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# LEM

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***A Very Reasonable Objective Still Beyond Our  
Reach: Economics as an Empirically  
Disciplined Social Science***

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# ***A Very Reasonable Objective Still Beyond Our Reach: Economics as an Empirically Disciplined Social Science***

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## **ABSTRACT**

The paper discusses some fundamental features of the 'Simonian' research program in microeconomics and compare them with two streams of thought which find their roots into Simon's pathbreaking work since the '50s and '60s, namely Transaction Cost Economics and Evolutionary Economics. One argues that the latter is in a particularly promising position to advance toward the kind of empirically disciplined microeconomics advocated by Herbert Simon. It does so also through painstaking attempts to operationalize the notion of 'bounded rationality' - in the broadest sense -, to make bridges with the microevidence from other social sciences - e.g. cognitive and social psychology, etc. -, and to "open up the organizational blackbox". And all that is undertaken outside the straightjacket of any religious commitment to equilibrium analysis.

## **KEY WORDS**

Herbert Simon; evolutionary economics; transaction cost economics; bounded rationality; organization.

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## (i) Introduction

It is well in tune with the example of Herb Simon to pick sound intellectual controversies irrespectively of prevailing academic doctrines, "self-evident" assumptions and entrenched prejudices.

I deeply believe that continuing to do so is a way of honoring his memory which he would have wholeheartedly encouraged. And this also what I shall try to do in the following.

My point of departure are his *Mattioli Lectures* [Simon (1997)]. They are a recent sharp summary of good parts of his *weltanschauung* in the domain of economics (as we know, he has been a major *maitre-à-penser* also in a few other disciplines...). And they entail the derived advantage of a somewhat "revisionist" critique by Ollie Williamson [Williamson (2000)]. I will still start from the latter and his succinct outline of the 'Carnegie research program', arguing (i) that a good deal of what in my view went wrong since the early Simon's years has to do with the prevailing 'ontology' of microeconomics agents, (ii) that an 'empirically disciplined microeconomics' of the sort advocated by Simon requires a radical departure from such a representation of the ultimate drivers of behaviors; and, that (iii) an evolutionary perspective is in a good position to undertake such a task.

Multiple, probably conflicting streams of research depart from the rich roots of Simon's Carnegie in the '50s and '60s. Indeed, historians of economic thought are likely to identify the Carnegie Institute of Technology (later Carnegie-Mellon University), as well as the Cowles Commission, as major springs of ideas and methods developed thereafter into the core of contemporary economics (for all good and all bad that goes with it).

Such springs were so rich that many alternative rivers could have been notionally alimented. In my view, it is a cruel irony that the major river which emerged took a course deeply at odds with the research program of the founders. But so it went, notwithstanding Herb Simon's unending determination to work, *first*, "for the 'hardening' of the social sciences, so that they will better equipped with the tools they need for their difficult research tasks" [Simon (1997), p. 196] and, together, *second*, for solid empirical groundings of purported microbehaviours, casual mechanisms, aggregate dynamics. Superficially, the former part went better than the most optimistic expectations half a century ago. In fact, things are a bit more complicated, and Simon kept screaming foul every time 'hardening' simply meant proving theorems without the slightest reference to empirical phenomena.

On the latter point, the picture is mixed. An impressive work has been done on "bottom-up" empirical analysis concerning e.g. individual and organisational behaviors, innovation and

technological change, industrial history, comparative institutional dynamics, etc. However, it is equally true that these diverse streams of empirical investigation and related theorizing did little to scratch the core of a paradigm which "omitted any serious testing of the validity of the assumptions of the model itself, even the kind of historical, experimental and anecdotal testing that we find in Smith and Marshall" [Simon (1997), p. 21].

Ultimately, such core of the neoclassical paradigm lingers upon the twin hypotheses (i) microeconomic rationality and (ii) collective equilibrium. True: many recent developments have tried to relax the former, but often they have done so in ways that would not pass any Simon-type check of empirical plausibility on how people actually behave, adapt, learn, etc.

At the same time, taken-for-granted assumptions on equilibrium most often continue to rest on the sheer faith in some unspecified process of adaptation and/or market selection.

This notwithstanding, ever simple exercises in (parsimonious!) "relaxation of rationality" together with a more explicit acknowledgement of institutions and organisations have yielded impressive fruits. One of them is the so-called 'New Institutional Economics' [for our purposes here cf. Williamson (2000) and (2000a)].

But is that the only possible river flowing from Simon's Carnegie springs? Certainly not. There is at least another, more radical, river originating there which is explicitly *evolutionary* - in the sense given to the word by Richard Nelson, Sidney Winter and many other likeminded scholars -, and *institutionalist* - although in a meaning broader than that implied by 'New Institutional Economics'. [cf. below. For more details I am forced to refer to Dosi (2000), especially chapter 23; ch. 11 co-authored with Nelson and ch. 12, co-authored with Benjamin Coriat, Dosi and Winter (2002), and to Nelson and Sampat (2001)].

In order to highlight overlappings, differences and subsequent developments I will follow part of the checklist of topics put forward in Williamson (2000) and compare the "Simon/Carnegie" Perspective (SCP), Transaction Cost Economics (TCE) - as such the central tenet of 'New Institutionalism' -, and what I shall call "Post-Simonian Evolutionary Economics" (PSEE). Williamson's points on TCE will be taken nearly verbatim, while I shall change every now and then according to my own views his reconstruction of SCP. The interpretation of PSEE is obviously my own.

In order to keep the controversy well alive, I will add scattered critical remarks on both orthodoxy and TCE. But I am sure that my friend Ollie Williamson will eagerly find a venue to reply.

Let me begin by focusing some central, *and diverse*, underlying hypotheses from the different theories on "human nature".

(ii) **Bounded Rationality**

One does not need to say much about the fundamental contribution of SCP in this area: "Bounded rationality, a rationality that is consistent with our knowledge of actual human choice behavior, assumes that the decision maker much search for alternatives, has egregiously incomplete and inaccurate knowledge about the consequences of actions and chooses actions that are expected to be satisfactory (attain targets while satisfying constraints)" [Simon (1997, p. 17)].

Conversely TCE takes on board 'bounded rationality' in much more homeopathic quantities and tends to reduce it to contract incompleteness [Williamson (2000), p. 14]. At the opposite extreme, PSEE fully subscribe the SCP perspective, and, if anything, it is even ready to abandon the notion of *procedural* rationality whenever the evidence on systematic cognitive biases warrants it [for a more elaborate argument along these lines, cf. Dosi, Marengo and Fagiolo (2002)]. Indeed, there is a sense in which the very term "bounded rationality" might be misleading insofar as it hints at some notional benchmarks of "full" rationality - whatever that means. But in fact it means very little outside the simple "closed and small worlds" (paraphrasing classic Savage), made of simple decisions over a finite menu of known choices, to which forerunners like Savage himself were careful to confine their axiomatics of "rational choice".

All this, of course, applies if one agrees, as I do, with Herb Simon's teaching that any *descriptive* theory of deliberation (or lack of it) and behavior ought to be robustly nested into corresponding empirical generalizations from e.g. cognitive psychology, experimental economics, organizational studies, sociology, etc. The other interpretation of rationality as an "objective" behavioral property of equilibrium - the so-called "as....if" assumption - has equally been target of the critiques of Simon and others, including prominently Sid Winter. In this spirit, just consider the following quotation:

“(T)he objections (against profit maximization) rest on the assumption, much stronger than any in biological Darwinism, that only profit maximizers can survive. Again, it is clear that the issue has to be decided by empirical inquiry. In the biologic world at least, many organisms survive that are not maximizers but that operate as far less that the highest empirical efficiency. Their survival is not threatened as long as no other organisms has evolved that can challenge the possession of their specific niches. Analogously, since there is no reason to suppose that every business firm is challenged by an optimally efficient competitor, survival only requires meeting the competition. In a system in which there are innumerable rents, of a long-term and short-term duration, even egregious sub-optimality may permit survival” (Simon, 1997a, p. 283)

Incidentally, note also that there is here a widespread misunderstanding claiming that "bounded rationality" is theoretically sloppy and undisciplined since "anything goes", the *adagio* being that "there is one way of doing things right and an infinite number of ways of doing them wrong". What

I think, in tune with Herb Simon, is quite the opposite: there is an infinite number of ways of setting up formally correct maximisation problems, just exploiting the degrees of freedom on functional forms and choice of the arguments that the theory allows, which have little to do with evidence at hand while there are very few, hard to find, empirically sound generations...

Let me also try to avoid here some possible misunderstandings. Mainstream discipline has indeed produced a great deal of empirical microeconomics, some of which is admirable at the technical level and some of which is quite informative. Indeed, the latter tends to be true in proportion as the authors let the data speak (under moderate theoretical guidance) and do not overly encumber the empirical analysis with hyper-rationality assumptions about the agents. Relatedly, in my view, the fictive characters of the assumptions about the agents roughly appears in three *genres* of models and empirical exercises.

A *first* class involves micro 'rationality' assumptions that are trivially common-sense and/or whose relaxation would in fact strengthen the qualitative conclusions of the model. On theoretical level, what comes immediately to mind are models *à la* Stiglitz and colleagues. Indeed, I conjecture that the qualitative properties of the conclusions - on e.g. the implications of asymmetric information, etc. - would *a fortiori* hold if the (relatively undemanding) rationality assumptions were loosen up [cf. also my introduction to Dosi (2000)]. Together, I don't have much of a substantive argument against empirical investigations whose 'microfoundations' ultimately entail propositions of the kind "it is better to be rich than poor", "it is better being healthy than sick", "one generally chooses, other things being equal, more profitable rather than less profitable courses of actions", etcetera, etcetera. I most often find irritating the ways these propositions are wrapped-up into an un-necessary baroque mathematical language (and too often by young scholars!), but let it be it: they are generally innocent *vis-à-vis* the empirical findings.

A *second* class of empirical investigations is *prima facie* much less 'innocent'. The canonic format is aiming at 'structural forms', possibly deriving reduced forms models and finally testing particular restrictions upon utility functions, productions functions and the like. As it happens, all these often more baroque efforts sometimes yield, *fortunately*, exercises not too different from the previous genre. That is, even if you left out the 'microfoundation efforts', the main qualitative results would nonetheless robustly hold. An example out of many: wouldn't most of Zvi Gliliches' findings on the empirics of innovation, patenting, etc. apply even without the jingles and bells of standard production functions, maximizations of expected returns, etc.? I think they would, and they would even better shine out as genuine, newly discovered, 'stylized facts'.

Conversely, think of many of the efforts that have gone into estimating utility functions consistent with micro consumption data. Here, a sort of 'pre-Copernican' epistemology comes fully into play:

as you bind yourself to never question the core max U (., ., ..) paradigm, you are driven to build as many epicycles as you need to accommodate the evidence.

In most respects, the *third* class of 'empirical' (?) analyses is the horror version of the former. An archetype that I like to quote is an article from the '80s, whose exact reference my unconscious repeatedly censored, set to show which type of utility functions American workers must have had in the '30s. After all, everyone knows (!) that (i) involuntary employment does *not* exist; (ii) people have rational forward-looking expectations; (iii) all empirical observations are equilibrium ones. Given that, it might have been quite rational for a good percentage of workers not to work, expecting the much higher wages after World War II (which of course everyone knew it was going to happen, with the Americans on the winning side...). But, then, the puzzle is why so many Americans enjoyed so little of their idle time, preferring e.g. to queue for a bowl of stinking soup rather than borrowing against their future income and have fun in Montecarlo...*(sic!)*

Of course, this would be a bad joke about psychiatric disorders, were it not taken seriously by a good part of our discipline (and indeed by far too many macroeconomists).

Notice, in any case, the historical change in the relative frequencies of academic contributions and relative academic respectabilities of the different *genres* over the last century of economic thought. Major contributors to economic theory in the 19<sup>th</sup>, early 20<sup>th</sup> century "orthodoxy", such as Marshall, deeply felt the challenge of "consonance with empirical data" as a criterion of theory evaluation. All that turned out to be increasingly rejected by later economic theorists as diverse as e.g. Samuelson and Hicks, among others, who saw microeconomic theory basically as a set of "tools" to be "applied" within a broader construction going from some axiomatic of micro behaviors to macro "laws of motions" and "predictions" (whatever that meant). At such a crucial deicide, around the 50's and 60's, just recall, amongst the most candid voices, Fritz Machlup claiming that microeconomic theory did *not* have anything to do with e.g. how firms went about making decisions, etc. and that genuine efforts ought to be put in "explaining away the data" in so far as they contradicted standard micro assumptions... In fact, things did not get much better since that time.

## **Motivations, self-interest and, more generally, the underlying anthropology**

Ultimately, I believe, this is one of the crucial points which make the rest of any social theory turn one way or the other.

In the Christian Middle Ages the dominant anthropology - powerful, elegant and totally shielded from refutation - meant interpreting the ultimate driver of behaviors as the endless fight between sin and grace, temptation and salvation... Nowadays, it seems, one of the prominent *weltanschauung* is the familiar one - equally general and equally shielded from refutation - grounded on ubiquitous self-interest as the basic motivational force.

The SCP painstakingly began to investigate a multiplicity of motivational drivers which certainly include, yes, self-seeking ones, but also altruism, sheer acceptance of authority, and "docility in its sense of teachability or educability" [Simon (1997), p. 41].

Indeed, interpreting all that as "frailty of motive"[Williamson (2000)] as TCE appears to do is, in my view, missing the point. Together, the emphasis on opportunism [i.e. "self-interest with guile": Williamson (1985)] places the anthropology of TCE rather too near orthodoxy for my personal tastes. Williamson puts it bluntly: "opportunism...takes us deep into the structure of contract and organization in ways that frailty of motive does not. Accordingly, even if frailty of motive describes day-to-day activity most of the time, candid reference to opportunism serves to uncover strategic issues that are ignored only at peril" [Williamson (2000), p. 15].

At the opposite extreme, the way I see, PSEE faces the urgent task of setting on firmer grounds its own breed of institutionalism whereby institutions themselves are a *quasi* - primitive concept of the theory rather than a derived one stemming from e.g. self-seeking rationality *cum* transaction cost considerations. After all, we are all born with a sort of "ontological dependency" on institutions - initially the family, and later also larger social groups -, we have all got our preferences and visions shaped by various authorities that we trusted or that were simply imposed upon us, like parents, teachers, etc.

Relatedly, "most of that we know, most of what we are able to do, is acquired from our social environments from the time of birth on into adulthood. Among the "facts" that we "know", or at least believe, there are very few that we have established ourselves and on the basis of evidence that we have ourselves and reasoning that we have carried out" [Simon (1997), p.40].

"Docility" and (possibly imperfect) adaptation entail both a motivational component - the goals of organizations/ institutions become at least in part our own - and a cognitive one - shaping the very identity of the actors. All this has led Jim March to suggest that humans a good deal of their time operate under a "logic of appropriateness" rather than a "logic of consequences". That is, our behaviors are driven by questions like "what would a good X (a father, a soldier, a priest, a

Taliban...) do in these circumstances?", rather than the more instrumental one "what would I gain according to the different courses of action?".

Of course, there is no clear-cut line between the domains of applicability of the two logics and in the overlappings always lingers the possibility of opportunism, "malicious strategizing", or even straightforwardly criminal behavior. Simply, I share with Herb Simon the skepticism about the notion that it is this shaded zone, where crooks and game theorists alike prosper, which justify the very existence of organizations (as opposed to "markets").

Two major points are worth emphasizing here.

*First*, following Sid Winter comments on an earlier draft, I fully accept to weaken the earlier statement on "institutions as primitives" to the wilder one on "institutions as *quasi*-primitives". As he forcefully pointed out, over the longer run, no entity is a true "primitive". All entities - including, of course, the writer and the readers of this paper, but also the institutions where we have all been nested - are "meta-stable" and bound to disappear with probability one as time goes to infinity (in most circumstances, well short than that!...), within a grand dynamical system where the candidates to be "primitive properties" might just be some properties of the long-run dynamic itself. Having acknowledged that, however, on shorter time scales - often corresponding to the scale of individual decisions, socio-economic interactions, institutional dynamics, etc. - the assumptions of institutions as *first approximation* primitives generally holds. (In order to illustrate all this with some caricature examples, I will turn to support of the opposite view entailing very 'plastic' institutions subject to easy historical change whenever I will suddenly observe e.g. the "representative" Afghan male respecting women's rights, modal voters of the current Italian Prime Minister paying taxes as a painful but dignifying duty, American protestants enjoying sex with innocent happiness...). In fact, over time scales that are typically longer than those over which most economic processes occur, a variety of institutions happen to be at the same time precious "social resources", path-dependent "carriers of history", and major determinants of individual and collective beliefs/"cognitive frames"/expectations...[Within an enormous literature, cf. Nelson and Sampat (2001), David (1994), Dosi (2000, chapter 23), March (1994)].

*Second*, let me stress that "rationality" (or lack of it) bears complicated, still mostly unexplained, links with "deliberation". Certainly, *procedural efficiency* in problem-solving often goes indeed together with *lack* of explicitly deliberation, with the development of *routinized action repertoires* and with the incremental refinements of routines themselves: cf. amongst others the analyses in Nelson and Winter (1982), Dosi and Egidi reproduced in Dosi (2000), Cohen et al. (1996) and Winter's chapter in this volume [Stan Metcalfe also signaled in his comments to an earlier draft of this paper the original contributions of G. Shackle on 'bounded' rationality as a serendipitous,

powerful, device to free us from the overwhelming tyranny of calculation, allowing humans to devote (at least in principle!) some of their time to search and creativity...].

Having said all that, I have no difficulty in admitting the persistent delays in operationalizing the foregoing ideas within PSEE.

However, I do believe, this is where one of the ultimate microfoundational challenges rest: and in its pursuit the conservatism of old researchers does not help in curbing the cowardice of most young ones...).<sup>1</sup>

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<sup>1</sup> Pernicious misunderstandings might stem from the overlapping appearance of diverse 'evolutionary' labels in economics and social sciences at large [more detailed discussions are in Dosi (2000) and Dosi and Winter (2002)]. For the purposes of this short work, let me just recall that "Post-Simonian Evolutionary Economics" (PSEE), as defined here, attempts to span well beyond the territory of so-called "evolutionary games" and indeed tries to take on board essential insights from cognitive and social psychology, experimental economics, organizational sciences, etc. on *how agents actually behave, learn, interact, etc...* .

Conversely it strikes me as rather bizarre that quite a few fashionable "evolutionary" approaches to socio-economic phenomena tend to entail rather Panglossian interpretations of the purported optimality of even the most awkward characters of human behaviors and cultures.

## Processes matter, in general, and selection processes, in particular

That “processes matter” is a statement about which, *prima facie*, all three SCP, TCE and PSEE agree. It is certainly a fundamental methodological maxim of PSEE: as one argues in Dosi and Winter (2002), if you need to explain why variable  $x$  has the value that it has some time  $t$ , tell a convincing dynamic story on how it got the value it has; if you need to explain why entity  $a$  became what it is, likewise, reconstruct the process through which it emerged and, possibly, how it was selected against competing entities...

So, it would also be fascinating to obtain particular organizational forms as results of explicit selection processes involving different governance arrangements, etc. (No matter whether the story is told in qualitative or formal modes). And, intuition suggests, this ought to be a field where TCE and PSEE have overlappings and synergies. Why is then that one find relatively few explicit *process stories*, even within TCE (let alone the rest of mainstream economics)?

My conjecture is that even TCE is too afraid of "rugged" selection landscape, endogeneity of selection criteria, multiple equilibria, path-dependencies, lock-ins, etc. Conversely, PSEE is generally inclined to assume that where the process will end-up is neither clear to the agent nor to the analyst before one runs it through. On the contrary, TSE makes a lot of the foresight abilities of the agents. Indeed, if processes can be fully run *ex ante* through the heads of the agents (and agents have the *same* true models-of-the-world, etc. etc.) you will never observe actual selection processes, since everyone will instantaneously converge to "rational expectation" equilibria of some kind. Certainly, TCE and PSEE agree that (at most!) selection operates in favour of some available (or feasible?) *fitter*, rather than an unconditional *fittest*, entities but the argument loses strength the more unbiased foresight one puts into the head of the agents themselves.

## (V) Foresight

One immediately detects here the same Carnegie root but two opposite branches. Very few social scientists, I believe, do not acknowledge the importance of motives and expectations (*right or wrong*) for the understanding of what individuals and organizations actually do. Hence also Herb Simon's repeated urge, since long time ago, to understand and model what goes on "in a people's heads". As it happened, one answer to that invitation has been that "... of course in people's head runs the true model of the economy, precisely the same as the one I am presenting here in the model that follows...". Conversely, a major disadvantage of those scientists searching for a different answer has been that they began to investigate what was *actually* going on in heads of people or in the metaphorical heads of organizations. And the search is still going on, slowly, with a lot of dead-

ends, often under the scorn by respectable theorists who know the answer even before looking...<sup>2</sup>

In which camp does TCE belong?

In my view, quite a few of TCE ideas are likely to withhold investigations of the genuine "Simonesque" kind. Relatedly, I cannot fully understand why TCE tends to overlook possible bridges with PSEE at both levels of analysis of micro cognition/behaviors, etc. (i.e. where units of analysis such as organizational routines rest) and of market-driven selection dynamics amongst competing organizational forms.

By the same token, I find quite dangerous the use of notions like that of (ir)*remediability* (... "If you observe a dominant organizational form, trust that it is the best *feasible* one. Otherwise, be sure someone would have seized the opportunity to construct more efficient governance alignment..."). For my tastes, it is a notion far too near the Panglossian spirit of Chicago and surroundings, against which Herb Simon restlessly fought: we do not live in the best possible world, and it might not be in the best use of our scientific abilities to assume that we do.

But let me go back to the issues of forecasts and expectations. Herb Simon was amongst the first to acknowledge the formidable tasks involved in the identification of a reasonable alternative to "rational" expectations, while fully aware of the crudeness of simple adaptive ones. I never asked him which of the extremes he would have chosen at gun-point. However, I believe that with a lot of qualifications he would have chosen the latter. This, of course, not because he believed that people were actually using e.g. devices like moving averages between yesterday and the day before in order to predict tomorrow, but rather as part of a general acknowledgement of the inertial features of our cognitive frames and information processing systems. And, indeed, the empirical properties of the latter turned out to be another field where Herb Simon gave pathbreaking contributions.

Interestingly, Simon (1997) emphasizes also the pioneering works of George Katona, undertaken as early as the 40's, 50's and 60's, painstakingly trying to identify empirical regularities in micro behavioral patterns (concerning e.g. consumption, saving, etc.) and expectation formation. It is a type of investigation that we should re-discover and refine, with a "bottom-up" respect for the evidence in tune with Katona's original investigations.

More generally, I do believe that an increasingly urgent task regards a much more systematic understanding of the links between evolutionary microfoundations and aggregate ("macro") regularities. In turn, all this requires also the identification of a few "stylized facts" on modal

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<sup>2</sup> I want to emphasize here my nearly total ignorance of the early cultural and sociological history of the interaction with between Herb Simon, Muth, Lucas, etc. Historians of economic analysis are much more suited to the task. The only point I want to make is that, for sure, Herb Simon has been a major driver urging at least a couple of generation of economists to "take microfoundations seriously" including, of course, expectation formation.

behaviors regarding e.g. corporate pricing and investments, consumers' patterns of demand, intertemporal choices, etc.

## (V) Some conclusions

There are two major components, in my view, to the persisting lag between the 'empirically based microeconomics' advocated by Herb Simon, on one hand, and the achievement of the current state-of-the-art in economics, on the other, - *even* in not-too-mainstream perspectives, such as Transaction Cost Economics (and the largely overlapping New Institutional Economics).

The *first* component is either a stubborn commitment to some postulated principle of microrationality, or, to the same effect, massive departures from it which however support processes whereby in equilibrium agents behave *as...if* they were fully "rational" (cf. the widespread use in this vein of many evolutionary games models).

Together, *second*, all this goes hand-in-hand with a dramatic underinvestment in humble, albeit theory-inspired, investigations into *what people and organizations actually do*, through a variety of methods, bridging across disciplinary boundaries to e.g. cognitive and social psychology, organization sciences, etc.

I have also advocated above that 'Post-Simonian Evolutionary Economics' might be quite apt to face the challenges set out by Herb Simon. It does so in so far as, *first*, it embodies an 'anthropology' quite richer than the running archetype of *homo economicus*; *second*, it is fully committed to *process stories* about individual and collective learning, innovation and adaptation; and, *third*, it is genuinely committed to the empirics of cognition, decision-making and behaviors rather than 'armchair axiomatics'.

Whether such a research program will succeed only time will tell. In any case, I can hardly accept a memory of Herb Simon just as a purported precursor of 'quasi-rational' adaptation models. Rather, I prefer to hold him as a gigantic intellectual figure who relentlessly pursued scientific reason in the analysis of human behaviors and social organizations in an epoch where theorists found it particularly difficult to distant themselves from clumsy introspections of greedy shopkeepers (with no offence for the many good-souled shopkeepers!).

By the same token I hold with a lot of pride that, few years ago, I and a couple of co-authors had a paper on industrial dynamics rejected by *Econometrica* with one of the referees suggesting that publication *there or anywhere else* would have "brought back the discussion of industrial change to the Dark Ages of Herbert Simon" (*sic!*). For sure, the integrity in the pursuit of knowledge has its costs: in any case, also on this ground, Herb Simon has been an example of total indifference to fashions and academic conformism.

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