

sanity, humanity and science

post-autistic economics review

Issue no. 24, 15 March 2004

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A Response to King's Argument for Pluralism

Paul Davidson (Editor of the *Journal of Post Keynesian Economics*)

In an article in the last issue of this journal, John King presented "Three Arguments for Pluralism in Economics".¹ In his paper, King misrepresents my view in this matter. More importantly I disagree with the arguments put forth by King. Accordingly, the following presents my responses (in the form of a dialog) to King's PAER statements:

King: Is there a single correct alternative to neoclassical economics? The purpose of this short paper is to suggest that there is not, and to show that this fact is increasingly recognized by eminent practitioners of several varieties of heterodox economic theory.

Davidson: If one wishes to explain (describe) the production, exchange and financial features and operations of a market-oriented, money using, entrepreneurial economy, then Keynes's "General Theory" is the sole "correct" alternative to neoclassical economics. Neoclassical theory is, as Keynes specifically noted (on page 3 of his 1936 book) merely a "special case" of his general theory.² Moreover I would argue that Sraffian, Kaleckian, and other heterodox theories that try to explain the operation of a market economy are other special cases obtained by adding additional restrictive axioms to Keynes's basic general theory.

King: Heinz Kurz and Neri Salvadori . . . surprisingly, [come] to the defence of pluralism. Economic reality, they note, is widely believed to be very complicated. The questions that economists ask are therefore inherently difficult, and it is unlikely that they have simple answers. Since no theory can consider all relevant factors in any particular economic context, there is a strong prima facie case for theoretical pluralism.

Davidson: All reality is complicated. But that is not a sufficient defense for pluralism. For example, it is said that an equation that takes account of all the gravitational forces that affect the tides on any place on Earth, can run to several pages. This does not stop weathermen from using a simplification

of the law of gravity to provide a useful approximation of the time of high tide at any specific place on the ocean shore by relating the tides solely to the gravitational forces of the Earth and the Moon. Complications per se do not require plural alternative explanations for observed phenomena.

King: Geoff Hodgson argues that the notion of a single, “general” theory applicable to human behaviour in all societies, at all points in time, is a dangerous delusion that has led astray not only neoclassical economists but also many heterodox theorists. Failure to appreciate the need for historical specificity in economic theorising has not only blighted the work of several generations of general equilibrium theorists, but also reduced the analytical achievements of some of their most vocal opponents, including Clarence Ayres, John Maynard Keynes and Joan Robinson.

Davidson: Keynes’s *General Theory* is meant to explain a modern, money using, market economy. If one wishes to analyze (explain, discuss) feudalism, or the economies of biblical times, one must add additional restrictive axioms to Keynes’s general theory to obtain a special case theory of feudalism, or of biblical economics, etc. Nevertheless, a common general theory will underlay all these specific cases of historical economies.

King: One does not have to agree with all [of] Hodgson... to accept the truth of his contention that “there are several problems with general theorizing in the social sciences. One is of analytical and computational intractability. Facing such computational limits, general theorists typically simplify their models, thus abandoning the generality of the theory. Another related problem with a general theory is that we are confined to broad principles governing all possible structures within the domain of analysis. In practice, a manageable theory has to confine itself to a relatively tiny subset of all possible structures. Furthermore, the cost of excessive generality is to miss out on key features common to a subset of phenomena”.

Davidson: Hodgson, as well as King and many others, have confused the concept of a general theory with that of Debreu’s concept of *general equilibrium* as the mother of all economic theory! Unlike Debreu’s general equilibrium theory, *Keynes’s general theory analysis is an axiomatic based approach that required fewer restrictive axioms than any other economic theory*. Moreover, in defending his fewer axiomatic approach as a realistic general economic theory, Keynes noted “It is for those who make a highly special assumption to justify it rather than for those who dispense with it to prove a general negative”. In that sense Keynes was not only a developer of economics as a mathematical (axiom-oriented) logical analysis, but his theory had a pragmatic vision of a physical real world process in mind.

Roy Weintraub in his recent book *How Economics Became a Mathematical Science*³ noted that a new image of mathematics emerged in the early decades of the 20th century, and this image shaped the development of mainstream mathematical economics. “To preserve the relationship between rigor and truth, economists began to associate rigor with axiomatic development of economic theories, since axiomatization was seen as the path to discovery of new scientific truths” (Weintraub, 2002,p.98)

But this mathematical approach leads to the question of whether “truth” is discovered by having sufficient axioms to obtain the “right” level of generality or by an axiomatic theory based on the least number of assumptions (“a general theory of employment, interest and money”) that is descriptive and applicable to reality? The right level of generality was Debreu’s vision of discovering truth. On the other hand, using the least number of assumptions descriptive of the real world was Keynes’s approach to the “truth” -- as suggested in his analogy of comparing classical economists with Euclidean geometers in a non-Euclidean world who continued to use the restrictive axiom of parallels to explain why lines apparently parallel often crash. This is also the belief that underlies Sidney Weintraub and my vision of a Post Keynesian economic theory where the axiomatic base of a general theory should not only be as small as possible – but these axioms should be applicable to the real world.

The restrictive bigger axiomatic foundation of Debreu's general equilibrium theory, in my view, is not applicable to the real world market economy that we live in.. The onus is therefore on those who, like Debreu, would add such restrictive axioms to obtain a general theory to demonstrate the relevance to the real world of their additional postulates of specific case analysis.

In economics, the school of Bourbaki mathematical philosophy was transplanted into post-war American economics by Debreu. The seed bed that encouraged the domination of this non-real world view of economic theory was the Cowles Commission of the early 1950s (Weintraub, 2002, p. 104). The Bourbaki method argued that economists developing special cases had to build on the foundation of general (Walrasian-Debreu) equilibrium case. The general structure of this equilibrium foundation was obtained by developing chains of syllogisms from what Debreu considered fundamental axioms that might be buried under accumulated debris of real world details. In this Bourbaki approach "*good general theory does not search for the maximum generality, but for the right generality*" (Weintraub, 2002, p. 113). In other words, Bourbaki did not accept Keynes's search for the "maximum" general theory, i.e., a general theory that had the smallest axiomatic foundation that still provides a readily recognizable description of a real world economy. (Keynes's general theory threw out three classical restrictive axioms⁴.) According to Bourbaki, Keynes's general theory -- based on fewer axioms than Debreu's general equilibrium theory -- is not "good" theory. Instead, Debreu's general equilibrium theory of value which expresses itself in terms that few, if any, would readily recognize as an apt description of a real world economy (Weintraub, 2002, p. 114) provides the Bourbakian "right" level of generality. In other words, in a Bourbaki view of economics, theories that are readily recognizable as descriptions of reality are not necessarily important. As Weintraub (2002, p. 120) notes, Debreu's 1959 monograph "*The Theory of Value . . .*" still stands as the benchmark axiomatization of the Walrasian General Equilibrium model . . . the 1959 book wore its Bourbakist credentials on its sleeve, though there may have been few economists at this juncture who would have understood the implications of" Debreu's statement on p. x of the preface:

The theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics. The effort towards rigor substitutes correct reasoning and results for incorrect ones.....leads to a deeper understanding of the problems to which it is applied...also lead to a radical change of mathematical tools.... Alliance to rigor determines the axiomatic form of analysis where the theory, in the strict sense, is logically disconnected from its interpretation.

Here is a declaration of independence indicating there is no need for the elements of a rigorous economic theory to have counterparts in the real world. Debreu considered that "the model of Walrasian equilibrium was the root structure [the right level of generality] from which all further work in economics would eventuate" and he showed disdain for attempts (like that of Kenneth Arrow and Frank Hahn) to forge explicit links between the Walrasian model and contemporary theoretical concerns in macroeconomics" (Weintraub, 2002, p. 121)

In his bold leap of faith, Debreu believed his work to be "the definitive mother-structure from which all further work in economics would start, primarily by weakening its assumptions or else superimposing new interpretations upon the existing formalism. This stance, however, requires one very crucial manoeuver that was never explicitly stated by Debreu, namely that the Walrasian general equilibrium approach was the root structure from which *all* further scientific work in economics must be developed (Weintraub, 2002, p. 122).

Just as Jefferson's declaration of independence liberated the thirteen colonies from fat King George, Debreu's declaration of what constituted the mother-structure "liberated" economics from its dependence on real world analogies. Weintraub (2002, p, 122) states that Debreu's "Bourbaki program marked a definitive break with physical metaphors". Successes in the natural sciences may depend upon "bold conjectures and experimental refutation, but economics had nothing else to fall back upon but mathematical rigor".

It is this Bourbakian view that, I believe, the proponents of “pluralism” are protesting against -- even though they do not know it.

King: The Post Keynesians Victoria Chick and Sheila Dow make an equally powerful, if largely implicit, case for pluralism in their penetrating analysis of what is implied by mathematical modeling in economics. Formalising an argument is not, they suggest, an unambiguous improvement, as neoclassicals believe. On the contrary, it is a matter of costs and benefits. Formalism entails a particular view of the world, namely that it displays event regularities strong enough for it to approximate to a closed system.

Davidson: Formalism can be consistent with “open models” as I (Davidson⁵) demonstrated in my development of the use of nonergodic systems in economic theory. It is the importance of nonergodic processes that makes refutation in economics difficult if not impossible. In my view most (but not necessarily all) important economic stochastic processes are nonergodic and hence a permanent rejection of any conjecture about important economic phenomena such as employment, economic growth, etc. are linked to specific historical events, culture, and an uncertain, not statistically reliably (even in principle) predictable future.

Although Debreu’s expresses “enthusiasm” for the way he incorporates “uncertainty” into his axiomatic model, his concept of uncertainty has nothing to do with the concept of an unpredictable future. Debreu introduces “uncertainty” by merely redefining the interpretation of a commodity to take account of contingencies (or expressed different states of the world) and a complete set of contingency markets for every date in the foreseeable future. Thus for Debreu uncertainty does not require an open model.

Weintraub (2002, p. 125) notes that the “Bourbakism propagated by Cowles had identified neo-Walrasianism and good economic theory . . . neo-Walrasian theory had become conflated with the very standard of mathematical rigor in economic thought . . . why precisely should the Walrasian framework be taken as the sole ‘structure’ from which all mathematical work should depart? . . . was it not better to make a case for the right level of generality, then claim one had the maximum level? The Bourbaki answer is that rigor was a matter “of style...and politics...and taste”.

Similarly, when King notes that “Hodgson’s own proposal for the reconstruction of economic theory, putting the history back, is innately and profoundly pluralistic” I believe that Hodgson’s view of what is good economics is a matter style, politics and taste on Hodgson’s part.

King: Chick and Dow do not completely deny the legitimacy of formalism in economics, in all circumstances, for all purposes. On the contrary: some problems lend themselves to closed-system thinking and cry out for precise, formal solutions. They argue only that it is a serious mistake to suppose that all economic problems are of this type.

Davidson: I believe that Chick and Dow are confusing Debreu’s Bourbakian variant of formalism with the use of formal logic. In Chick’s and Dow’s view what problems are susceptible to Debreu’s formalism is, I think, a matter of taste, style, and politics.

King: If pluralism does not (quite) rule out formalism, what does it exclude? Unqualified relativism, for one thing; logical incoherence, for another. Hodgson is the most outspoken in denying that “anything goes”, and the most sternly critical of postmodernist claims in this regard. “An acceptable policy of pluralism”, he suggests, “concerns the policy of institutions towards the funding and nurturing of science. Such a policy involves ‘pluralism in the academy’. But it would not extend to the individual practices of science itself. This confusion, between encouraging contradictory ideas in the academy and encouraging them in our own heads, is widespread in post-modernism . . . There is much to be said for tolerance of many and even antagonistic scientific research programmes within an academic discipline or university. But we should not tolerate the existence of inconsistent ideas within our own heads.”

Davidson: But how can we assure that different models are not logically inconsistent unless we have a benchmark “general” model with a minimum number of well-specified axioms that acts as the foundation of all other models?

King: King notes that Hodgson states “the policy towards science must be pluralistic and tolerant, but science itself must be intolerant of what it regards as falsehood . . . Any failure of social science to erect an adequate and coherent general theory is not rectified by applauding incoherence”
Horses for courses, as Geoff Harcourt has always put it, but they must each have four legs and a jockey and proceed anti-clockwise around the course.

Davidson: The horses for courses analogy is misleading. In my view the legs of any economic model (horse) must be the same basic axioms underlying Keynes’s general theory. Those who wish to add additional restrictive axioms must, as Keynes notes, specifically justify the use of these additional postulates as realistically applicable to the real world. Only if all the horses shares the same basic axiomatic legs can we let them race on different courses

King: Kurz and Salvadori also insist on the need for logical consistency in economic theorising. For them this criterion is enough to rule neoclassical analysis out of the race, since its conception of capital is fundamentally flawed. If the “principle of substitution” is central to mainstream theory, they argue, it should be applied in a logically consistent manner. In the long period, this means that an increase in the price of one input induces a decrease in the quantity of that input per unit of output. “All propositions of the theory can be traced back to this basic idea. If it is not true in general, the theory appears to be in trouble” (Kurz and Salvadori, 2000:238). But it has been known since the mid-1960s that it is, in general, false when applied to the collection of heterogeneous commodities known as “capital”.

Davidson: Unfortunately for Kurz, Salvadori and King, Keynes rejected the axiom of gross substitution long before the 1960s capital controversy. In Keynes’s chapter 17 on the essential properties of interest and money, Keynes specifically rejects the ubiquitous applicability of the axiom of gross substitution. And Arrow and Hahn⁶ have demonstrated that in the absence of gross substitutability, all existence proofs of a general equilibrium are jeopardized.

King: From a quite different perspective the Post Keynesian Paul Davidson has criticised what he terms the “babel” of New Keynesian economics, in which market imperfections that prevent downward price and wage flexibility are denounced as the fundamental cause of involuntary unemployment while in the same breath a falling price level (“deflation”) is decried as a serious macroeconomic evil (Davidson, 1999; compare Solow, 1997 and Taylor, 1997 for graphic examples of this incoherence). Horses for courses, once again, but all four legs must be pointing in the same direction.

Davidson: And the horse merely must show that his legs display the essential properties of interest and money– independently of the substitutability of labor and/or capital as factors of production. In sum, then, I believe that encouraging pluralism in economics without a common general theory foundation merely encourages heterodox economists to erect a modern Tower of Babel, thereby making it easier for Mainstream economists to ignore the resulting incomprehensible babel coming from this heterodox structure.

Instead, heterodox economists who want to affect the development of their discipline as taught in major universities and economic journals must unite behind Keynes’s general theory and demonstrate that what passes as mainstream theory is merely a special logical case requiring additional restrictive axioms that are unrealistic and therefore policies based on this special case will be disastrous if applied to the real world global economy of the 21st century.

Notes

1. J. E. King, "Three Arguments for Pluralism in Economics", *post-autistic economics review*, issue no. 23, 5 January 2004, article 2, http://www.btinternet.com/~pae_news/review/issue23.htm
2. "The characteristics of this special case...happens not to be those of the economic society in which we actually live, with the result that its teaching is misleading and disastrous if we attempt to apply it to the facts of experience". J. M. Keynes, *The General Theory of Employment Interest and Money* (Harcourt, Brace, New York, 1936), p. 3.
3. E. R. Weintraub, *How Economics Became A Mathematical Science* (Duke University Press, Chapel Hill, 2002).
4. These are (1) the ergodic axiom for stochastic models or the ordering axiom for deterministic models, (2) the gross substitution axiom, and (3) the neutrality of money axiom.
5. P. Davidson, *Financial Markets, Money and the Real World* (Elgar, Cheltenham, 2002), chapter 3.
6. K. Arrow and F. H. Hahn, *General Competitive Analysis* (Holden Day, San Francisco, 1971), p. 361.

SUGGESTED CITATION:

Paul Davidson, "A Response To King's Argument For Pluralism", *post-autistic economics review*, issue no. 24, 15 March 2004, article 1, http://www.btinternet.com/~pae_news/review/issue24.htm

Neutrality Is Overrated

Juan Pablo Pardo-Guerra (National University of Mexico)

In past issues of this journal I have encountered some interesting statements on the things that must change in order to build a post-autistic economic theory. Being a citizen of the South, I subscribe to most of them and greatly welcome the discussions. However, of the sea of comments and proposals, the ones that caught the greatest part of my attention were those which used specific terms that conceal, in one way or another, the idea of "good" and "bad" science, and in general hinting at the notion that existing economics falls into the second category and should be moved onto the first. So let me start this paper by saying that science, in any of its forms and in any time and place, cannot be measured with the uncomfortable parameters of "goodness" or "badness". Doing so is a wild goose chase and one of the impasses that a post-autistic economic theory should try to avoid. Therefore, as I will explain, talking of a more scientific economic theory is a dead-end road.

In particular, there have been two PAER articles in which economics is either directly or indirectly related to science by means of the construction of either a new discourse or of a new methodology. The first and most comprehensive of these is the paper by James Galbraith, in which he mentions the necessity of building a "theory of human behavior based on the principles of social interaction" which privileges empirical work while being free from "interest-group politics"¹. On the other side of the spectrum, Claude Mouchot presents ideas on how to develop a "scientific discourse in economics" from a purely philosophical, realist perspective².

The problem with these two and many other discussions on the topic lies in the attempt to make economics a *more scientific* discipline, as if its level of *scientificity* were to assure success of any kind. Pursuing a scientific discourse for economics is perpetuating the age-old idea that science is better, that science is good, and that science leads us to the truth and to an improved description of our universe and our future.

But science is, just as economics, a particular discourse, a tradition connected to the ideals of rationality and of progress which are rooted in a culture-specific, western perspective³. We say that science works and that science is good because, to a considerable degree, we have defined

“efficiency” and “goodness” on the basis of what science can achieve. Science is not a miracle-worker nor is it a tool with unlimited reach. Science has boundaries, science is human, science is flawed and science is biased. Any economic theory consistent with the prevailing scientific discourse is bound to inherit all the flaws and quirks of the discourse it was built upon.

Furthermore, the idea that a *scientific economic theory*—whatever this may come to mean—is neutral, cannot be further from the truth. The neutrality of science is a construct and, in general terms, grossly overrated. And in a world where billions of people live under the line of extreme poverty, where macro policies have left myriad micro-disasters, and where conflicts for resources as vital as water are imminent in the short-run, neutrality is our worst reference. What we need is not to pursue the ghost of neutrality and *scientificity*, but rather to build economic theories for the new political agenda, confronting the issues of cultural diversity, resource sustainability and overall human security.

A central component in many of the discussions on the steps we need to follow in order to achieve a *more scientific* corpus for economic theories is the idea of assembling a dogma-free, politically sterile discipline. Many of the complaints about the current way economics is being handled are that it hides vested interests, conceals political agendas and sequesters propaganda, thus being a sort of ill-constructed doctrine freely imposed throughout the world. Some believe that by ridding the discipline of its *unscientific nature*, all this will fade away.

To some extent the first ideas about the biased nature of economics are correct. For the specific case of the neo-classical theory we can find a great number of suppositions and hypotheses that are unlikely to be the object of generalization. A peasant in rural Oaxaca is doubtfully the embodiment of the utility maximizing rational agent portrayed throughout mainstream economic textbooks. Neoclassical economic theory provides the prototype that is (was) needed for economic survival and expansion in the West, but that does not necessarily have to work elsewhere. However, the problem here is not with the theory itself, but rather with the way it is being implemented. Theory is simply not the same as application. They work at different levels and thus have to be clearly delimited. But by trying to link economics to a scientific discourse, this application is immediately shielded from criticisms and hard to break down, since it no longer is the prescription of a *fragile* discipline but rather the product of *hard* science.

In this sense, it is imperative to recognize that economics has a manifest dual nature: on one hand it can serve as a reinforcement for a set of normative structures by generating instructions about what things *ought to be*; on the other hand, it can be a descriptive set of statements on how the world will react given a very specific collection of conditions and hypotheses. If we are careful enough to segregate these two faces of the discipline in both classrooms and textbooks and remember constantly that application is not the same as theory, many problems can be prevented, specifically the unmeasured use of economic theory as a form of the scientific discourse. Economics is not physics (even physics may not be physics), so we should cut this risky idea at the root.

Closing the gap between the hard sciences and economics could be counterproductive in domains other than the applied. Instead of expanding our knowledge, pursuing this may leave us with less than what we started with. Science, specifically physics, is the art of simplification, a body of approximated *facts* about our surroundings (as one of my teachers once said, “physicists live in a world of point masses, coherent states and spherical cows.”) The economic world is far more complex than what physics, biology or chemistry can depict. In fact, even the physical world is far more complex than what physics can depict. And though the last couple of decades have seen the birth of a new type of physics—namely, complex systems analysis -- dedicated precisely to studying processes which normal, old-school physics has trouble approaching -- this area is still in diapers and is a long way from obtaining important, tangible achievements. Until there is a scientific theory of complex systems, binding economics and science is one of the worst things we can do.

So what can we do with science in connection to economics? Is searching for a connection useful at

all? My answer to the last question is a robust yes. Other sciences may not provide us with the blueprint for a post-autistic economic theory, but they do give us an impressive toolbox for the analysis of real economic systems. Computer simulations, the dynamics of phase transitions, non-equilibrium thermodynamics, genetic algorithms and evolutionary theory are just some of the specific areas that might prove to be useful for the study of economic processes since they are, in many ways, the first theories of complex, real-world behaviors. However, this does not mean that we should be constrained only to what physics and other disciplines may offer. If we are to succeed in the creation of a stronger and *less autistic* economic theory, we need to develop our own toolbox—perhaps inspired by the one provided by other disciplines—applicable to the behaviors of individuals, groups and societies with different historical and cultural backgrounds and without relying on specific behavioral hypotheses. But tools are only tools, and we should not lose sight that they do not constitute the main body of a theory. Economics should be more than mere analysis. If we are to build a theory that escapes the current autistic cycle, we must become more sensitive to the possibility of change, something that cannot come from analysis alone. We need to innovate, not to copy what others have done or at least attempted at doing. The world we live in, with its mountain of problems and needs, requires fresh solutions, not old ones dressed in postmodern clothes.

Notes

1. James K. Galbraith, “Can we please move on? A note on the Guerrien debate”, *post-autistic economics review*, issue no. 15, September 4, 2002, article 2.
2. Claude Mouchot, “Towards a realist epistemology for economics”, *post-autistic economics review*, issue no. 15, September 4, 2002, article 4.
3. Jurgen Habermas, *Ciencia y tecnica como “ideologia”* Tecnos: Madrid (1997).

SUGGESTED CITATION:

Juan Pablo Pardo-Guerra, “Neutrality Is Overrated”, *post-autistic economics review*, issue no. 24, 15 March 2004, article 2, http://www.btinternet.com/~pae_news/review/issue24.htm

Modernist and Pre-modernist Explanation in Economics

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Science likes to imagine that it has vanquished religious approaches to the world, but it remains vulnerable to religious criticism precisely because it remains religious in important respects. The idea that truth is singular, rather than potentially plural, that it is non-arbitrary, and that it is meaningful – all of these dogmas amount to a survival, in the heart of science, of an essentially religious, pre-modern, approach to the world. The silly, post-modern-inspired argument that science, as one more interpretation of the world, stands on equal footing with religious interpretations, can thus (for different reasons than it imagines: the pre-modernism that clings to science is *anti-science*, not its essence) gain a foothold. To avoid confusion with religion, science needs to shed its vestigial religiosity and achieve its modernist potential. As Shakespeare knew, reality is “a tale told by an idiot, signifying nothing” – divine or otherwise: it is not an allegory for God – pre-modern science gets that right – but neither is it an allegory for Nature, or Reason or Progress, for Fitness or Complexity or anything else.

Pre-modern Science: Smith and Coase on Smith

Smith's concept of The Invisible Hand, many have argued, has roots in theology. And in general it is easy to find passages in Smith that seem to rely on the notion of a divinely-ordained harmony in the world. In his essay, "Adam Smith's View of Man¹," Ronald Coase argues, contra Jacob Viner, that Smith's views on psychology in *The Theory of Moral Sentiments* do not, despite appearances, have theological underpinnings. Smith, says Coase, in showing "that particular characteristics of human beings which were in various ways disagreeable were accompanied by offsetting social benefits²," did *not* typically appeal to a divine harmony as an explanation. I think he makes a persuasive case in this regard. Smith appeals not to God but to Nature as the well-designing author of our harmonious-despite-appearances psychology. Coase goes on to say that, in this respect, Smith was essentially an evolutionist before his time: "In all these cases nature, as Adam Smith would say, or *natural selection*, as we would say, has made sure that man possesses those properties which would secure the propagation of the species. (emphasis added).³"

Examine this astonishing statement. To vindicate Smith's scientific credentials, Coase assimilates a patent providentialism to modern science! What is the difference that makes a difference between an evolutionist providentialism and a divine one? And yet, of course, to this day evolutionary theory is marred by such providentialism – a thoroughly anti-scientific excrescence. The idea that evolution promotes the good of the species is more or less gone, thankfully – though it had a long run. But the idea that evolution promotes the good of the organism is alive and kicking. The fact that Darwin himself, in his theory of sexual selection, rejected this more subtle species of providentialism, has not prevented its remaining intact in biology until fairly recently. But we still have prominent evolutionists trying to explain the human brain, human art and science, human morality, by appeal to the survival value of these innovations – and rejecting more or less out of hand explanations that fail to identify such survival value.

The history of the reception of the theory of sexual selection in biology, recently well recounted by Geoffrey Miller in his book, *The Mating Mind*⁴, is a case study in the struggle of the pre-modern and the modern in science, and can serve as a preliminary to a more general discussion of the elements of what I am calling modernist explanation. This will be followed by an account of the struggles of modernism in that most pre-modern of sciences, economics, culminating in a claim that the real scandal that Keynes' work represented for the discipline was its modernism.

Sexual selection, especially the idea of runaway sexual selection developed by H. A. Fisher in 1930⁵, makes clear in a startling way that adaptive traits may hinder the organism's chances of survival. The peacock's tail, famously, reduces the peacock's chances of survival but increases the chances that its genes will spread by making it more attractive to mates. A providentialist may still take solace in the thought that the female preference for long tails remains unexplained, but here is where, in its runaway version, sexual selection becomes strikingly modern, in my terms: the female preference for long tails, so the theory goes, can be self-justifying. If enough females have a bias toward longer tails in mates, the preference for longer tails will be adaptive, by leading to offspring with longer tails who will be preferred as mates! Certain conceptions of science, those I am calling pre-modern, find this sort of theory *prima facie* absurd. It opens the door, patently, to arbitrariness and indeterminacy and unpredictability: why not short tails? The ground starts to slip out from under the explanation: how can a "scientific" explanation make something, in effect, its own cause? And providentialism is obviously shaken to its roots by this sort of thinking, Miller summarizes the reaction to Fisher of the famous biologist Julian Huxley: "He defined evolutionary progress as 'improvement in efficiency of living' and 'increased control over and independence of the environment'. Since sexual ornaments had high costs that undermined survival chances and did not help an animal cope with a hostile environment, Huxley viewed them as anti-progressive, degenerate indulgences⁶." Huxley was not unique: sexual selection, which Darwin regarded as equally as important as natural selection, did not enter the mainstream of biological thinking until more than 100 years after Darwin wrote – until the 1980's.

The Modern

The modernism I want to discuss finds its proper antonym not in the post-modern but in the pre-modern or traditional. The sense I intend is most adequately delineated in Marshall Berman's *All That Is Solid Melts Into Air: The Experience of Modernity*⁷, an enormous and sui generis piece of scholarship. The hallmarks of modernism I want to focus on are, first, the subsumption of ends by means, and, second, closely related, the ubiquity of self-reference. An example will clarify. How does modern art differ from pre-modern art? One important way, surely is that for a good deal of the former, art is not the transparent means to an end outside itself, mimesis or representation, but instead becomes its own subject--art about art, art for its own sake, etc. So art, traditionally the means of representing the world, now seeks to represent its own activity--the end has been subsumed by the means in some sense--and self-reference, with its associated paradoxes invariably moves center stage. An associated idea is that of bootstrap phenomena. Bootstraps, as in "pulling oneself up by one's own" are self-generated or self-caused phenomena. Modern thinking spurns foundations: think of Sartre's notion that man's essence is to have no essence, to be condemned to be free and forced to create his own meaning, willy-nilly. The absence of external foundations, theological or otherwise, makes modernity both exhilarating and terrifying. It would fill reams and reams of paper to do justice to all the ways in which the theme of means become ends, and the associated themes of self-reference and bootstrapping, are played out in area after area of modern thought and thought about the modern.

I don't intend these three elements to capture the richness of Berman's argument, but I believe they are central to modernism in the sense he uses it, although in no way exhaustive of that sense.

Summing up his argument, he writes:

To be modern . . . is to experience personal and social life as a maelstrom, to find one's world and oneself in perpetual disintegration and renewal.

Examples of "Modernist" Explanation

How and where do we see modernism in this sense in scientific explanation? What follows surveys the ground with a collection of examples, some of which will be further elaborated in later sections.

1. *Asset Bubbles*: Why does an asset have a high price today? Because it is expected to have an even higher price tomorrow. Alternatively, why does an asset have a low price today? Because it is expected to have an even lower price tomorrow. (See Keynes' famous Ch. 12 in the *General Theory*⁸, on the stock market as beauty pageant.)

2. *Increasing Returns*: Suppose that there is a positive externality associated with investment, so that the greater the level of aggregate investment, the higher the average level of return on investment. (Investment in knowledge may have this characteristic). Then we can ask, why is the level of investment so high? Because the rate of return on investment is high. But why is the rate of return on investment high? Because the level of investment is high. Alternatively,--in the same economy, same fundamentals-- why is the level of investment so low? Because the rate of return on investment is low. And why is that? Because the level of investment is low. (See Weill, Phillippe, "Animal Spirits and Increasing Returns"⁹).

3. *Conventions*: Why do you, an American, drive on the right side of the road? Because you expect others to do the same. Why do others do so? Because they expect you and others to do so as well. Collectively, then, we drive on the right side of the road because we drive on the right side on the road. Alternatively, why do you, an Englishman, drive on the left side of the road. Because you expect other English people to do the same, etc.

4. *Money*: Why do you give up real goods and services for worthless pieces of paper? Because you expect others to give you (different) real goods and services for the paper in turn tomorrow. Alternatively--same fundamentals--why do you refuse to give up real goods for worthless pieces of

paper? Because you expect others to refuse as well¹⁰.

5. *Co-evolution*: Why does animal A have such long, sharp teeth? Because animal B, its prey, has such a hard carapace. Why does animal B have such a hard carapace? Because animal A, its predator, has such sharp teeth. Alternatively--same fundamentals--why does animal A have such short, dull teeth. Because animal B, its prey, is so soft and mushy. Why is animal B so soft and mushy? Because animal A, its predator, has such short, dull teeth. (See Sigmund, *Games of Life*, on co-evolution.¹¹)

6. *Runaway Redux*: Why is the peacock's tail so long. Because long tails are preferred by females, so the low survival value is offset by the increased chance of mating. But why do females prefer long tails? Because, *given a substantial group of females in the population who prefer long tails*, a female with a gene for preferring long tails will also carry the gene for long tails. Its offspring will thus do better reproductively.

7. *Leijonhufvud's Keynes*: Imagine saving and investment curves as functions of the interest rate. Saving is saving out of full employment income. The intersection determines the Wicksellian natural rate of interest. The investment curve shifts back. The natural rate of interest falls. But bear speculators with inelastic expectations sell bonds to prevent the adjustment. They expect the rate of interest to remain at the old level. Their action leads to a positive gap between full employment saving and investment, a shifted-in saving function due to falling income, and an equilibrium rate of interest higher than the new natural rate which will now prevail even without speculation. The bears are proved correct. Their expectations that the interest rate would not fall have been confirmed, for the nonce. This is Keynes as interpreted by Axel Leijonhufvud in *On Keynesian Economics and the Economics of Keynes*¹². Why is the rate of interest so high? Because it was expected to be high.

8. *19th Century Capitalism*: How can the level of investment be so high while the level of consumption is so low? Means of Production are being produced today to be used to produce means of production tomorrow etc. -- the means have become ends. Alternatively, a low level of investment might make sense despite robust consumption if the level of investment will be low tomorrow -- the means of production needed for the consumption goods industry is high, but the means of production needed to produce means of production are low.

So *modernist* explanations, I shall stipulate, are characterized by:

- a. The ubiquity of self-reference : X because, ultimately, X.
- b. No appeal to fundamentals: God, providence, Reason, Efficiency, Fitness.
- c. The reversal of means/end relationships. Means become ends in themselves.
- d. Bootstrapping phenomena, as in "pulling yourself up by your own" As a consequence, arbitrariness, and multiple equilibria.

Modernism on the Fringe: Marx

In economics, the locus classicus of modernism, indeed the source--in the *Manifesto*-- of the phrase Berman uses as the title of his book, is the work of Karl Marx, definitely far out of the mainstream. Marx, throughout his writing, returns again and again to the essential difference between a pre-modern economy of small producers where, in his well-known terminology, exchange proceeds according to the transparent schema C-M-C' (a commodity of one type, C, is exchanged for money, M, which is in turn used to purchase a different commodity C'), on the one hand; and the modern capitalist economy, whose dynamism springs from its obedience to a diametrically opposed schema: M-C-M', on the other. Here, money purchases commodities (labor and raw materials) which are fashioned into goods to be sold for still more money, so that the process can begin again. Unlike the first, this second process has no natural stopping point, and no foundation or rationale outside of itself, in some pre-existing human needs, the need to satisfy which begins and the satisfaction of which ends the exchange process in the first schema. The mere means in the first schema, money,

has become the end in the second. And what is the money for? To create more money. Thinking about the second schema, we experience the same dizziness, the same hall-of-mirrors effect that I would argue characterizes the modernist turn in all areas of life and culture. (I think of this modernist experience as the Land 'O Lakes effect, after the butter box of my youth, which pictures an Indian woman holding a box of butter, on which is pictured an Indian woman holding a box of butter, on which is pictured . . .) Marx's most succinct definition of capital captures this modernist theme beautifully. He calls capital "self-expanding value". Again we have a self-referring infinity in which means has become end: value creating value creating value Our pre-modern, traditionalist, religious inclination is to ask "to what end?" and to feel frustrated by our inability to get an answer.

Marx argued that to represent the modern capitalist economy as, underneath the trappings of a sophisticated financial system and a highly complex division of labor, nothing but a barter economy operating according to C-M-C', a giant means to satisfy the end of human consumption, was a huge mistake. He raged against the "Robinsonades" of the classical economists -- their attempts to explain the workings of a modern capitalist economy by telling stories about Robinson Crusoe solving his economic problem (*the* economic problem) all alone on his desert island. The idea that *capital*, the dynamic process of self-expanding value whose revolutionary consequences Marx documented, could be understood by analogy with the fishing net that Robinson sacrificed some potential fish today to construct, in order transparently to increase his fish consumption tomorrow -- Marx found absurd and laughable. On the contrary, the capitalism he saw and described was just as capable of producing means of production today to increase the capacity for producing means of production tomorrow, which in turn would make possible further means of production *ad infinitum*--to produce for production's sake, as it were. The economic world he described, in other words, was a modern economy, not the pre-modern and traditional economy of a Robinson Crusoe. To miss this distinction, Marx would have said, is to miss, in effect, everything.

Thoroughly Modern Maynard

Prior to Keynes, however, the mainstream of the profession *did* miss this distinction, and, despite Keynes, still in large part does. What are the "representative agent" models so beloved of modern macroeconomists, real business cyclists and others, if not hi-tech Robinsonades?

I believe Keynes' modernism was pervasive. Its most obvious manifestations, however, can seem at first sight fairly isolated in his work, and have been so treated by his interpreters. The Keynesian who believes Keynes' message to have been well captured in the Hicksian ISLM apparatus has very little use, it would seem, for Keynes' brilliant Chapter 12 in *The General Theory*, "The State of Long-term Expectation"¹³. Here is Keynes' oft-cited discussion of the Stock Market, of Infinitely-lived Asset valuation in general. The modernism of this chapter is hard to miss. Here we are asked to contemplate the bootstrap character of the valuation of an open-ended asset whose price today depends on dividends it is expected to pay, to be sure, but also on the price it is expected to have tomorrow, which latter price will depend on the price it is expected to have further on in the future, and so on ad infinitum. Keynes' asks us to take seriously the notion that the asset's price may very well lose any connection with the "solid" fundamentals and become an airy bubble of self-fulfilling expectations. It is important to see, too, that such an essentially modernist phenomenon Keynes regards not as temporary and bound to disappear just as soon as professionals -- investors knowledgeable about the fundamentals -- appear on the scene, but as comparatively long-lasting and immune to arbitrage:

This battle of wits to anticipate the basis of conventional valuation a few months hence, rather than the prospective yield of an investment over a long term of years, does not even require gulls among the public to feed the maws of the professionals; -- it can be played by professionals among themselves.

Nor is it necessary that anyone should keep his simple faith in the conventional basis of valuation having any genuine long-term validity. For it is so to speak a game of Snap, of Old Maid, of Musical Chairs--a pastime in which he is victor who says Snap neither too soon nor too late, who passes the Old Maid to his neighbor before the game is over, who secures a chair for himself before the music stops. These games can be played with zest and enjoyment, though all the players know that it is the Old Maid which is circulating, or that when the music stops some of the players will find themselves unseated.¹⁴

In contemporary terms, Keynes is talking in this passage about "rational bubbles".¹⁵ They are rational because there is no assumption of stupidity on the part of purchasers of the bubbled asset, stupidity that a canny professional might profit from--and by doing so burst the bubble. The bubbled asset provides a normal return in expectation, with the bubble itself growing at the rate of return, and therefore passes a no-arbitrage or efficient markets test, no matter how wildly divergent from fundamentals its price becomes, and is destined increasingly to become.

Only an infinitely-lived agent could undo, via arbitrage, a bubble on an infinitely-lived asset, which fact puts Keynes' reminder that "in the long run we're all dead" in a whole new light! It is somewhat ironic that the development of rational expectations, a development that in its early stages was used as a battering ram against Keynesian economics, enables us to understand the bootstraps and bubbles of Chapter 12 Keynes with much greater depth and clarity than we could before. The determination of the present not by the past but by the unknown future -- via expectations -- can never be grasped, with all its dramatically modernist implications for our economic lives, as long as we reduce expectations about the future, by means of an adaptive expectations scheme, to some determinate function of the past. Rational expectations -- honestly deployed -- can be a potent generator of modernist outcomes: unfortunately, this is usually noted, if at all, in the footnotes, where one finds the specious arguments for ignoring all but the fundamental solutions covered in the main text.

It is important to see that Keynes, despite twinges of pre-modern revulsion which lead him to propose at one point, half-seriously, that we marry the asset to the asset-holder for life, to defeat speculation and thus the melting of all that is solid into air -- ultimately felt that bubbles could not be disposed of so easily: "This is the *inevitable* result of investment markets organised with a view to so-called liquidity."¹⁶

Contemporary thinkers who have carried on and developed Keynes' modernist views of asset bubbles find the profession scarcely more receptive than it was and is to Chapter 12. The pre-modernism of the profession lies very deep: Look, for one example, at the vehemence of the reaction to Robert Shiller's 1981 article on Stock Market volatility, work directly in the tradition of Chapter 12.¹⁷ In a symposium on bubbles in the *Journal of Economic Perspectives* of a few years back, we find one participant arguing quite seriously that the Great Tulip Mania in 17th century Amsterdam--what Saddam Hussein might have called the Mother of All Bubbles, on previous accounts -- can be parsimoniously explained as a response to changes in fundamentals!¹⁸

But I have argued that modernism is pervasive in Keynes, not a phenomenon confined to a chapter here or there. Here, I want to suggest that we broaden our minds about the Keynesian message and remember, above all that his work stands in two traditions simultaneously, both the mainstream, and the underground, heretical tradition of underconsumption theorists, numbering among its members thinkers such as Marx, Hobson, Major Douglas and Malthus¹⁹ -- some of whom Keynes explicitly acknowledges as progenitors in his appendices to the *General Theory*. The common vision of this latter tradition is the one I have identified in Marx, of a modern capitalist economy subject to stagnation because its ability to produce outruns its ability to consume: the modernist possibility of production for production's sake is here taken very seriously indeed.

Moderns and Pre-Moderns: Keynes, Robertson, and Our Grandchildren

The modernist impulse in Keynes can be observed in the reaction it provoked in his anti-modernist contemporaries. A small but symptomatic incident provides an illustration. Keynes' theory of liquidity preference contained the modernist idea that what determines the interest rate today is speculator's expectations of what it will be tomorrow. This couldn't be the end of the story, D. H. Robertson²⁰ and others (Leontief, famously) insisted: Where were the fundamentals of the process? Robertson's reaction was vehemently anti-modernist:

Thus the rate of interest is what it is because it is expected to become other than it is; if it is not expected to become other than it is, there is nothing left to tell us why it is what it is. The organ which secretes it has been amputated, and yet it somehow still exists--a grin without a cat.²¹

Robertson is not alone among economists in thinking that to establish the bootstrap, foundation-less character a theory attributes to an economic phenomenon is *ipso facto* to refute that theory. Alice in Wonderland is one thing; *reality* cannot have this airy character. If your theory tells you it does, it must need work. As with under-consumption, I cite this aspect of Keynes as an instance of his attraction to modernist explanations. I don't mean to condition my argument on an acceptance of the speculative demand for money any more than on the acceptance of, say, Alvin Hansen's Keynesian Stagnationism. There are contemporary theories of the interest rate which inherit from Keynes the modernist form without the particular content he filled it with.

Keynes himself, like many another great modernist, combines his modernist description with a deep anti-modernist revulsion at the *prima facie* absurdity of the phenomena he is transcribing and, in his weaker moments, with what amounts to a pious hope for an overcoming of modernism and a return to a pre-modern golden age where means have been put back in their place as means to independent ends to which they are transparently related, where bubbles have burst and social life, as it were, makes sense again. (Berman, by the way finds some of these same tendencies in the arch-modernist Marx, who seems sometimes to hold up a vision of socialism as a rest from the ceaseless flux, an overcoming, indeed, of history, a putting-paid to the ceaseless, permanent revolution of modern life.)

This modernist/anti-modernist dialectic in Keynes is most apparent in his 1930 essay, "Economic Possibilities for our Grandchildren"²², where he contrasts the "purposiveness" of contemporary economic life with its potential overcoming in the lives of our grandchildren. The former idea represents still another ringing in Keynes' work of the by now familiar modernist changes. The purposive man, he says:

is always trying to secure a spurious and delusive immortality for his acts by pushing his interest in them forward in time. He does not love his cat, but his cat's kittens; nor, in truth, the kittens but only the kittens' kittens; and so on forward forever to the end of cat-dom. To him jam is not jam unless it is a case of jam tomorrow and never jam today.²³

But after describing and dissecting this modernist purposiveness--interestingly named since it seems almost paradigmatically anti-purposive to pre-modern eyes--Keynes sounds an almost religious anti-modernism. The purposive era will one day end ("when science and the power of compound interest " have solved the economic problem!). And in this future made possible precisely by virtue of the abundance obtained through centuries of purposiveness:

We shall once more value ends above means and prefer the good to the useful. We shall honor those who teach us to pluck the day virtuously and well, the delightful people who are capable of taking direct enjoyment in things, the lilies of the field, who toil not, neither do they spin.²⁴

But Keynes, unlike the great majority of the economics profession in his day and our own, did not allow his anti-modern hopes and values -- delusive or not -- to interfere with his ability to limn the modernist reality in which we live and breathe. The modernist present is highlighted and set off by the stark contrast with the imagined anti-modernist future.

Keynes' modernism is, I believe, the most deeply interesting and at the same time has proven so far the least assimilable dimension of his legacy to the economics profession.

L'Envoi

Taking the contra-positive formulation of Nietzsche's famous declaration, if everything is not permitted, then God is not dead. A determinist science, science that recoils from arbitrariness and meaninglessness, that doesn't permit, in principle, *everything*, that only counts as explanations the pre-modernist subset – keeps God alive, and its adherents children.

Notes

1. Coase, Ronald, 1994. *Essays on Economics and Economists*. University of Chicago Press, Chicago: 95-118.
2. Ibid: 107.
3. Ibid: 109.
4. Miller, Geoffrey, 2000. *The Mating Mind*. Doubleday, New York.
5. Sigmund, Karl, 1993. *Games of Life*. Oxford University Press, New York: 128-31.
6. Miller, op. cit: 58.
7. Berman, Marshall, 1982, *All That Is Solid Melts Into Air*, Penguin Books, New York.
8. Keynes, J. M., 1965, *The General Theory*, Harbinger, New York.
9. Weill, Philippe, 1989. "Animal Spirits and Increasing Returns." *American Economic Review*: September.
10. See Wallace, Neil, 1980. "Overlapping Generations Models of Fiat Money" in *Models of Monetary Economies*. Federal Reserve Bank of Minneapolis.
11. Sigmund, op. cit.: 148, et seq. , on co-evolution.
12. Leijonhufvud, Axel, 1968. *On Keynesian Economics and The Economics of Keynes*. Oxford University Press, London.
13. Keynes, J. M., 1965, *The General Theory*, Harbinger, New York: 147-164.
14. Keynes, 1965, op.cit., p.155.
15. See Blanchard, Olivier and Stanley Fischer (1989), *Lectures on Macroeconomics*, MIT Press, Cambridge, Chapter 5, for a good discussion of this literature. Chapter 5 is titled "Multiple Equilibria, Bubbles and Stability", and it sits uncomfortably in the text, an apparent swerving away from the main track (which of course it is). The last sentence of the chapter states ". . . though we find the phenomena analyzed in this chapter both interesting and disturbing, we are willing to proceed on the working assumption that the conditions need to generate stable multiplicities of equilibria are not met in practice". So it's goodbye to bubble solutions from then on. I wonder how many syllabi in courses that use the text leave this chapter out?
16. Ibid, emphasis added.
17. Shiller, Robert, (1989), *Market Volatility*, MIT press, Cambridge, contains the original article along with Shiller's responses to the veritable cottage industry of critics which grew in its wake. The article is "Do Stock Prices Move too Much to be Justified by Subsequent Changes in Dividends", *American Economic Review* 71 (1981): 421-35, and appears as Chapter 5 of *Market Volatility*.
18. Garber, Peter, (1990), "Famous First Bubbles", *Journal of Economic Perspectives* 4: 35-54.
19. See Bleaney, Michael, *Underconsumption Theories*, (1976), International Publishers, New York, for an excellent overview of this tradition.
20. Robertson, D. H., (1940), "Mr. Keynes and the Rate of Interest" in *Essays in Monetary Theory*, Staples Press, London: 1-38.
21. Ibid.: 25.
22. In Keynes, (1963), *Essays in Persuasion*, Norton, New York: 358-373.
23. Ibid: 370.
24. Ibid: 372.

SUGGESTED CITATION:

Kevin Quinn, "Modernist and Pre-modernist Explanation in Economics", *post-autistic economics review*, issue no. 24, 15 March 2004, article 3, http://www.btinternet.com/~pae_news/review/issue24.htm

A Science Too Human? Economics*

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(This article was commissioned for *La Science en 2004* of the *L'Encyclopédia Universalis*.)

Economic science is far from being exact: the divisions between economists are notorious and their predictions are subject to disputes and revisions. Nor has there been any major discovery in economics in 2003 or in 2002 or in preceding years. One could even ask if there has ever been any; the annual awarding since 1968 by the Swedish Royal Academy of Sciences of a prize for economic science (commonly called the “Nobel prize for economics” although from this it does not follow that it is one) fails to convince.

Nevertheless, economic relations exist; they even constitute an important part of human activities, and a scientific mind can only try to understand them. Generations of economists, of whom the most famous have often had a solid scientific education, have tried; thus one cannot ignore their thought, and eventually the influence that they may have had on the evolution of societies. So doing an update on economic knowledge and on the theories of economists is consistent with a scientific approach – even if, in the end, the results are slight or subject to caution. Knowing that we don't know, or that we know little, is also part of scientific knowledge.

Economic science and science

The term “economic science” is usually used to designate a collection of economic theories. By “science” one generally means a body of knowledge or set of theories about which there is a broad consensus: they are considered to be true on the whole because they have been verified – or at least non falsified – by experience or observation. But in economics it is not uncommon to see different theories coexist for a long time, although they concern the same phenomena and give rise to divergent, maybe opposite, predictions. One can offer two reasons for this, which explain why the situation in economics is radically different from sciences in the strict sense.

1. Economic theories are concerned with relations between men, i.e., relations which are difficult to reduce to a few simple parameters (which all theories do); furthermore, these relations vary in space (they are not the same in all the regions of the world) and in time (societies and customs change, sometimes very fast).
2. The theoretician is not, even if he wants to be, a disinterested party to the societies he studies, because he inevitably has an opinion about them, and thus also an opinion on what is to be done to improve them. This is why economists rarely content themselves with observing what is (or what they believe is) and find it so difficult to refrain from saying what *should be* (what they think is good for the society). This frequency of the normative dimension in the discourse of economists is a source of numerous confusions. Among others, it explains certain reluctances to consider their approach as scientific. It is why it is essential to distinguish the normative from the positive in all presentations linked to the economy – which is as difficult to do as it is to set aside one's opinions.

Economic theory and experimentation

Theories, whatever they may be, are in the beginning the fruit of imagination, of beliefs, or even sometimes of the opinions of those who formulate them. In order to sort them out, so as to retain

only one concerning a given phenomenon, the ideal method is that of controlled experiment, where the studied phenomenon is isolated, by conserving only what is taken into account by the theory, apart from certain perturbations considered as trivial. But in economics such experiments are not possible. As John Stuart Mill noted in 1843:

The instances requisite for the prosecution of a directly experimental inquiry into the formation of character would be a number of human beings to bring up and educate from infancy to mature age ; and to perform any one of these experiments with scientific propriety, it would be necessary to know and record every sensation or impression derived by the young pupil long before it could speak. It is not only impossible to do this completely, but even to do so much of it as should constitute a tolerable approximation. One apparently trivial circumstance which eluded our vigilance might let in a train of impressions and associations sufficient to vitiate the experiment as an authentic exhibition of the effects flowing from given causes. (*A system of Logic*, Book VI. chap 5. point 3)

What is true on an individual level is even more true on the level of society, formed by a multitude of individuals and where the “apparently insignificant circumstances” are obviously much more numerous.

Yet for a long time some economists have, in spite of everything, undertaken “experiments”. It was not, however, until 2003 that the profession took a little interest in this kind of approach (Nobel Prize awarded jointly to Daniel Kahneman and Vernon Smith). This reluctance is easily explained. On the one hand, the first type of experiment, concerning the behaviour of individuals (the subject of Kahneman’s work), revealed that the subjects of the experiments (including students in economics) generally do not react as the theory assumes they do – the “apparently insignificant causes” (routine, a sense of injustice, for example) to which Mill refers, seem, on the contrary, very significant. On the other hand, the second type of experiment, like those conducted by Vernon Smith on the functioning of markets, does not attempt to reproduce what happens in reality because this is impossible; the aim is to test the reaction of individuals placed in particular frameworks, and to look for those that are the most efficient (the approach is *de facto* normative).

A student in economics can, however, go through his entire degree course without having heard of such “experiments” and, obviously, without ever having carried one out – a situation which would be inconceivable in physics, in chemistry, or even in biology.

Knowledge and laws in economics

Certainly astronomers, for example, do not carry out experiments. However they use results reached by sciences that do undertake them and, above all, give an essential place to observation. The regularity of physical phenomena, their repetition, their universal character (in time and space, at least on a certain scale), enable astronomy to explain a great number of phenomena and, even, to make highly accurate predictions.

The situation is very different in economics, where it is impossible to find situations which in the main are differentiated only by the action of one or a few well identified factors – the first step towards the establishment of causal relationships and therefore of laws.

It is why it is not possible to find in economics laws taking the form of precise relations, always verified, between two or more variables, other things remaining unchanged -- this last condition being almost never verified, even approximately. Economists however create confusion by using the word “law” where it does not apply. This is the case, for example, of the “law of supply and demand”, according to which the price of a good whose supply exceeds its demand tends to decrease – or to increase in the contrary case. As soon as one tries to give a more precise content to this so called “law”, one perceives that it is very vague: who makes the price vary? And how? Is this price unique? Can’t it happen that the people who demand the product organise themselves and

refuse to pay a higher price? Can they not purchase other goods instead? In fact, the use of the verb “tend” fits what economists can at the very most hope to achieve: to detect some tendencies in the phenomena studied.

Tendencies rather than laws

The word “tendency” suggests a direction, but not a certain result. The tendency is in itself the manifestation of a law, but this one does not appear clearly because of the existence of disruptive non negligible elements, that can be called “counter tendencies”, and for which it is not possible to isolate their effects. Thus, in place of talking of a “law” of the equalization of profit rates (widely evoked by David Ricardo, John Stuart Mill and even Karl Marx), we will say there is a “tendency”, because this equalization can take time and also resources in collecting information and in comparing various profit rates and the risks with which they are associated.

A more controversial case is that of the tendency for the profit rate to decrease (set out by Karl Marx). The idea is simple: if one thinks that all value comes from labour, and that with time, the accumulated labour (under the form of machines, equipment, offices, etc.), or “dead labour”, increases in comparison to the living labour; then the profit rate must diminish. But here there is only a “tendency”, which can be blocked by an increase of the profit (the share of the living labour appropriated by the capitalists), or by a diminution of the value of the “accumulated” labour (obsolescent or unused equipment). The problem faced by the theoretician, if the diminution of the profit rate is not evident, is to know whether it is due to the existence of counter tendencies or due to the erroneous nature of the theory, i.e., the tendency to diminution does not exist. Because it is not possible to have a controlled experiment to settle the matter, both points of view can continue to coexist indefinitely. (. . .)

Economic theory and self-realisation

There is another aspect of economics that distinguishes it fundamentally from the natural sciences; its theories can transform the world that it studies. This is what we call, not altogether correctly, self-realisation.

The discourse of economists, their predictions and their speculations often turn out to be erroneous. Even so economics influences the people at which it is aimed, and whose actions shape economic life and constitute its substance. People base their economic decisions on more than just “objective” factors such as tastes, available technology and the distribution of resources. They also base their decisions on their beliefs at the time of deciding, for example, beliefs regarding the “business climate” or future prospects. There is also the fact that the government, the big firms and the participants in stock exchanges act according to economics theories – which often take the form of mathematical models – whose form has thus an effect, more or less important, on economic reality, even if this influence is not that assumed by these models. It is this action of the subjective, of beliefs, on the real world that is called, somewhat incorrectly, self-realisation – which concerns the very special case where what has been predicted is realised as a direct consequence of the prediction itself.

Where then is the “reality”, the “real” world that the science intends to analyse and understand independently of the opinions and beliefs of the scientist? Two typical examples show why the answer to this question is not obvious. Assume that one has observed that an upturn in the stock market accompanied by a decrease in interest rates and an increase in household expenditure has been followed, say 4 out of 5 times, by a revival of the economy. If these conditions (stock market upturn, low interest rates, increase of spending) are observed at a given moment, and if the idea according to which they should entail a revival is widely held, then those who share it will, by their actions, make it happen. The revival is thus as much a consequence of shared beliefs, concerning a causal relationship, as it is a consequence of the causal relationship itself (assuming that it really

exists).

Another example concerns the stock market, where the beliefs of investors play a central role. Take the price of options, i.e., the premium that someone has to pay at a certain time to have the right to buy a security or commodity at a future date at a given price (fixed in advance). This premium will depend in particular on the expectations held regarding future fluctuations of stock market prices, on their “volatility”. It is thus that Fisher Black and Myron Scholes (Nobel Prize winners) proposed a formula to calculate the price of options – assuming among other things that stock exchange prices follow a law of the type “random walk”. If all actors in stock markets adopt this formula, attributing the same value to these parameters, then this formula will, very precisely, give the observed prices of the options. But is one to say that Black and Scholes’s model perfectly explains reality, as if it were independent of the model? No, of course not. One can, at the most, note that there is consensus among the actors on the price of options – all agreeing on the price given by the formula of Black and Scholes, which plays the role of a convention. Here the conjunction of the subjective and the objective is at a maximum.

The beliefs of members of a society and the theories and models of the economists – resulting from *their* beliefs – are facts, data, which can play an important role in the economic life. Even if they are difficult to figure out and define, a scientific approach in economics must take them into account – even if this has the consequence of rendering vain or impossible purely mathematical formulations (something the profession has difficulty accepting).

Economy and Mathematics: a serious drift

The prominence that economics books and journals give to mathematics, sometimes very complex, is impressive. Together with physicists, economists are probably those who use advanced mathematics the most. There would appear to be a paradox here: mathematics being synonymous with rigour and precision, how is it that they can play such a role in a discipline where vagueness reigns?. The answer probably lies in the roots of this vagueness: the economic and social world being particularly difficult to grasp schematically, to reduce to simple laws, the temptation is great to flee it and to take refuge in fictitious worlds, in models having little to do with what we can observe (especially concerning forms of social organisation), but which lend themselves to endless mathematical developments.

It is symptomatic that among the journals of reputable disciplines, that it is those of economics that have, by far, the highest proportion of purely theoretical articles, with lots of mathematics but without any concrete data (this also happens in theoretical physics, but much less). Some economists – including famous ones who have sometimes built their reputation on their mathematical expertise – lament this state of affairs. These include the Nobel Prize winners Wassily Leontief, John Hicks, Paul Samuelson, Robert Solow and Joseph Stiglitz. Nevertheless, the recruitment and selection processes for economics teachers and researchers continue to privilege those who demonstrate (particularly in their publications) their knowledge of mathematics, thereby perpetuating the situation or even making it worse. This approach that economics uses to give itself the image of having a scientific character can, however, have the opposite effect, by providing evidence that economists are charlatans and pedants, who try to impress others with their formulas, while the predictions that follow from them leave, at the very least, much to be desired.

Economy and Ideology

The desire to prove that economic science could be different from other human and social sciences, because it can be put into a mathematical form, also leads to aberrations. It was with this desire that the currently dominant theory of the formation of prices was originally proposed in the 1870’s by Leon Walras, who above all sought to determine prices that would, according to him, be “fair”, that is

to say, such that the rights of each person would be respected. In order to do this, Walras conceived a form of social organisation where prices are “called out” by an entity exterior to the traders, and where there is “tatonnement” (without exchanges) until the “faire prices” are reached, (these prices happening to be those which equalize the global demands and supplies). The mathematic form which was progressively adopted to represent this system happens to describe a very centralized economy, where the person who calls out the prices, and makes them vary, plays an essential role, especially by organising the exchanges (they can only occur through him). However, this very special form of organisation is necessary for the demonstration of what is considered to be the principal result of economic theory: there exists a system of prices which equalizes, on the basis of these prices, the supplies and demands. The demonstration of this “existence theorem” – for which the Nobel Prize was awarded to those who first made it, Kenneth Arrow and Gerard Debreu – is undoubtedly a piece of technical wizardry. But it also is the source of a great confusion, because it is systematically presented as proving “mathematically” that a market devoid of hindrances – “perfect” – always leads to a desired situation (where the choices of participants are compatible, and thus realizable). This is absurd, because the demonstration assumes a very centralised form of organisation, the opposite of the idea one generally has of market systems. Only a central planner can possibly be interested by this “theorem”. Even so it is generally what formal economic theory relies upon and pretends relates to markets.

Another example of this kind of absurdity are the “representative agent” models, very fashionable since the 1990’s (one here thinks of another Nobel Prize winner, Robert Lucas). These models suppose that the observed evolution, “macroeconomic”, of some of the basic variables of an economy (such as the GNP, consumption and investment, and employment and price levels) can be assimilated to the choice of a unique individual (obviously imaginary), who is both a consumer and producer, and who decides to divide his available time (present and future) between labour and leisure, and to divide what he produces between consumption and investment. Various mathematical techniques are then used – among others, the optimisation of non linear programs – to determine the share which enables this individual to maximise his (present and future) satisfaction. The result obtained is then compared to that of the economy on the whole (at it appears in statistical series, concerning employment, production, etc.), and theoreticians then try to give the parameters, which characterize the “representative agent”, values which reproduce at best the observed evolutions.

Only ideology – here, the belief in the all-powerfulness of mathematics joined to the virtues of the “market” – can explain why people, otherwise very reasonable, can dedicate their time and energy to these types of models.

Do we need economists and economic theories?

For a critical mind, the situation in economics is like this. On the one hand, it consists of an important accumulation of facts, data and statistical treatments, more or less elaborate, which try to bring out relations or tendencies by relying on relatively simple theories – but between which it is hardly possible to discriminate, because the elements which are not included in each theory are numerous and often non negligible. On the other hand, it consists of endless speculations, which use mathematics like Moliere’s doctors used Latin, trying to make us believe in the scientific character of the discourse, when on the contrary, it is the scientific approach which is sacrificed.

Many economists, however, both undertake sensible studies, on specific points, using a certain number of simple ideas, and yet participate in the speculations of the “grand theory”, when it has (almost) nothing in common with what they do when they carry out their empirical studies. The “simple ideas” that are the basis of these studies are generally old ideas, the fruit of observation and of the experiences of our societies. Thus, there is now a fashion for the “asymmetry of information” (a theme which led Joseph Stiglitz to his Nobel Prize in 2001). This term refers to the fact that in many transactions the parties involved do not have the same information regarding the object of the

transactions. A typical example is that of the relation between insurer and insured, or between a banker and a borrower. Insurers and bankers have always known of the problem and tried to avoid it, but without talking about “asymmetry of information” or trying to put it in a mathematical form. Stiglitz, however, has earned his stripes (and the Nobel Prize) by “demonstrating” that the existence of asymmetries of information profoundly modifies the behaviours and the allocation of resources – a thing we have known for long time. But he also has lead concrete studies, based on observations and available data, where he shows the importance of the asymmetry of information to many important questions of economic policy. To do so, he called upon a few simple ideas, accessible to all, far from the mathematical formulas of his academic publications. The conclusions he comes to, and the policies he recommends, are however far from being approved unanimously, as testifies the controversy – at the end of the 1990’s – between the IMF and the World Bank (where Stiglitz was at the time the chief economist) on the way to tackle the crises which then affected certain developing or “in transition” countries. It is clear that mathematics are not the element which will enable the settling of the controversy, and that behind it lie very different visions of the world and different arguments concerning especially the consequences of the intervention of the State.

It is obviously unsatisfactory not be able to settle such matters. But knowing what are the arguments advanced, and on the basis of what observations and from what data, is part of scientific knowledge. Given the importance of things economic in the life of our societies, this knowledge is necessary, even though it is inevitably limited.

Note

* Translated by Emmanuelle Benicourt and Edward Fullbrook.

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SUGGESTED CITATION:

Bernard Gurrien, “A Science Too Human? Economics”, *post-autistic economics review*, issue no. 24, 15 March 2004, article 4, http://www.btinternet.com/~pae_news/review/issue24.htm

Amartya Sen Again

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In issue 15 of this journal¹, I argued that Sen was a neoclassical economist, and questioned why heterodox economists considered his “capability approach” as a real force in post-autistic economics. Two responses have appeared. First, Ingrid Robeyns² argued that the view according to which the capability approach is undeniably neoclassical, just a variation of standard economics, is “fundamentally mistaken” (i.e., Sen is not neoclassical). Second, Jorge Buzaglo³ admitted Sen was neoclassical, but argued that he was a radical-progressive economist (i.e., Sen applies the conventional apparatus to the advancement of a progressive cause). Curiously, these responses are

contradictory. I will examine each in turn.

Sen's normative framework

Ingrid Robeyns pursues her *Defence of Amartya Sen* by saying :

“the capability approach gives a consistent normative framework to place these scattered studies [of development, development ethics, unemployment, famines, gender inequality, etc] , thus providing a sort of theoretical umbrella for existing empirical work. Moreover, the capability approach makes it very clear how different dimensions, such as commodities, observable outcomes and unobservable opportunities are related.”

This is incorrect. Indeed it runs contrary to Sen's central idea. Remember that Sen's normative approach is deliberately pluralist. This comes from his critique of and departure from utilitarianism. Sen refuses to apprehend well-being in a unilateral way (with the criterion of general happiness or public utility). As he says in *Development as Freedom*:

To insist that there should be only one homogeneous magnitude that we value is to reduce drastically the range of our evaluative reasoning. It is not, for example, to the credit of classical utilitarianism that it values only pleasure, without taking any direct interest in freedom, rights, creativity or actual living conditions. To insist on the mechanical comfort of having just one homogeneous 'good thing' would be to deny our humanity of reasoning creatures. It is like to make the life of the chef easier by finding something which –and which *alone* – we all like (such as smoked salmon, or perhaps even French fries), or some one quality which we must all try to maximize (such as the saltiness of the food). (Sen 1999, p. 77)⁴.

The system he proposes instead is, to the contrary, based on a “plurality of focus” (Sen 1987, p. 63). As Sen explains in “Capability and Well-Being”:

Because of the nature of the evaluative space, the capability approach differs from utilitarian evaluation (...) in making room for a variety of human acts and states as important in themselves (not just *because* they may produce utility, nor just to the *extent* that they yield utility). (Sen 1993, p. 33).

At first, all this seems obvious: who can deny the importance of “self respect”, of “fulfilling one's creativity”, of “avoiding morbidity”, etc.? No one I suspect, not even the utilitarians. So then why did they stick to a single criterion? This is a very old question, as old as ethics, but one which Sen seems to ignore. He contents himself with criticizing the “arbitrary” and “defective” nature (Sen 1987, p. 62) of monist approaches, as if he did not know of this long-standing problem so central to philosophical ethics.

The problem of the multiplicity of ethical criteria

If the multiplicity of ethical criteria has been refused by all great philosophers, utilitarian or not, it is for a very simple reason: it does not permit one to settle all situations with which a philosopher , or a man of action, may be confronted. John Stuart Mill summarized the problem as follows:

There exists no moral system under which there do not arise equivocal cases of conflicting obligation. These are the real difficulties, the knotty points both in the theory of ethics, and in the conscientious guidance of personal conduct. (...) If utility is the ultimate source of moral obligations, utility may be invoked to decide between them when their demands are incompatible. Though the application of the standard may be difficult, it is better than none at all: while in other systems, the moral laws claiming independent authority, there is no common umpire entitled to interfere between them; their claims to precedence one over the other rest on little better than sophistry, and unless determined, as they generally are, by the acknowledged influence of considerations of utility, afford a free scope for the actions of

personal desires and partialities. We must remember that only in these cases of conflict between secondary principles is it requisite that first principles should be appealed to” (emphasis added) (Mill 1861, pp. 157-158).

Adam Smith (admired by Sen) also used a monist criteria :

“ All constitutions of government (...) are valued only in proportion as they tend to promote the happiness of those who live under them. This is their sole use and end. ” (Smith 1790, p. 185).

Emmanuel Kant, a non-utilitarian philosopher with whom Sen claims affinity, was also very clear on this subject:

Considered objectively, there can be only one human reason. (...) So the *moralist* rightly says that there is only one virtue and one doctrine of virtue, that is, a single system that connects all duties of virtue by one principle.” (Kant 1796, p. 81).

Relying on an ultimate criterion enables one to make, in all cases, a choice between two actions, rules, or institutions that are in conflict with one another. In the capability approach, how is one to choose between constructing a school and building a dam? No one knows. Séverine Deneulin seems to feel there is a problem when she explains and asks: “Sen gives a reason for not specifying what is to be counted as relevant capabilities: his concern for pluralism. (...). [But,] if one refuses to take any position regarding the ends that are to be promoted, how then can we know which opportunities have to be given to people in order to improve their quality of life? How can we give people conditions for a better human life, without knowing what a better life consists of?” (Deneulin 2002, pp. 500-501). Thus, Sen’s pluralist perspective is precisely what makes the approach non-operational for policy makers.

This leads us to Jorge Buzaglo’s arguments. He rejects the partition of economics between the categories “orthodox/heterodox”, and asserts that although Amartya Sen is an orthodox economist, he applies the “conventional apparatus to the advancement of a progressive cause”.

Beyond the *homo economicus*?

Jorge Buzaglo believes that the real force of the capability approach is that it enables us to go beyond the “*homo economicus* model of conventional microeconomics [which] does not specify how the preferences of the mind have been themselves determined, and even less how the mind determines the body to perform its “optimal” decisions in the market”. He proposes, following the “Spinozian roots” of the capability approach, to introduce “the notion of an (intersubjective) economic mental space”, which would make parts of standard theory lose their “enchanted power”: “A case in point is the Arrow-Debreu model of general equilibrium, the central piece of conventional economic theory, and the archetype of interaction between atomistic, self-caused minds, and passive bodies (consumers, factor owners, firms, etc.) acting in the markets”.

A few things need to be noted here. First, Amartya Sen has never rejected the Arrow-Debreu model: he simply proposes to adjust it in order to extend it “to the perspective of substantial freedoms” (Sen 1999, p. 119). Second, Sen has never criticized the notion of society that this model represents. Regardless of how consumers are represented, the Arrow-Debreu model of general equilibrium *is not* a representation of “decentralized” or “market economies”, as Sen (Sen 1999, p. 117) and Buzaglo imply. The society represented is a centralized system with price-taker agents and an auctioneer that establishes, through “tâtonnement”, the prices on the basis of the total quantities supplied and demanded. Agents can neither propose prices nor exchange directly.

Changing the representation of the consumer in microeconomic theory (from the *homo economicus* to some other representation) does not change the nature of the society which is represented. It does not remove the “enchanted power” of “markets”, which, in the idealized theoretical case, are

centralised systems.

Because the society described by the Arrow-Debreu model refers to some kind of planned economy, real-life reforms based on this model would entail “more imposed rules”, “given prices”, etc. Yet this is far from being Sen’s position. In fact if one looks at Sen’s works concerning the intervention of the state in the economy, no clear position can be found. Indeed, his stance is highly ambiguous and sometimes contradicts the theoretical framework he retains, that is, the Arrow-Debreu model.

For example, in “Radical Needs and Moderate Reforms”, Sen claims, concerning the economic reforms aiming at India’s “liberalisation” and “deregulation”, that:

The departures are too moderate – and too tolerant of parts of established tradition of economic planning in India. More – rather than less -- radicalism is needed at this time. (Sen 1997, p. 4)

He also says:

The counter productive nature of some of the governmental restrictions, controls and regulations has been clear for a long time. They have not only interfered with the efficiency of economic operations (especially for modern industries), but also have often failed lamentably to promote any kind of real equity in distributional matters. (ibid, p. 9)

Yet, Sen admits in another book written with Jean Drèze:

The government may have a major role in initiating and facilitating market-reliant economic growth (...) This role is easy to understand in the light of economic theory – particularly related to difficulties of initiation, connected with such difficulties of ‘tâtonnement’ (pre-exchange negotiations about market prices, leading to simultaneous production decisions), economies of large scale, importance of technological externalities, and the integral nature of skill formation. The nurturing of an early market mechanism by an active state does not, of course, preclude a more self-sufficient role of the market later on.” (Drèze & Sen 1995, p. 19)⁵.

Drèze and Sen would still have to explain how, theoretically, this “market” could “later on” be “more self-sufficient”...

One can indeed ask: Are the European and the US markets “self sufficient”? To answer these sorts of questions, one has to think about what markets really are, to reflect on their actual “mechanisms”, etc.. And, as far as I know, studying Sen doesn’t help much in tackling these difficult questions.

Conclusion

Although Amartya Sen possesses admirable personal qualities (tolerance, enthusiasm and, as I myself experienced during an OFCE conference in Paris, a great sense of humour), I really do not see how the theory he proposes can be used for analysing real-world issues, nor how his positions in matters of economic policy can be considered “radical” or “progressive”: Sen doesn’t propose anything, except generalities about “freedom”, education, and health. Furthermore, he never treats issues relating to the means to implement these general positions: How are the schools to be financed? What fiscal system leads to the “equality of capabilities”?

Furthermore, wouldn’t it be most peculiar if international organisations such as the World Bank took as a reference point a “progressive” and “radical” economist? Sure, some neoclassical economists take “radical” and “progressive” positions. For example, Joseph Stiglitz harshly criticized IMF policy and supported Argentina’s non payment. But this is far from being Sen’s case, since he has never offered clear and open positions on concrete matters.

Notes

1. Emmanuelle Benicourt, “Is Amartya Sen a Post-Autistic Economist?”, *post-autistic economics review*, issue no. 15,

- September 4, 2002, article 4. http://www.btinternet.com/~pae_news/review/issue15.htm.
2. Ingrid Robeyns, "In Defence of Amartya Sen, *post-autistic economics review*, issue no. 17, December 4, 2002, article 5. http://www.btinternet.com/~pae_news/review/issue17.
3. Jorge Buzaglio, "Capabilities: From Spinoza to Sen and Beyond.: Parts I and II", *post-autistic economics review*, issues no. 20 and 21, June and September 2003, http://www.btinternet.com/~pae_news/review/issue20.htm and http://www.btinternet.com/~pae_news/review/issue21.htm.
4. He had already critiqued monist approaches in *On Ethics and Economics*: "In the utilitarian approach all the diverse goods are reduced into a homogeneous descriptive magnitude (as utility is supposed to be). (...) Not only is there a unified complete view of ethical goodness (weighting the different objects of value vis-à-vis each other), but even the objects of value must be all of the *same* type (singular and homogeneous) in this 'monist' conception." (Sen 1987, pp. 62-63).
5. They also assert: "the formal theory of achievements of the market mechanism is, *implicitly*, much dependent on governmental action" (Drèze & Sen 1995, p. 19).

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SUGGESTED CITATION:

Emmanuelle Benicourt, Amartya Sen Again "", *post-autistic economics review*, issue no. 24, 15 March 2004, article 5, http://www.btinternet.com/~pae_news/review/issue24.htm

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