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WHY ECONOMISTS ARE A BIT MORE IMPORTANT THAN GARBAGEMEN

Few economists have the breadth of knowledge and wisdom necessary to write meaningfully about the economics profession. Melvin Reder is one of those few. He is an Emeritus Professor at the University of Chicago; his previous work includes an honest and insightful look at the Chicago school of economics along with some solid work in labor economics. In this book Reder turns his insightful eyes upon the economics profession as a whole. The result is not all that pretty. Indeed, of the four parts of the book, the first three are largely a negative assessment of the current state of economics. Reder recognizes this and, in chapter 11, writes:

So far the thrust of this book has been negative. It has emphasized the limitations and shortcomings of economics so strongly that anyone persuaded by its arguments might well ask why society at large pays heed to those whose claim to attention is based on its mastery.

(p. 263)

This largely negative assessment is not a slam job on the profession. Reder’s views are not those of Robert Kuttner or John Cassidy. He likes the economics profession; the assessment is more that of an elderly father (whose training is in economics, and thus he can be nothing but reasonable) describing to a close friend his progeny – progeny who has come home with tattoos and a pierced body: He loves the bum, but is not completely happy with how he has turned out. There is some, but not too much, of ‘Why couldn’t he have been more like we were’ in it.

It is unclear to me precisely for whom the book was written. Some of the
chapters seem written for economists; others seem written for the broader public. On the whole, I’d say the book is mainly appropriate for introspective economists, which I hope is not a null set. One thing that puzzled me about the book was the subtitle, ‘The Culture of a Controversial Science.’ Just the fact that an economist would use the term culture seemed unusual. Perhaps my puzzlement is due to the fact that I’m not sure what ‘culture’ means. (In one book I wrote, I offered thirteen definitions for the term because I didn’t know what it meant.) Essentially, what I’ve come to believe is that ‘culture’ in a title is a key to saying that the author is taking an anthropological approach to his subject. But in this case that isn’t true. Only occasionally does Reder give us anthropological jargon. There were times, however. Consider:

Like many other academic disciplines, economics is the product of a particular culture whose members constitute a profession that is coextensive with the domain of the culture. Such professional cultures are characterized by (1) a matrix of professional associations; (2) culturally specific media of communications (e.g., specialized journals).

(pp. 3–4)

Luckily such jargon reared its head only rarely, and can probably be attributed to an overenthusiastic editor; such phrases don’t come out of real economists’ mouths, and Reder is a real economist.

The first chapters are written for lay people and new economists; they cover what economists are, what they believe, and such things. They provide the ‘big picture’ approach to economics, and in doing so will be a useful summary of the state of the profession for those interested in topics like: what we do, and how we do it. For the most part it was a good summary. Let me outline the arguments in these chapters.

Chapter 1 is an overview of the book; it states its purpose and what it will cover, and gives various strategies for reading the book. Chapter 2 discusses economics in relation to the other sciences. In it we can see the generally negative assessment of economics that runs through the first ten chapters. For example, he writes:

A major theme of this book is that, although economists generally recognize the validity of the criteria of prediction and control as standards for judging the performance of a science and strive to satisfy these standards, thus far their success in doing so has been quite limited. As the Economist – a bit uncharitably – put it, ‘an economist, it is said, is an expert who will know tomorrow why the things he predicted yesterday did not happen today.’

(pp. 22–23)

In it he offers a jigsaw puzzle metaphor to explain how economics is approached and taught as compared to other sciences. He writes:
In the natural sciences, where students are taught by workbooks and salient examples, paradigms are allowed to emerge from the process of solving problems rather than inculcated as ‘principles.’ Typically, the reverse is the case in economics, where the student is drilled in the principles of the subject with the applications introduced as illustrations.

(p. 37)

Chapter 3 discusses the resource allocation paradigm (the RAP) that is captured in Lionel Robbins’ definition of economics – the ‘allocation of scarce resources among alternative uses for the maximization of want satisfactions.’ The RAP is, of course, close to the heart of any Chicago economist, where the RAP is often viewed as a religion. For those of us outside of Chicago, it is a secular view; the RAP is a useful and sometimes very useful tool to think about certain problems. But it must be seen as a tool, and placed in a larger context. I suspect Reder sees less of the secular view of the RAP than I do, which means I would have told the story slightly differently but there are enough strong believers so that he did not do any great injustice in his presentation.

Chapter 4 discusses the Keynesian paradigm, and reads a bit like a textbook. He reviews the consumption function and IS/LM analysis, concluding, I believe correctly that ‘it is the change of analytical framework that KP [Keynesian Paradigm] introduced that has been crucial to its influence on the culture of economics’ (p. 91). Still, I think he missed some of the deeper themes of the Keynesian paradigm – how there were many possible avenues it could have taken, and why the one it took was, politically, the easiest. The chosen policy implications were those that affected the government budget, not investment generally. Its analytic implications were quickly eliminated. By the 1960s Keynesian economics was changed from an approach that challenged broad issues to an approach that could easily be incorporated into the RAP. Nonetheless, for a Chicago economist, perhaps because he was a student of Lerner’s, it’s a reasonably fair discussion and it gives more credence to Keynesianism than I think the majority of economists today would give.

Chapter 5 contrasts the Keynesian and the resource allocation paradigm views of debt and taxes (a strange topic for a book about the culture of economics, but then the book really isn’t about the culture of economics). In it he quite rightly contrasts Lerner’s functional finance approach with the RAP approach. I loved his characterization of Keynesian economic policies as ‘attempts to snatch a sandwich.’ He correctly points out that ‘RAP adherents deplore such attempts’ to snatch, and that their arguments why it can’t be done vary between ‘outright denial of the existence of the sandwich and concern that attempts to eat it will prove counterproductive’ (p. 107).

Chapter 6 discusses some other paradigms including: the classical distribution paradigm; Marxian economics; the beyond rationality paradigm; Austrian economics; and feminist economics. He justifies this discussion by saying he has provided it:
so that the reader may better comprehend the roots of the methodologically tinged debates that keep recurring among economists. These debates, whose issues evade resolution by the approved procedures of science, cast doubt on the scientific status of the subject of economics and undermine the claims to expertise of its practitioners.

(pp. 140–141)

I found this chapter one of the least fulfilling; in my view it does not give a good sense of alternative views. Public choice is not covered, nor is post-Keynesian economics, and those schools that are covered are covered superficially, although he is definitely attempting to be fair in his presentation. I suspect that this weakness is a reflection of the fact that Reder is an insider, not an outsider, and his views of the other paradigms are from the outside, whereas his views of the profession are from the inside.

Indeed, much of what he describes as heterodox is, in my view, part of a broader resource allocation paradigm. (It’s the outside of the inside.) Again, that different view, I suspect, comes from our geographic placement – his in Chicago, mine in Vermont, closer to the Boston/New York/New Haven nexus. Still, his conclusion seems sound and honest, and I think he does a good job of casting doubt on the scientific status of the subject. My view, actually, would have cast less doubt, but would not have elevated science so high.

Chapter 7 discusses the criteria of validity in economics. Here he recounts the ‘failure of economics to satisfy the criteria of facilitating prediction and/or control.’ He attributes this failure to the greater complexity of the subject, but asserts that:

Although there are many examples of . . . [ideological behavior,] economists who command wide respect within the profession typically give great weight to empirical evidence in appraising the validity of propositions whenever such evidence is generally perceived to be relevant and unambiguous.

(p. 174)

The reality is, of course, that very little of the empirical evidence is perceived as both relevant and unambiguous, so in my view, the assessment is a bit more positive than is warranted – a reflection of the father viewing the actions of his offspring in a good light.

Chapter 8 discusses two examples of the ‘successes’ of positive economics – one dealing with labor economics and one dealing with finance (the quotation marks around successes are his). His conclusion about the success of labor economics is somewhat limited. He writes ‘I fear that the overall tone of the discussion in this first part of the chapter may seem to deprecate the achievements of labor economics during the past 50 years’ (p. 196). His second example of ‘success’ – finance – is much more upbeat, but he states that with only a few exceptions, of which finance is one, almost the entire body
of economics ‘is either vacuous or unsusceptible of confirmation by empirical testing’ (p. 207). Rather damning praise from an insider.

Chapter 9 deals with welfare economics, which is the application of the resource allocation paradigm to policy. After reviewing the central elements of welfare economics he concludes, I believe correctly, that for welfare economics to be meaningful, it is necessary to place it in the context of the state. How economists do this is discussed in chapter 10. Again, I think this discussion is influenced by his Chicago perspective. From the secular perspective there is less need to dwell on the fact that one can support both RAP and active government intervention; it is obvious. I think RAP rules out certain types of action more than others. However, the reasons for deviating from RAP were not clearly stated by Reder because he was interested in showing that it is reasonable to deviate. Still the general conclusion of both these chapters – that no policy conclusion directly follows from the resource allocation paradigm – are reasonable. The RAP is a way of framing issues.

After dumping on economics for ten chapters, his fatherly instincts come out and he devotes the last four chapters to placing the best face on economics and building up the morale of his progeny. He calls chapter 11 ‘What is Good Economics?’ In it he offers four justifications for economics. They are:

1. Economists construct and interpret certain important institutional facts. As examples of what he means he gives developing and keeping up the cost-of-living index and the dating of business cycles.
2. Economists do quasi forecasting, which he defines as short-term forecasting of aggregate quantities such as aggregate expenditures on plant and equipment.
3. Economists construct beliefs about ‘how the world works.’ He includes in this category information gathering, presenting the lessons of history, providing ideological support, and the teaching of economics.
4. Economists play a role in creating institutions. Examples include the IMF and the World Bank.

He is, of course, correct; each of these is an important role that economists play. But what’s interesting about these roles, from my perspective, is that they are not the roles that graduate school teaches economists to play. In graduate school they are taught to play the role of theoretical researcher. They are given little background on the technicalities of price indices, the type of dirty forecasting the business economists do, the teaching of the subject of economics to undergraduates or the general public, or the politics that play such a central role in the creation of any institution. Let me take just one aspect that is dear to my heart. In a couple of places he states that teaching undergraduates is one of the most important roles that economists play. What qualifications would one want a good teacher of undergraduates to have? One is that they have a broader sense of the field so they can place ideas in context. But few graduate schools offer such training. Generally, they offer specific training that reflects the
narrow research interests of their graduate teacher. They have training in how
to be a cog in a research machine; they don’t have training in how economics
fits into the lives of undergraduates.

Reder’s next chapter deals with the question, ‘What is good economics?’
He answers this question with a corollary to Viner’s definition of economics.
Since economics is what economists do, good economics is what good econ-
omists do. He admits that this raises the obvious question: who decides what is
a good economist. He answers this question in the following way: good
economics is what recognized economists say it is, whenever they speak with
one voice. The problem, of course, is that economists seldom speak with one
voice, so there is generally ambiguity about what good economics is. In dis-
cussing the issue, he gives three headings under which judgments are made –
validity, importance, and intuitive appeal.

In his discussion of validity he admits that there is ‘a relatively low valu-
ation of efforts to improve the accuracy of the subject’s database,’ and writes
the following as a rationale:

In economics we have no prospect of being able to devise theories that
can satisfy both the requirements of one or another of our paradigms and
yet account for an appreciable part of the variety of empirical phenomena
that such theories are called upon to explain. Improving the quality of the
data is not likely to improve this situation: indeed, it might be that better
data would reveal even greater complexities of behavior than are yet
recognized, thereby adding to the burden placed upon theory. Conse-
quently, let us get the ‘gross facts’ as straight as we can, but not worry
too much about their details. Instead, let us focus attention on the con-
struction of relatively simple theories that have some prospect of accounting
for the gross facts while remaining valid regardless of how the details of
these facts turn out.

(pp. 297–8)

In the discussion he states that the obverse side of economics culture’s
rather relaxed attitude toward matters of empirical detail is its close attentions
to fine points in logic. This tendency is the reason for my, and that of many
critics of economics argument that much of economics gets lost in the logic
and never asks what ‘big’ is. An infinite number of logical models can be
constructed saying just about anything. The question is: which are important,
and which aren’t?

In describing important work he suggests that the origination of a paradigm
is the most important and concept alteration is the next most important. As
examples of concept alteration he cites the works of Piero Sraffa’s, Albert
Hirschman’s and Kenneth Arrow. After concept alteration, discovering a
structural characteristic of the economy is the next most important contrib-
ution. As examples he gives the discovery of the permanent income
hypothesis, the Phillips curve, and the work on technological progress and
growth accounting. Next in the importance hierarchy comes establishing facts and here he includes the work of Kusnetts and Fogel and Engerman.

In determining what is important he states that it is intuitive appeal, although what has intuitive appeal often differs from inside and outside the profession. He gives as examples of good non-technical economics: A.A. Berle’s and G. Means’ The Modern Corporation and Private Property; Keynes’ The Economic Consequences of the Peace; and Gunnar Myrdal’s An American Dilemma. He concludes the discussion reasonably stating ‘One of the main lessons of this chapter and its predecessor is that in economics there is no simple relation between the good and the useful’ (p. 320).

The last two chapters of the book deal with prizes, establishments and heroes and with the boundaries of economics. In these he carries through his discussion of what is good economics by determining who wins the prizes, and how economists expand economic ideas into other fields.

It did not surprise me that I, the author of Why Aren’t Economists as Important as Garbagemen?, was asked by a number of journals to review this piece. This book could well have been entitled Why Are Economists a Bit More Important than Garbagemen? As I have stated throughout this review I am, in large part, in agreement with Reder’s assessment. It is an honest and useful reflection on the economics profession. He is somewhat more accepting than I of some of economics’ foibles: he reports; I try to raise a little bit of rabble; but our views are much the same.

Seen together with Tom Mayer’s book Truth vs. Precision, and Robert Solow’s article ‘How did economics get this way, and what way is it?’ Reder’s book provides the older generations of economists, consideration of where the younger generation has taken the profession. It is one of concern, but acceptance. They would certainly like it if new economists were a little more like they were, but they will put up with the earrings and body piercing because, after all, their progeny is a chip off the old block.

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THEORETICAL ANALYSES OF BOUNDED RATIONALITY AND LEARNING

Standard microeconomic theory is silent on the procedures that economic agents use in making decisions: the rational economic agent is assumed to have a consistent set of preferences and he simply chooses the optimal action,
subject to the constraints that he faces; in interaction with other such agents, each one successfully deduces what the others will do and an equilibrium results. The uncovering of more and more instances in which the traditional rational actor model predicts actual human behavior rather badly has increased dissatisfaction with the standard model and has stimulated the development of alternative models of choice. As game theoretic models push the perfect rationality assumption to its limits, the limitations have been most visible in that context and alternatives have naturally also been developed there. During 1998, two books appeared with MIT Press: Ariel Rubinstein’s ‘Modeling bounded rationality’ and Drew Fudenberg and David Levine’s ‘Theory of learning in games’ that together provide a good overview of recent work in this area.

The two books are complementary with Rubinstein covering a broader area and Fudenberg/Levine going more into depth on the specific issue of learning in repeated games. The advanced text of Fudenberg/Levine investigates whether learning processes will produce the same type of equilibria as traditional models that assume that players in a game can perfectly predict each other’s actions. It will mainly be of interest to researchers in game theory and related fields. The book by Rubinstein, being less specialist and providing material for a one-term graduate course, may be attractive for a wider audience. Rubinstein discusses a collection of models, each one incorporating a specific procedural element of choice making. He deliberately does not touch upon the growing literature that deals with evolutionary and learning models, the main arguments being that that topic deserves a book of its own, that it uses different mathematical tools and that, in the standard literature on evolution and learning, there is little room for deliberations about decisions, rather choice arises by applying a mechanical rule. Fudenberg and Levine’s focus is precisely on these learning models. We now discuss both books in turn.

In his 1950 Ph.D thesis John Nash already argued that the Nash equilibrium concept can be interpreted in two completely different ways, viz. as an outcome of deliberation and as outcome of learning. The game theoretic literature since then has mainly relied on Nash’s first, rationalistic, interpretation. In this interpretation, players are perfectly rational and can predict correctly what their opponents will do; an equilibrium is a situation in which, given these correct predictions, each player maximizes his payoff. Nash’s second ‘mass-action’ interpretation involves the game being repeatedly played over time with players being boundedly rational. These players observe the outcomes through time, learn and gradually adjust their behavior. Nash claimed that if behavior converges it must converge to a Nash equilibrium. In their book, Fudenberg and Levine investigate in what contexts the original intuition of Nash can be confirmed. The book thus addresses two main questions: (i) when and why should we expect play to converge to a Nash equilibrium? and (ii) if the game has multiple Nash equilibria, which ones should we expect to observe? In addition, it addresses such issues as how long will it take before players
have learned to play an equilibrium. It, however, does not extensively address the question of what we might expect when behavior does not settle down.

The Fudenberg/Levine book makes a useful distinction between learning models and evolutionary models. In the former class, learning is modeled at the level of the individual; there is an explicit model about how an individual incorporates new information and how the adjusted state leads to a new action. An example, that plays an important role throughout the book, is fictitious play in which agents always play a best response against the empirical distribution observed in the past. Hence, if an individual has seen that his opponent has played seven times T and three times B, he will believe that in the next period T will occur with 70 per cent probability and B with 30 per cent. This process can be viewed as a Bayesian learning process. The second class consists of models of ‘social learning’. Here the adjustment is described directly at the aggregate level of the population without going into details about how such changes result from changes at the micro level. The question, of course, is why such a dynamic would be interesting for economic settings. As the authors show, such processes might result when, at the micro level, agents learn by asking around and by imitating other successful agents, or when there is learning until a certain aspiration payoff has been reached.

The first part of the book is devoted to normal form games. Chapter 2 deals with fictitious play. The basic result is that, if the empirical distributions of actions converge, the product of these distributions must be a Nash equilibrium. While this is a positive result, the authors point out its limitations: actual play need not converge (it may cycle), nor need the payoffs converge to the equilibrium payoffs. Furthermore, because players base themselves on the same history, correlations and correlated equilibria arise naturally. Hence, a first conclusion is that Nash’s original intuition cannot be sustained in general. Chapter 3 then reviews the results that are available for the ‘replicator equation’ and for related models with monotone dynamics, i.e. actions that do better spread more quickly through the population. Again, stable steady states of these systems must be Nash equilibria. Chapter 4 discusses models with a persistent stochastic component, this randomness, for example, being caused by mutations or errors in decision making. The book reviews the models of Kandori, Mailath, Rob, Young and Ellison that establish equilibrium selection results, showing specifically that in the long run, the system will end up in the risk dominant equilibrium.

The second part of the book investigates the same set of issues, but now in the context of extensive form games. The important insight is that, when players only learn the actual outcomes of play, they need not have common expectations about behavior of the equilibrium path, hence, that outcomes need not be Nash equilibria. (The Nash equilibrium concept insists on common beliefs, both on and off the equilibrium path.) Nash equilibria are obtained only if there is sufficient player experimentation so that enough data off the path are generated. In general, however, only the weaker concept of
self-confirming equilibrium that was introduced by the authors is obtained. The book concludes with a chapter on ‘sophisticated learning’ that deals with the general question of whether and how a player can exploit the fact that his co-players are learning as well. For example, a player could learn to detect patterns in past play and exploit these. Alternatively, a player who plays the game more than once, against short-term players, need not play myopically, it is better for him to ‘teach’ his co-players, in this way he can obtain his more preferred Stackelberg outcome.

Overall, the Fudenberg and Levine book provides an excellent overview of the recent theoretical literature on learning and evolution in games. The book focuses on the mathematical aspects. While there is some discussion about observations from experimental games at places one may, perhaps, regret that there is not more discussion of actual learning processes and of the speed of these. Some of these experiments have taught us that, even in relatively simply contexts, learning may be very slow, so that the relevance of the long-run equilibrium may be limited. (See, for example, the discussion on the simplified version of Akerlof’s lemons problem in Selten (1998).) A more extensive discussion of this experimental literature might have enabled the reader a better judgement on the domain where the theory that is developed here is relevant.

As stated above, Ariel Rubinstein sees the main distinction between his work and the type described in the book by Fudenberg/Levine in that his deals with deliberate choice making using procedures and not with mechanical adaptation to the environment.

Rubinstein starts by reviewing the main implicit assumptions in the standard rational actor model: a rational agent is assumed to know (to have a full overview of) the problem, to know what he wants, to do what he wants (i.e. he has the ability to optimize) and not to be misled by the way in which the problem is presented to him (i.e. framing effects do not play a role). The problem with this model is not just that the assumptions are unrealistic but also, and more importantly, that it produces results which differ significantly from actual human behavior, hence, that it may not offer much help in understanding human behavior. For example, the psychological literature provides convincing evidence that framing effects are important: the way the situation is presented may determine the choice that the subject takes in that situation. The reason for this ‘frame dependence’ may lie in the heuristics that subjects use in simplifying the original problem that is presented to them: one frame may lead to a different simplified problem that is solved by cognitive effort than another frame. Alternatively, subjects search for a way to rationalize their choices and one frame may enable a different rationalization than another.

The challenge that Rubinstein sets for himself is ‘to model formally procedures of choice that exhibit a certain procedural element and then to investigate whether or not such procedures are compatible with rationality’ (p. 25). Various chapters of his book describe different aspects of and approaches
to this problem. The first half devotes itself to choice problems (one-person decision problems), while the second half addresses these issues in a game context.

Chapter 2 deals with a formalization of the idea that individuals simplify problems by using the notion of similarity. A complicated problem may be related to a similar, but less complicated one. For example, in choices between lotteries, probabilities may be judged to be similar or prizes may be considered to be so. As in one case one similarity relation may be considered more prominent and in the other case another, this provides an explanation for choice behavior that conflicts with that of the rational agent but that is more in agreement with actual behavior.

Chapters 3–6 deal with various aspects of knowledge: they review the basic models of Hintikka and Kripke and consider extensions (non-partition models) which could then explain such phenomena as pure speculative trade. Standard models assume perfect memory, i.e. knowledge once acquired remains available intact. Chapter 4 discusses models of imperfect recall and shows again that these lead to qualitatively different conclusions such as, for example, the possibility to improve payoffs by making use of a randomizing device. As the discussion of the ‘absent minded driver paradox’ illustrates, such models raise challenging conceptual issues. Chapter 5 considers the question of what knowledge to acquire, given the constraint that only a limited amount can be acquired. Furthermore, it gives a specific example of how economic institutions could be explained from such bounds on knowledge. Concretely, if some consumers face knowledge constraints, a seller could increase profits by deliberately reducing market transparency and making price comparison more difficult. Chapter 6 discusses similar issues in the context of the theory of the firm: how to structure communication between units of the firm when gathering and communicating the information is costly?

The chapters 7–10 deal with games. In chapter 7 each player is characterized by a simple rule (try out all of your actions once, see which action did best and continue to play that one) and it is investigated what outcomes will result in that case. The procedures lead to an equilibrium, but this need not be the same as the usual Nash equilibrium. Chapter 8 focuses on the complexity of repeated game strategies. From the so-called ‘folk theorem’ it is well known that cooperative outcomes may be obtained in repeated games when players threaten to punish deviations by moving to unattractive outcomes. In equilibrium such punishments will, however, not be carried out and when memory is costly, players will be tempted to drop the punishment clauses, making the original equilibrium unstable. The chapter addresses the question of what outcomes can be sustained when players are not just concerned to maximize payoffs, but also want to use simple (low memory) strategies. It is shown that the folk theorem does not survive. Chapter 9 addresses the question of whether the models from the book could offer a convincing explanation for the ‘finite horizon paradoxes’ from game theory. While models can be constructed that
produce cooperation in the finitely repeated prisoner’s dilemma, Rubinstein himself is not convinced of their relevance, arguing that we do not yet have a good understanding about the reasoning procedures of humans in finite horizon games. Chapter 10 briefly discusses some ‘computability’ issues, arising out of the question ‘is there a rational player?’ but this part seems only loosely related to the rest of the book.

The book concludes with a very informative discussion between the author and the founding father of the field, Herbert Simon, about what the present book has contributed and about the aims of economic theory more generally. In essence, Simon accuses Rubinstein of ‘armchair theorizing’ on the basis of casual empiricism; he argues that the latter is no good basis on which to erect a theory and urges for more detailed observation and experimentation first. While admitting that the departures of the rational actor model have to be based on some empirical observations about actual behavior, especially since the space of possible deviations is so large, Rubinstein defends himself in this debate by taking the theorist’s position that the logic underlying these procedural models deserves to be explored as it clarifies the concepts that we use and sharpens our intuitions. Hence, while Simon’s first priority is to find out the kind of reasoning procedures that people actually use (and why and how these are shaped by experience and the social environment), Rubinstein’s is to analyze properties of tractable processes that are inspired by, and relate in their basic concepts to, actual decision-making processes. Such discussions on priorities are, of course, not new, not even in game theory where the founding fathers discussed them (see Von Neumann an Morgenstern (1944 Sect. 2.1.).

**REFERENCES**


**THE HABIT TO SURPASS MARX**


Joseph A. Schumpeter was one of the outstanding economists of the first half of the twentieth century who, at Harvard, was a teacher of a great many eminent economists of the second half. They all have kept him in high esteem and good memory without having become Schumpeterian economists. An
apparently charming personality with deep insights in the process of economic development and in the science of economics, Schumpeter, unlike Keynes or even Hayek, did not generate a school of his own, however. For what is called Schumpeterian economics is an occupation with his themes rather than with his theories. His theme in economics was evolution: innovation and the entrepreneur, economic development, systemic evolution, and the evolution of ideas. It is typical of post-Schumpeterian work on these themes that the authors refer to him as a source of inspiration, but do not elaborate on his theories. The reason, it will be seen, is that there are no such theories. Schumpeter was the man of vision, of the grand view, not of the worked-out individual theory. Nevertheless he was an outstanding economist.

In his fascinating book Yuichi Shionoya sets out to show that the restriction of Schumpeter to his three best known books, Theory of Economic Development, Capitalism, Socialism and Democracy, and History of Economic Analysis, or any particular of his themes does not do justice to his contribution to social science. Schumpeter’s ‘desperate brevity’ in fundamental theoretical groundwork (‘He was never absorbed by small and fragmented problems; his themes were always grandiose yet basic’ p. 179) is due to a gigantic coherent research program, not well understood in his time and later, the vision of a universal social science. Grown up with the Austro-German Methodenstreit, with the aspirations of Austrian marginalism, Lausanne equilibrium, the German historical school, and Marxist historical marterialism, Schumpeter accepted the viability and legitimacy of each of these approaches which were only in need of integration – his scientific task. This has won him the often heard critique of eclecticism. Now, Shionoya reconstructs Schumpeter’s scholarly work as contributions to a general system of a universal social science. As a general theory this system is treated along the criteria of philosophy, sociology and history of science. As a theory of the social system it deals with statics, dynamics and economic sociology. What the author calls a metatheoretical study is a complicated matrix of these two levels of analysis which are informed by historical reality, society, on the one side and philosophy of science, thought, on the other.

Whether or not the reader is at the end convinced of Shionoya’s hypothesis, he will have read a brilliant account of Schumpeter’s work and its evolution by an author who seems to know every line Schumpeter has published and who is well acquainted with the ‘intellectual field’ in which Schumpeter operated, the discussions and contributions which have influenced him (especially Wieser, Schmoller, Max Weber – not to speak of Walras and Marx who were Schumpeter’s intellectual heroes). The hypothesis of a life-long research programme may be seen to be supported by the fact that Schumpeter formed his basic ideas in the relatively short first period of his scientific activity before World War I. The plausibility of the hypothesis is enhanced by the introduction of the concept of habitus borrowed from Bourdieu, ‘a set of dispositions created by objective conditions and personal history’ (p. 24). ‘Habitus is not only a personal propensity but also can be held socially in common’ (p. 3). In
the latter case it may be equated with the Zeitgeist, another concept introduced later in the book denoting the ideological expression of the hierarchy of social values.

The constitutive social value of the prevailing social system capitalism was ‘leadership’ which had already been identified by Wieser and others. Leadership according to Schumpeter, however, was a legacy from the pre-capitalist period, while capitalism itself produced the values of rationality. The dialectic relation between pre-capitalist leadership and capitalist rationality is the motive force for further system development. Schumpeter’s idea of leadership was heroic and romantic, the exceptional individual with a grand vision, the knight, the entrepreneur who transforms a given situation. Where there is the individual leader there must also be the mass of followers. Clearly, Schumpeter’s personal habitus was to be among the heroes, a renaissance figure, and it demanded that he constructed nothing less than a universal social science, to address the historical evolution of social systems. It was, in fact, the programme of Marx who had valiantly failed in accomplishing it. Indeed, the Zeitgeist of Schumpeter’s time was one of leadership, not so much to the benefit of the following masses. And, indeed, Schumpeter aspired the position of the world’s leading economist of his time, a position which was occupied as a matter of course by his contemporary Keynes. Perhaps there is here one of the sources for his melancholic vision that the time of heroes and leadership is over. The future belongs to rationality, bureaucracy and the democratic masses. The period of capitalism already carries the germs of a new Zeitgeist which, in the end, will substitute it by a new system, socialism. This, of course, will also leave the habitus of scholars not unaffected. ‘His idea of social science was the combined product of the German and Austrian intellectual fields of the early twentieth century and his personal habitus’ (p. 30). It must be said here, that Shionoya does not fully carry out the programme to analyse Schumpeter from this point of view which would have required an intellectual biography rather than a metatheoretical study.

Whatever Schumpeter’s personal ideas and intentions may have been, the question now is whether or not we can read his works as contributions to a universal social science. The object of such a science is the social system of which he distinguishes three constitutive aspects: the static state; the dynamic state; and sociocultural development. In order to analyse them, he applies four methods: theory; statistics; history; and economic sociology. Whether this is a consistent set may be questioned. It would be wrong to assume that the static and the dynamic state are the economic aspects of the social system whereas sociocultural development applies to the institutional environment. For Schumpeter seems to think that such a trichotomization is applicable to all spheres of social life. This prompts the question how to distinguish the three aspects which, in fact, are treated as states of reality. The distinction is provided by the actors or leading force. Different types of man are operating in the static and the dynamic state: rational man whose behaviour is attracted by
equilibrium in the static, the entrepreneur whose behaviour leads away from equilibrium in the dynamic, whereas sociocultural development is a macro phenomenon subject to holistic leading forces. It is clear why the Austrian school does not count Schumpeter among their crowd: his dichotomization of types of man and separation of aspects of social life runs counter to their view of the market as a process and every man as an entrepreneur searching for presently undetected solutions, a process which if left alone will yield a spontaneous order. His view, as Shionoya sees it, allows Schumpeter to treat statics, dynamics, and sociocultural development separately, to apply different methods to the analysis of each, methodological individualism to the first two and methodological holism to the third, and to leave Walras, Marx, and Schmoller, the representatives of static analysis, dynamic analysis and sociocultural or historical analysis in their own right. The impression of eclecticism and particularism rather than a universal approach cannot be dismissed easily.

The biggest problem seems to be the location of statics and the static system, the realm of rational man, which was treated by Schumpeter in his first book *Das Wesen und der Hauptinhalt der theoretischen Nationalökonomie*. He called statics ‘pure economics’ or the ‘logic of economic phenomena’, abstract and universally valid, yet not to be subjected to statistical testing, i.e. confronted with historical reality. This looks like Austrian *a priorism* as we find it in Mises – economics as praxeology. However, it is likewise informed by the Lausanne school which means it does not analyse human behaviour but the mechanics of the exchange of goods. Utility is not the essential pivot of pure economics but only an instrumental device to mediate between scarcity and preferences. It is not quite clear what it means to call the static system unrelated to historical reality if the static system is indeed the realm of rational man or the simple manager who is a real type of a human being and if it is held at the same time that ‘the economic system is regarded as stable after all’ (p. 75), i.e. equilibrium is the normal state of the economic system and its disturbance a rather unique dynamic event caused by the entrepreneur. The proof of the theoretical pudding is in its empirical eating.

Shionoya devotes a whole chapter to explain Schumpeter’s methodology of instrumentalism which he considers a major contribution in *Wesen*. The chapter is very fundamental going back to Mach and Poincaré and comparing Schumpeter with Friedman. One could as well be rather sanguine about Schumpeter’s instrumentalism. For the essence of theoretical economics – that it is not essentialist, but only an instrumental device to describe economic facts – is generally accepted today. There are, however, some hidden difficulties which Schumpeter overlooked according to Shionoya – namely the epistemological nature of facts. If facts are not independent of theories, the practical success of theories as the ultimate test of their usefulness becomes slippery. In *Wesen*, however, Schumpeter had to defend the abstract Walrasian approach against the realism of Menger and the historicism of Schmoller. This more or less required an instrumentalist view. Whether Schumpeter’s meth-
odology of instrumentalism extends beyond *Wesen* is argued, but not wholly convincingly shown in the book.

Take, for instance, Schumpeter’s best known ‘vision’, the entrepreneur. He is presented as a historical idealttype whose behaviour is called irrational and nonhedonistic, but rather motivated by the wish to found a private kingdom, the will to conquer and to exercise his energy and ingenuity – a modern knight errant. The fading away of such pre-capitalist values in the culture of capitalism which is basically rationalist makes for the transition to socialism. Is the entrepreneur only an instrumentalist device? At least in the context of the *Theory of Economic Development* he may be considered as such. For what is needed in this theory is an explanation of creative destruction of an existing equilibrium. Schumpeter can offer only an exogenous explanation: the entrepreneur. There is no theory of entrepreneurship – in fact, there cannot be, since creative activity ‘causes discontinuity from preceding situations’ (p. 175) whereas a theory reduces a given situation to a preceding one. So, the entrepreneur may be considered a theoretical instrument unrelated to historical figures. Schumpeter never took the trouble to study the behaviour and motives of historical captains of industry. He would have found a totally different type from his heroic knights (see, e.g. Kocka 1975). But as there is, according to Schumpeter, no need to justify psychological hypotheses in static theory, he may have upheld that there is also no need to justify the construct of entrepreneur in dynamic theory. *Economic Development* actually can be read as Hamlet without the prince of Denmark, if it only works.

There are sufficient reasons to doubt whether it works. For next to Hamlet there is a second leading, but not materially present personage in *Economic Development* without whom creative destruction does not function and who has been badly neglected by Schumpeter, the banker who supplies the credit which destroys equilibrium. The banker needs the same qualities as the entrepreneur, since he has no more information, in fact even less. For he does not know whether the entrepreneur approaching him for credit is a knight errant or a moral hazardeur. It may be conjectured that Schumpeter’s neglect (or failure) to present a coherent theory of money and banking was due to the conflict between his dynamic vision of the role of banking in development and his static habits as Walrasian which should have led him rather into the direction of Hayek’s neutral money (see also Schefold 1986 and Minsky 1986). And second there are sufficient reasons to doubt whether the entrepreneur, so admired by his author, was really meant as an instrumentalist device. Certainly in *Capitalism, Socialism and Democracy* he became a historical idealttype. But this brings us to Schumpeter’s economic sociology which is admirably presented by Shionoya.

The author develops Schumpeter’s economic sociology as a successful attempt to mediate in the notorious *Methodenstreit* between Schmoller and Menger and to come to grips with Max Weber’s *Verstehende Soziologie*. Statics and dynamics is governed by rational (or non-rational as in the case of
the entrepreneur) autonomous individuals. Economic sociology takes account of the fact of social institutions which make individuals culturally embedded. It would be wrong to think, as sometimes seems to be suggested in the book, that this affects rationality. Rationality is instrumental to any arguments of the utility function which may be shaped by culture. Institutions could imply behavioural constraints and values, i.e. different utility arguments, or typical reductions of transaction costs. The abstract analysis of rational utility maximization under constraints remains unaffected, only its concrete policy application (not a strong point in Schumpeter anyhow) needs historical specification. The problem of static equilibrium is its dependence on stable preferences of the isolated individual. ‘The nature of interactions between institutional conditions and individual behaviors is best described as evolutionary’ (p. 199). There is no static stability.

While the problem of inconsistency between statics and dynamics had been (unsatisfactorily) solved by the assumption of two different types of man, the cultural interdependence and conditioning of men raises without mercy the question of the usefulness of the static equilibrium model. Shionoya’s edifice of a universal Schumpeterian social science consisting of statics, dynamics, and economic sociology looks rather shaky. But he may be right in interpreting Schumpeter’s vision. If we read what Schumpeter wrote in 1926 and what Shionoya (p. 291) calls the essential trend of history: ‘The time will soon come, when social preferences are unified so that in every given situation the choice of goals is made possible by means of science’ it can only be interpreted as the ultimate establishment of the static state if it can meaningfully be interpreted at all: socialism as the end of history.

Schumpeter’s major contribution to economic sociology is *Capitalism, Socialism and Democracy*, a book which was immensely successful with the only exception of the economics community. It is a grand vision without much theory. *Mutatis mutandis* it may be compared to Oswald Spengler’s *Untergang des Abendlands*. Seen as a product of the intellectual field plus the personal habitus, the march into socialism was the logical outcome of the great depression and the great war. While in the 1920s the inevitability of socialism was a theoretical possibility, in the mid-1940s it seemed a historical fact. Just to say ‘Marx was right’ would not have conformed with Schumpeter’s personal habit. He had to develop a new story – if possible a paradoxical one. Its theoretical content compares poorly with its rhetoric, its ‘charm of literary play’. The historical generalizations, for instance in his theory of class, are not based on serious research. Take the feudal class which produces the values for the entrepreneur. According to Schumpeter the feudal lords all had been warriors in the middle ages endowed with knightly values. He disregards the ministerial nobility (*Dienstadel*) and the emergence of the modern state (see, e.g. Kerhervé 1998).

Let it be mentioned that there is next to the lack of a coherent treatment of money another lacuna in the Schumpeterian system of social science which is
the economic role of the state. The item ‘state’ does not figure in the index of Shionoya’s book which is indicative of its importance in Schumpeter’s work. It can be hypothesized that it is exactly the neglect of the economic role of the state within the capitalist world that eases the apparently erroneous observation of a trend towards socialism. Such a view, however, would be inconsistent with Schumpeter’s own hypothesis of the fading away of the heroic values among the capitalist bourgeoisie, as the bureaucratic state certainly does not support or substitute the heroic values of the entrepreneur. A more consistent answer would be that static (not stationary) capitalism plus a strong state is what is meant by socialism – socialism as the highest stage of capitalism. Apart from thus diluting the basic concepts (after all also Schumpeter defines capitalism by private property and market coordination and socialism by collective ownership rights and planning), it would make of the theoretical prediction a kind of tautology not worth thinking about. So it is perhaps closer to the mark that Schumpeter’s ‘Austrian’ habitus made him overlook the role of the state in capitalism, because he disliked it to have any.

If Capitalism, Socialism and Democracy is ‘a treasure of vision’ (p. 311) and Schumpeter’s contribution to our science in general is seen more in his grand view that in meticulously worked-out theories, then one has to ask about the scientific status of vision. ‘Vision is a preliminary image of problems in a prescientific stage’ (p. 60) into which ideology is likely to be incorporated. The value of a vision shows up in the successfully formulated theory, which means a theory which is useful. If this stage is not reached, the vision ‘is merely an illusion or a Weltanschauung’ (p. 61). The vision of a universal social science as well as many of the other grand Schumpeterian visions may be seen as a Weltanschauung produced by the intellectual field of pre-World War I Vienna and the personal habitus of Schumpeter. Would such a conclusion really be unfair to Schumpeter the social scientist?

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REAFFIRMING THE ENLIGHTENMENT VISION

At first glance, a book written by a biologist, even one as eminent as Edward Wilson, whose subject is not specifically economics, is a surprising choice for review in a journal specializing in economic methodology. However, for reasons I hope to explain, it is a book that makes some very important points about both economics and thinking about economic methodology.

As his subtitle explains, Wilson’s thesis is that knowledge should be seen not as divided into independent compartments but as a unified whole. The term Wilson uses to describe the links between disciplines is ‘consilience’, a term first used by William Whewell in 1840. Quoting Whewell, he defines consilience as:

literally a ‘jumping together’ of knowledge by the linking of facts and fact-based theory across disciplines to create a common groundwork of explanation. He [Whewell] said, ‘The Consilience of Inductions takes place when an Induction, obtained from one class of facts, coincides with an Induction, obtained from another different class. This Consilience is a test of the truth of the Theory in which it occurs’.

( pp. 6–7)

What makes this thesis so controversial is that Wilson seeks to unify not just the natural sciences but also the social sciences, the humanities and even ethics and religion, fields long thought separate from natural science. The book ends with a chapter in which he discusses the choices that the human race has to make in response to the impending environmental crisis. Its reductionist thesis is clearly summed up at the start of this chapter:

The central idea of the consilience world view is that all tangible phenomena, from the birth of the stars to the workings of social institutions, are based on material processes that are ultimately reducible, however long and tortuous the sequences, to the laws of physics. In support of this idea is the conclusion of biologists that humanity is kin to all other life forms by common descent. . . . Our hereditary human nature, which evolved during hundreds of millenia . . ., still profoundly affects the evolution of culture.

( pp. 297–8)

Economics is linked to the natural sciences through psychology. Wilson’s argument here is that psychologists began to make progress in understanding human behaviour only when they started to look for physical evidence to help them distinguish between alternative theories of how the brain operated. It was, for example, work on the physiology of the brain, that led to an understanding of dreams, not the untested and ‘mostly wrong’ theories of Freud. In
the same way that progress in psychology involved testing theories against knowledge of biology, so progress in economic knowledge will require, Wilson claims, consilience between economic theories and what psychologists know about human behaviour. This is substantially the thesis argued by Rosenberg (1992), but Wilson’s exposition of it is the more forceful for being placed in a much broader context.

If one is prepared to accept the sociobiology, for which Wilson has made his reputation, the force of this claim is clear. However, the underlying thesis that we should be looking for consilience between different disciplines does not necessarily require this, even though Wilson does find that the evidence, as he sees it, supports an evolutionary psychology and sociobiology. The thesis that economics should be consilient with what we know of psychology and that economics is unlikely to make progress without any input from psychology, is independent of any particular psychological theory. It goes against the ‘separateness that, Hausman has argued, characterizes contemporary economics.

Economics is but one of many disciplines covered in this book. Its particular importance, however, emerges in the final chapter where Wilson discusses the impending environmental crisis caused by a rising world population. He offers an essentially Malthusian story of population pushing against resources (primarily food and water). Given the finite size of the earth, the population growth rate must eventually be reduced to zero, either by what Malthus called ‘moral restraint’ (deliberate decisions being taken on reducing population growth) or by ‘misery and vice’. (Wilson argues that it is no coincidence that the recent tragedy in Rwanda took place in the most densely-populated country in Africa.) Despite the certainty of this crisis, should present growth rates continue, there is great reluctance to face up to it and a major role is played by economists. His words here are so strong that they are worth quoting in full:

The greatest single obstacle to environmental realism . . . is the myopia of most professional economists. In Chapter 9 I described the insular nature of neoclassical economic theory. Its models, while elegant cabinet specimens of applied mathematics, largely ignore human behaviour as understood by contemporary psychology and biology. Lacking such a foundation, the conclusions often describe abstract worlds that do not exist. The flaw is especially noticeable in microeconomics . . . The weakness of economics is most worrisome, however, in its general failure to incorporate the environment.

(p. 324)

The errors of economists are also faced by theoretical biologists who, suffering from physics-envy, are easily seduced into building physics-like models. These produce structures that look like emergent phenomena found in the real world, perhaps like the ‘calibrated’ models of real business cycle theory. (Wilson confesses to having been guilty of this himself and to having
produced his share of failed theories.) Their creators then fall into the error of assuming that because a correct result was obtained, the theory is necessarily correct. More rigorous testing is required.

Perhaps more important is Wilson’s unequivocal endorsement of the Enlightenment view that ‘entirely on our own we can know, and in knowing, understand, and in understanding, chose wisely’ (p. 332). It was, he contends, during the Enlightenment that the dream of intellectual unity ‘first came to full flower’ (p. 13). Though the Enlightenment, which Wilson traces back to Francis Bacon, may have failed with the horrors of the French Revolution (he dates failure of the Enlightenment to the death of Condorcet in 1794) it survives as the only possible way to do science. Wilson is very critical of postmodernism, but argues that it has one virtue – it reminds us that we may be wrong:

We will always need postmodernists or their rebellious equivalents. For what better way to strengthen organized knowledge than continually to defend it from hostile forces? . . . And if somehow, against all the evidence, against all reason, the linchpin falls out and everything is reduced to epistemological confusion, we will find the courage to admit that the postmodernists were right, and in the best spirit of the Enlightenment, we will start all over again. Because, as the great mathematician David Hilbert once said, capturing so well that part of the human spirit expressed through the Enlightenment, Wir müssen wissen. Wir werden wissen. We must know, we will know.

(pp. 46–7)

It is not necessary to accept all the details of his argument, to accept that Wilson’s book is a tour de force. It offers an inspiring restatement of the values of the Enlightenment in the face of postmodern criticisms. Wilson’s thesis that the drive towards consilience – towards unified science – is the route to progress is backed up by numerous examples from the natural sciences and he has suggestive ideas to offer on how this might be extended to the humanities. His criticisms of economics are well informed, and though they may not be original, they derive new significance from the context in which he places them. Consilience of economics with psychology may or may not be possible for some time (I refrain from expressing a view one way or the other on this). However, this does not weaken the argument that lies at the heart of Wilson’s claims for consilience – that knowledge claims should be tested against all possible sources of evidence. This implies that it is dangerous to use ‘as if’ reasoning to justify the use of models or assumptions that are inconsistent with available evidence, whether this evidence comes from within economics or without. Such an attitude may make the task facing economics more difficult but it is arguably the only way forward in the long term.

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