic, decentralized, adaptive economic agents - but without explicit market competition (cf. footnote 14) - offer very useful insights into the collective outcomes of mutual adjustments by boundedly rational agents. In a similar spirit, so do various models of innovation diffusion with heterogeneous agents, increasing returns and/or network externalities, and sequential adoptions.¹⁴ Within a distinctly different modeling style, most AE models also focus upon the collective properties of adaptive interactions (e.g. in the simplest cases some rudimentary forms of local learning). Nearer the ETEC philosophy, Dosi and Fagiolo [1998] study the properties of a reduced-form evolutionary model wherein, absent any form of market competition, the whole dynamics is driven by decentralized activities of technological exploration, learning and imitation over a notionally unbounded opportunity space.

At the opposite extreme, quite a few models explore the properties of selection dynamics driven by competitive interactions in the absence of any

explicit micro learning: cf. most EOP models. Within the ETEC perspective, among others Metcalfe [1992] and [1995] refines the Fisher Law, originally developed with reference to populations in biology ¹⁵, and insightfully applies it to industrial dynamics - while Winter et al. [1997] study generic properties of market selection fed by the persistent arrival of technologically heterogeneous entrants.

EG's extensively rely upon selection dynamics of some sort. However many models in this perspective appear to fold together properties of population dynamics and properties involving, at least implicitly, some form of adaptive learning at the level of microentities: we shall come back to this shortly.

As we see it, the basic shortcoming of most current modeling of interaction mechanisms does *not* concern the *ceteris paribus* assumptions typically made (to repeat, a possibly healthy style of preliminary exploration). Rather, our concerns primarily relate to the general lack of any disciplined mapping between the theoretical constructs on interaction processes and empirical "stylized facts" on those very processes.

¹⁴ More on them in David [1992] and Bassanini and Dosi [1998]

¹⁵ In brief, the Fisher Law establishes the *law of motion* of average "fitness" - and, thus, of the relative frequencies of the related traits - as a function of the higher moments of the distribution of the latter in the appropriate fitness space. In particular, the rate of increase of average fitness is proportional to its variance. Setting aside some important technical qualifications, one may regard it as a typical implication of the standard replicator dynamics common to theoretical biology, EG's in economics and many ETEC models: relative frequencies change with monotonic dependence upon the relative "fitness" of particular trait(s) as compared to the overall populations averages. Nelson and Winter [1982, pp. 240-43] establish a dynamic analogue of the standard comparative statics result for a price change in the in the context of the Fisher Law type of replicator dynamics. (For a germane critical discussion of these issues, see G. Silverberg [1988].)

Consider competitive selection dynamics. (We shall discuss learning processes below). Most of us have utilized on several occasions some “blackboxed” representation of an aggregate interaction law - be it an aggregate demand curve or a replicator dynamics of some sort. So, for example, in Nelson and Winter [1982] interactions in the product markets are compressed into an unmodeled market clearing process involving a standard demand curve. In turn, the outcome of that collective interaction feeds back upon the growth possibilities of each individual entity (and, thus operates as a selection mechanism) via the gross surplus each entity is able to invest - determined by the difference between the industry-wide price and firm-specific unit costs. Similarly, in models such as Silversberg et al. [1988], selection is explicitly modeled via a replicator dynamics, defining the “law of change” in market shares as a function of the “competitiveness” of each firm as compared to the industry average.

The point that we want to emphasize here is that it might be time to go beyond such “blackbox” assumptions and reconstruct the collective dynamics starting from an explicit account of the “microphysics” of market interactions ¹⁶. This would imply taking much more seriously the question of *how markets work*

¹⁷: e.g. what are the effects of the specific institutional architectures of particular

¹⁶ This indeed is a challenge that has been powerfully brought to evolutionary modeling by the AE methodology, with its abhorrence of pre-defined “laws of motion” and its emphasis on explicit rules for “local” interactions among micro entities; cf. also Fagiolo [1998].

¹⁷ One of the few noticeable works in this spirit is Kirman and Vignes [1991] on the Marseille fish market.

markets upon the ensuing collective dynamics? How do they affect selection processes? In what circumstances, is something like a replicator dynamics a good approximation of collective selection patterns? etc. A simulation environment for financial markets designed in this spirit is presented in Chiaromonte and Dosi [1998] and will soon be available on the Net.

A distinct, albeit overlapping, issue concerns the dimensions over which selection occurs. In the economic arena an obviously fundamental one has to do with output prices, but what about the non-price dimensions of competition?¹⁸ This is another domain where evolutionary modeling can be substantially enriched by empirical investigations into the actual determinants of competitiveness in different industries and under different institutional settings.

More generally, as argued at greater length in Dosi and Nelson [1994] and Tordjman [1998], analysis of selection in the social arena -- and its relationship to some notion of "fitness" -- must confront the question of the endogeneity of the selection criteria themselves. As in biological evolution, what is selected is likely to be determined in some complicated and non-linear way by the distributions of actual populations present at a point in time and by their history. However, in biological systems one might reasonably hold that the selection *criteria* (e.g. the reproduction abilities or the food processing efficiencies) remain relatively invariant. This might not be so in many socio-economic circumstances: that is,

¹⁸ Here one is primarily talking about real markets (i.e. markets for commodities and services): financial markets are likely to embody quite different selection criteria, much more related to speculative phenomena: more on this in Marengo and Tordjman [1996].

not only one is likely to find “dancing” fitness landscapes, but also the dimensions of the space over which such landscapes are defined are likely to change, too.¹⁹

On the usefulness of the phenotype/genotype distinction in the social domain A general feature of evolutionary interpretations of change (in biology but also in economics) is that selection ultimately operates upon a pool of ‘fundamental traits’ of some kind, determining also their probabilities of transmission over time. The common biological story is known: the ‘fundamental units’ of selection are the genes which (together with the environment) shape the phenotypic characteristics of the individual entities upon which selection operate; inheritance is the mechanism of transmission; and selection acts by “weeding out” phenotypic distributions, affecting over time the frequencies in the underlying genetic pool.

In fact, the real story is not so simple even in biology, and it is even less so in the social domain. For example, there might not exist a clear one-to-one mapping between the genes and their phenotypic manifestations affecting fitness (for example, due to fitness-neutral genetic drifts). Or, there might be *epistatic correlation* (for an in-depth discussion, cf. Kaufmann [1993]), so that different traits combine non-linearly to yield a particular phenotypic fitness.

¹⁹ Note incidentally that all this casts serious doubts on the monotonicity in the effects of selection forces generally assumed in EG’s.

Further peculiarities are specific to the socio-economic domain (see also Dosi and Nelson [1994], Winter [1984] and [1988]). First there is, *rightly*, much more ambiguity in the identification of what the “fundamental units” are. Good candidates - depending on the problem at hand - are routines, technologies, organizational forms, behavioral patterns, or even “mental models”. Second, technologies, behavioral patterns, etc. can be improved and modified over time in quite “Lamarckian” fashion. Third, “inheritance” takes diverse meanings. In some circumstances, the biological metaphor on inter-generational transmission is not too much off the mark. In others, inheritance should be understood mainly as “social imitation”. Yet, in others, there is no generation-to-generation “inheritance” but rather the indefinite perpetuation (and possibly growth over time) of the “phenotypic” expression of the underlying traits: this is the case of industrial evolution, where, at least in part, the frequency of particular technologies or organizational forms grow, shrink or die together with the organizational entities embodying them (see Winter [1990] for discussion).

Finally note that, in biology and even more so in economics, the objects over which selection is exerted are not single elementary traits but structures of much higher dimension in which traits are nested. So, for example, markets choose relatively complex products or technological systems -- not individual elements of technological knowledge or organizational routine. Therefore, even after assuming some underlying space of technological and organizational traits as the appropriate “primitive” dimension of evolution, one still needs some

interpretation of organizational development in order to relate “evolution” and “selection”. Putting it another way, one needs a much better grasp of the relationships between the “genotypic” and “phenotypic” level: we understand this is so in biology and it is certainly a priority in economics. This is also a major area of complementarity between evolutionary theories on the one hand, and business economics and experimental psychology, on the other. (See also below).

Here let us just mention two points. First, studies focused on “organizational routines” and “capabilities” begin to forge that link, building on an ETEC perspective and exploring e.g. the way organizational knowledge is stored and reproduced within organizations (cf. Dosi, Nelson and Winter [2000]), the nature and origins of routines themselves (Cohen et al. [1996], among others), etc. Second, note also that the genotype/phenotype problem has been largely neglected within the EGs perspective, at least so far. In fact the straightforward biological interpretation of an EG is as populations dynamics at the genetic level. Economic applications are more ambiguous, but one may reasonably understand them as defining dynamics on some metaphorically equivalent space of strategies (the “fundamental units of selection”). Here “selection”, which has - at least in principle - a clear meaning in biology as an environmental pressure toward/against the reproduction of particular traits, is taken to be equivalent to some payoff-driven pressure on the adaptive learning of boundedly rational agents (as L. Samuelson puts it, “I describe the forces

guiding agents' strategy choice as 'selection' or 'learning'. I use these terms interchangeably, though the former has a more biological flavor, while the latter is commonly used in economic contexts"; (Samuelson [1997], p.22). But in a way the 'learning' interpretation is like saying that there is some set of given strategies, and that agents 'climb' through them by means of some unspecified algorithm operating internally in each agent--which however yields, at least on the average in the population, results identical to genuine environmental selection operating upon different pure strategy types.²⁰ But ultimately this boils down to an assumption that, one way or another, the "selection landscape" in the heads of the agents is isomorphic to the "true" selective ecology in which the whole population lives.

By contrast, the distinction between selection and learning is highlighted within the ETEC perspective. Selection, to repeat has to do with what actually happens to agents (that is at the *phenotypic* level) in terms of markets shares, profitability, survival probability, etc. Learning, on the other hand, concerns what "goes on in their minds". Once one abandons the idea that the latter accurately mirrors the former, then the obvious question is: what and how do agents learn?

²⁰ "The agents in biological application of evolutionary game theory never choose strategic and never change their strategies. Instead, they are 'hard-wired' to play a particular strategy, which they do until they die. Variations in the mix of strategic within a population are caused by differing rates of reproduction. In economic applications, we have a different process in mind. The players in the game are people, who choose and may change their strategies. An evolutionary approach in economics begins with model of this strategy adjustment process (Samuelson [1997], p.18).

Learning in evolutionary environments This issue is clearly too broad to be adequately handled in these short, notes: see Dosi, Marengo and Fagiolo [1995] for a much more extensive discussion. The question here concerns how agents (individuals and organizations) make sense of complex and changing environments and how they act in them. Different strands of 'evolutionary' thinking certainly have in common the hypothesis of some 'bounds' on agents rationality. Generally, this should be taken just to mean that they are less omniscient than God (or than the local gods, the creators of the model at hand). Beyond that, there is not much agreement on where the 'bounds' are and what they imply.

Indeed, in Dosi et al. [1995], it is argued *first*, that agents (including of course ourselves) are not only characterized by information-processing and memory limitations, but more fundamentally, by an intrinsic *competence-gap* concerning a) the representation of the environment; b) the problem-solving repertoire; c) the detection and assessment of payoffs; and d) the very nature of goals and preferences. Learning affects all four domains, and their co-evolution.

Second, one is likely to obtain precious clues on such learning mechanisms by the observation of cognitive and behavioral patterns in circumstances that are not at all "evolutionary" -- the context is stationary and relatively simple, the payoffs are straightforward and the action menu is trivial -- wherein agents nevertheless display systematic biases vis-à-vis the prescriptions of "rational" decisions making, as experiments have shown.

More generally, *third*, the evidence both on individual patterns of cognition/decision behaviors and on regularities in organizational adjustments and learning should be considered as fundamental disciplining criteria for the micro-foundations of evolutionary models.

At this level, the difference among alternative perspectives is striking. At one extreme, ETEC models have attempted to represent agents reflecting certain “phenomenological” regularities - e.g. on pricing, R&D investment, etc. At the other extreme, EG modelers – while congenial to heroic simplifying assumptions in other respects -- have almost entirely eschewed the incorporation of stylized reflections of empirical considerations reported from experimental economic and cognitive sciences. Indeed, Camerer’s remark that “when game theory does aim to describe behavior, it often proceeds with a disturbingly low ratio of careful observation to theorizing” (Camerer [1997] p. 167) applies equally to its EG version. As a case in point, compare Fudenberg and Levine [1998] and Samuelson [1997], on the one hand, with the experimental evidence discussed in Camerer [1995] and [1997], on the other. And even more striking is the contrast between what modelers of industrial organization theorize (or prescribe?) and the evidence on actual corporate behaviors.²¹

As an illustration, consider the suggestive examples of interactions involving a strategic dimension presented in the pathbreaking work of Schelling

²¹ For some insightful remarks cf. the reviews by Loasby [1995] and Metcalfe [1995] of Milgrom and Roberts [1992].

[1978]. Certainly, the environments defining most of these “games” are not evolutionary in any specific sense: there is no dynamics in the “fundamentals;” the menu of actions is fixed, given from the start and fairly understandable to the agents; there is no replicator dynamics. If there is an evolutionary part to them, it is in the way people come to understand such environments and develop their action repertoires. Since Schelling’s contribution, what have we learned in terms of descriptive theories? (Agreed, we have learned a lot about the logic of structure and incentive of the interactions by re-stating them in formal game-theoretic terms.) What descriptive gains derive from postulating some variety of dynamics in the EG style? In fact, those dynamics point toward an implicit theory of adaptive learning. Whether it is a sound descriptive theory is indicated, presumably, by the empirical check from the behavioral and cognitive disciplines. For example, do people actually learn via mechanisms that entail the self-seeking reinforcements postulated by most EGs? Roughly speaking, the experimental answer seems to be negative. Rather, as is argued at greater length in Dosi et al. [1995], a more promising avenue of research appears to be an explicit account of the nature and dynamics of *mental models* and *interpretative categories* through which agents make sense of the environment they are facing and adjust to it. *A fortiori*, we suggest, this applies to evolutionary environments with the characteristics described earlier. But all that, in turn, demands descriptive theories of economic behaviors resting on empirically disciplined “nanofoundations”, taking on board the evidence from the growing literature on

the dynamics of cognition, “reasoning,” “sentiments,” etc., – on the individual level – and organizational behaviors – at collective levels.

5. Conclusion

In sum, there are multiple dimensions over which the different perspectives invoking evolutionary arguments may be evaluated.

A *first* one is the the degree of “rational,” forward-looking understanding of their decision environments that agents are assumed to possess, as compared to the objective complexity of their cognitive and problem-solving tasks.

A *second* relates to the nature of the evolutionary environments analyzed, and in particular whether the focus is upon “small-world” set-ups versus an “open-ended” dynamics allowing for various forms of endogenously generated novelty.

Third, at a methodological level, a crucial issue concerns the degree of phenomenological discipline to which models subscribe in such significant respects as agents’ cognitive abilities, learning mechanisms, behavioral rules, interaction format, etc.

And, finally, there is the question of what the various models are meant to interpret, and what testing criteria they set for themselves.

It is clear from this brief assessment of the state of the art that wide differences persist across all these dimensions. The hopeful message is that, at the least, all the reviewed perspectives subscribe to the “dynamics first”

commitments discussed above: interpreting change means telling, in one way or another, a process story rather than a “rationalization” of how what exist may coherently be accounted for. But this is certainly not enough to yield a consistent evolutionary interpretation of economic dynamics. The challenge ahead, as we see it, involves a painstaking reassessment of the microfoundations characterizing what agents do, how they learn, their interactions and the ways all that is embedded into institutional structures and “habits of thought” that shape the possible worlds achievable at any point in time. Certainly, for economists, that reassessment requires more attention to the wealth of evidence from other disciplines, from psychology to political science. The upside of such inquiry might be that the descriptive analysis of observed courses of individual and collective behavior is freed (at last) from the conceptual prison of a deductive, prescriptive theory of action.

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