

History of economic analysis

as

analysis of historic economics

Rodolphe Dos Santos Ferreira

BETA-*Theme*, University Louis Pasteur, Strasbourg

Institut Universitaire de France

History of Economics Research Group Second Workshop

Audiences for the history of economics

21 December 2006

Nanterre University

1. Economics

The title of this workshop of the *History of Economics* Research Group is: “Audiences for the *history of economics*”. Margaret Schabas’ (1992) and (2002) efforts to promote a new name to our discipline seem to have succeeded. I certainly agree with the move, even if I anticipate that we shall go on referring to the old name *history of economic thought*, for at least the good reason that it appears in the title of one of the leading journals in the field, together with other major reviews. However, in order to emphasize my more traditional approach to our activity, and at the same time to introduce a restriction to the much too vast expression “economic thought”, I decided to insert the title of Schumpeter’s (1954) seminal work in the title of my talk. By *history of economic analysis* Schumpeter indeed meant – as he wrote in the very first sentence of his book – “the history of the analytic or scientific aspects of economic thought”. If we follow his approach, we should leave out of our scope the *history of economic ideas*, covering more or less organised economic doctrines, even when associated with an identifiable corpus of theory (such as liberalism, socialism, Marxism, Keynesianism, monetarism, and all other possible *isms*), as well as “spontaneous” economic representations (like “the market economy” or “globalization”). This is in contrast with the position defended by instance by the Editors of *History of Economic Ideas*, when they reject the *dichotomy* between “analysis” and “culture” (even if I readily admit their statement that “both aspects are of equal importance for a wider understanding of the subject”).

I will come back to “analysis”. For the moment, let me take “economics”. The word is defined in the *Merriam-Webster* as “a social science concerned chiefly with description and analysis of the production, distribution, and consumption of goods and services”. Although I would rather use a behavioural definition of economics, in the spirit of the one we owe to Lionel Robbins, this sentence contains the two key words I want to stress: *science* and, again, *analysis*. But the *Merriam-Webster* goes on defining “economics” as “economic theory, principles, or practices”, so that after all the word is not immune from the confusion between economic science and economic ideas. In the following, I will in any case always have the former in mind, and take, at least in this sense, “history of economics as history of science”.

2. Historic economics

There are different legitimate forms of doing history of economics. Here I want just to promote one of them, without excluding other approaches. As an object of history (of science), economics would rather be *historic*, not only in the obvious sense of “known or established in the past”, but also in the sense of “having great and lasting importance”, to quote again the *Merriam-Webster*. But how do we know that some piece of past economic analysis has lasting importance? By referring to its legacy or, more precisely, by evaluating it in the light of the present, which alone ends up conferring significance to it. This view has been developed by Bachelard (1951a, b) as the necessity of a *recurrent history*, in the sense that “it becomes necessary that the history of the sciences be frequently redone, be often reconsidered” (Bachelard, 1951b, p.144). As Chamizo (2007) has recently put it, in a plea for recurrent historic teaching of modern chemistry, “the aim of recurrent history is not to find our concepts already formed at some point in the past, but to reveal the way by which our concepts emerged from other concepts by a sequence of corrections or ‘rectifications’. When a new concept ‘appears’ it introduces a reorganization of the field of study and an evaluation of the cognitive value of previously acquired knowledge” (p.206).

This approach differs crucially from the anti-retrospective position defended by Margaret Schabas (2002), reacting to critiques of her “Breaking Away” which suggest that history can serve present analysis:

“This I think it can never do satisfactorily, as it would result in half-baked attempts to render Ricardo into the calculus. What purpose does this serve other than an intellectual exercise? One might as well play a game of chess. To understand Ricardo is to put him in a historical context, to link his ideas to the intellectual milieu in which he lived. The more the past economic ideas are treated in their own right, rather than as precursors to present theory, the better” (Schabas, 2002, p.220).

A contextualized history of economics is of course in itself a legitimate approach, and sometimes an indispensable step for our understanding of “historic economics”. To give but two examples, it does not make sense to examine the building up of modern mathematical economics in the second third of the last century without taking into account the intellectual context of the *Wiener Kreis* and, two decades later, of Bourbakism, as well as the institutional context provided by the Cowles Commission (see Roy Weintraub, 2000, and his former work on the same topic). If the intellectual context – of the formalist school of mathematics in this example – is ignored, the text itself can be easily misinterpreted, as it often happened to the well-known statement in the preface of Debreu’s *Theory of Value* that “the theory, in the strict sense, is logically entirely disconnected from its interpretations” (1959, p.viii). The second example is given by the quick diffusion of the calibration-simulation methodology introduced by the Real Business Cycles literature, a phenomenon intimately linked to the microcomputer technological revolution of the three last decades.

But contextualization may be quite disappointing. Take for instance the case of Cournot’s *Recherches*. All the information we can gather about the signs of Ricardian influence (denounced by Walras), about the author’s mathematical training at the *Ecole Normale Supérieure*, about the (meagre) impact of the book upon contemporary economists (Ekelund and Hébert, 1990), seems negligible when compared with the knowledge acquired by just (carefully) reading the text itself and then by undertaking an evaluation of its legacy. Only the recurrent approach can make the content of *Recherches* into *historic economics*. Recurrent history invites us to see in Cournot not just one among 35 Proto-Neoclassicals (Ekelund and Hébert, 2002) but the real founder of Neoclassical economics, except as concerns the utility foundation of market demand – an all-important item in the late nineteenth century economics, but less so from the point of view of modern economic theory (Dos Santos Ferreira, 2006). This approach further brings into light the role of Cournot’s analysis of oligopolistic competition as a source of modern (game-theoretic) industrial organization.

In an often quoted passage of his Introduction to the *Grundrisse*, Marx wrote that “the difficulty lies not in understanding that the Greek arts and epic are bound up with certain forms of social development. The difficulty is that they still afford us artistic pleasure and that in a certain respect they count as a norm and as an unattainable model” (1857, p.111). Of course, art and science should not be amalgamated. Yet we find in the history of science the same duality of objectives: on one hand to understand how the works of the past are bound up with certain intellectual and institutional formations, on the other to understand how they become historic through their links with subsequent strata of scientific production.

In the entry “Aristotle” of the *New Palgrave*, Moses Finley expresses his discomfort with a retrospective reading of the Aristotelian work, which pretends to detect in it pieces of economic analysis:

“[...] On a straight reading of Aristotle’s words, the conclusion seems clear to me that he never pretended to examine the price mechanism or any other aspect of market exchange *as it was practised*. He was offering a normative ethical analysis: much that went on in practice was unethical on his definition and therefore outside his discourse.

In sum, there is no economic analysis in Aristotle, not even in intention; judgements of his performance on that score or attempts to interpret his words so as to rescue them as economic analysis are doomed from the outset” (Finley, 1987, p.113).

Finley is certainly right in considering that there is no analysis of the price mechanism or any other aspect of *market* exchange in Aristotle's writings. But the scope he attributes to economic analysis is much too narrow: it does not seem to cover bilateral (as opposed to market) exchange, which is precisely the type of exchange figuring explicitly in the discussion of justice of the *Nicomachean Ethics*. Aristotle's argument has accordingly been considered by several historians of economics in the light of the Neoclassical analysis of isolated exchange developed in mathematical terms by Jevons or Edgeworth (Soudek, 1952; Lowry, 1969; Jaffé, 1974). More significantly, Aristotle's discussion of equity in exchange, with equity defined in terms of conformity with a mathematical structure (proportion), may be easily brought together with the *normative* treatment of the bargaining problem by non-cooperative game theory (Dos Santos Ferreira, 2002). This is a good example of the recurrent re-interpretation and re-evaluation of historic economics, according to successive developments of live economic science.

3. Analysis

Analysis occurs at three levels in our story. To begin with, past economic *analysis* forms, as we have seen, the object of our historical inquiry. It is contained in texts constituting the material to be in turn *analysed*, in order to interpret and evaluate their economic theoretic content. This task is possibly achieved by appealing to instruments of live economic *analysis*. As concerns the last point, "rendering Ricardo into the calculus", to use Margaret Schabas' words, is admittedly not a necessary step for recurrent history. But rendering Marx into the linear algebra, for instance, may help a lot. The duality of commodity in its natural and value forms, and the corresponding duality of concrete and abstract labour, which appear at first inspection as a simple dialectical coquetry, may then be seen as properties of an elaborate conceptual framework, roughly corresponding to (or at least related with) the duality of the commodity and price spaces in linear production models (Bródy, 1970, 1.3.3; Dos Santos Ferreira, 1990). Also, rendering Quesnay into the matrix formalism (Phillips, 1955; Samuelson, 1982) is more than a simple curiosity. The graphic formalism of the *Tableau Economique* is indeed isomorphic with the Leontief matrix formalism: the diagonal and horizontal lines (with the associated data) correspond to the current and capital accounting entries of Leontief matrices *A* and *B* (Dos Santos Ferreira and Ege, 2000).

But whether we resort or not to concepts and instruments of modern economic theory, thoroughly analysing the relevant texts of some economist of the past – both in their detail and in their overall organization – seems an indispensable task, which cannot be circumvented by putting all the emphasis in that economist's system or "vision" (Schumpeter, 1954, p.41). This is by no means a simple task. In order to accomplish it, a convenient (maybe necessary) working hypothesis is the consistency assumption, which plays a role similar to the one of the rationality postulate in Neoclassical economics. We all know that flesh and bone individuals are not optimizing machines, yet we may reproduce observed patterns of economic behaviour by assuming that economic agents act rationally. Similarly, we all know that it happens that we think and write inconsistently, but we are doomed to serious misinterpretation if we are ready to accept that obscurity in any passage or relationship between passages is the fruit of inconsistent reasoning. Keynes' *General Theory* offers one of the best examples of that danger. It is common to admit that it is "a badly written book", based on "a model [which] contains logical errors, [so that] later interpreters are given considerable freedom in deciding where "repairs" should be undertaken" (Leijonhufvud, 1968, pp.10-11). From this perspective, "the aim of interpretation is the rational reconstruction of the text in order to recover the author's original meaning" (Gerrard, 1991, p.277). But both the aim and the freedom originating in pretended inconsistencies are questionable.

There are other ways of tackling the question. Stressing the importance of context, more precisely, the fact that both “the writing and reading of texts are processes which are socially and historically contingent”, Gerrard (1991, p.282) defends an interpretation of the *General Theory* according to what he calls the hermeneutic approach, characterized in terms of four propositions:

- “1. Interpretation is a multi-dimensional process with multiple objects.
2. Interpretation is not an objective process but this does not imply that anything goes.
3. The significance of an interpretation is not determined by correspondence with the author’s original meaning.
4. The existence of multiple interpretations is not a problem” (*ibid.*, pp.284-285).

Since Gerrard considers context as applying not only to the writing process – which has taken place only once – but also to the reading process – which is undertaken again and again – we should not be surprised to find that these propositions, in particular Propositions 3 and 4, do not conflict with the recurrent history approach. As to Proposition 2, the statement that anything does not go is justified by Gerrard on the basis of the social and historical contingency of interpretation. I would rather take the statement in a normative sense, emphasizing that “the text constrains the range of potential meanings”, even if it cannot in general *completely* “determine the actualisation of any specific reading by the reader” (*ibid.*, p.285). And I would certainly take quite seriously the constraint imposed by textual evidence. For that reason, I prefer a pedestrian “analysis” to an aerial “interpretation” of the relevant texts.

4. By whom?

The recurrent and analytical approach to history of economics which I am defending – and which, once again, is by no means exclusive – can hardly be practiced by historians who are not familiar with modern economic analysis. By this statement, I do not mean a mere general acquaintance with economics in its present state, nor a simple economic training, however serious. I mean a reasonable degree of intimacy with the field that is relevant for the texts to be analysed, and preferably some research experience in the scientific production of that field. Such familiarity is indispensable in particular to detect significant contributions that have been neglected, or even ignored, by contemporaries and successors of their authors.

Take for instance the case of Maurice Potron, whose work has been recently “discovered” by Gilbert Abraham-Frois and Emeric Lendjel (Abraham-Frois and Lendjel, 2004; Bidard, 2006). In the three years preceding the *First World War*, Potron built a complete Leontief-Sraffa model, showed the duality of the corresponding price and quantity problems, and proved existence of a solution by applying the Perron-Frobenius theorems, which he extended from positive to non-negative matrices. Part of this contribution, in particular the introduction of these theorems in economic analysis and other technical aspects, had to wait until the end of the *Second World War* to become effective (without any reference to his work). The point I wanted to stress is that a detailed knowledge of the literature of the Fifties and Sixties on linear production models is necessary to identify all the analytical anticipations of Potron and to attempt a fair evaluation of his contribution.

I would also like to emphasize that this case is outside the alternative mentioned by Margaret Schabas in her critique of retrospective history of economics: “The more the past economic ideas are treated in their own right, rather than as precursors to present theory, the better” (Schabas, 2002, p.220). Indeed, in order to treat the economic contribution of Maurice Potron in its own right, we must in fact situate it in the vicinity of its fellow contributions of the second half of the last century, of which it can hardly be called a “precursor” (not even in the sense that the *Tableau Economique* is a precursor of the input-output table). It is easy to find other examples of the same type of situation. For instance, in what sense can we see in

Launhardt's model a precursor of Hotelling's model? The latter is indeed nothing but a special case of its predecessor of four decades, at least as far as the pricing game is concerned (Dos Santos Ferreira, 1998).

5. For whom?

We come at last to the theme of this workshop: *audiences*. For whom do we want to analyse historic economics? Although I think that we have a lot to learn from historians and sociologists of science (possibly something to teach them too), and although I have always defended that opinion in my university – where, by the way, science studies belong to the economics department, not the other way round –, I disagree with Margaret Schabas' idea “that we historians of economics would actually garner more respect from the economics profession if we distanced ourselves from mainstream economists and acquired a more independent voice” (Schabas, 2002, p.219). I think that the opposite conjecture is more probably true.

It is clear that we have been losing audiences in the economics profession during the last two decades. We find less and less courses of history of economic thought in undergraduate teaching programmes, we find less and less papers on history of economics in generalist journals and generalist conferences, we find less and less acceptance from referees for historical developments in our economic theoretical papers, and we find less and less economic theorists (like Schumpeter himself, Samuelson or Shubik) who are interested in history of economic analysis or actually practicing it. This is part of a more general phenomenon touching “culture”, at least as we used to conceive it. But I think that there is in addition a local factor of this regression. Economic analysis, empirical as well as theoretical, has become more and more technical, creating a need for more and more specialization, and becoming more and more inaccessible to pure historians of economic thought. This creates an incentive for them to specialize in works of a more distant past, and also to resist to any recurrence temptation. The consequence is that the output of historians of economics appears more and more divorced from mainstream economists' concerns, except when the latter have a genuine interest for “culture”, and consequently have towards the history of their own discipline an attitude which is not very different from the one of any other scientist with the same cultural interest.

This is precisely where my emphasis on recurrence and analysis goes in. If we approach historic economics with our feet firmly grounded on live economic analysis, we have more chances of finding an echo among fellow economists. Often, this is simply because we are speaking the same language. But it may happen that history serves present analysis, contrary to Margaret Schabas' conviction that it can never do so satisfactorily. I will give just an example. My “antiquarian” interest for Launhardt made me discover that, by introducing good-specific transportation costs in the spatial oligopoly, we may elegantly and efficiently consider both vertical and horizontal differentiation within the same simple model – a device that may be used for strict theoretical purposes (Dos Santos Ferreira and Thisse, 1996).

References

- ABRAHAM-FROIS, Gilbert, LENDJEL, Emeric (2004) – *Les œuvres économiques de l'Abbé Potron*, Paris, L'Harmattan.
- BACHELARD, Gaston (1951a) – *L'activité rationaliste de la physique contemporaine*, Paris, Presses Universitaires de France, 1951.
- BACHELARD, Gaston (1951b) – “L'actualité de l'histoire des sciences”, in *L'engagement rationaliste*, Paris, Presses Universitaires de France, 1972, 137-152.
- BIDARD, Christian (2006) – Review of Abraham-Frois and Lendjel (2004), *European Journal of the History of Economic Thought*, 13, 163-167.
- BRÓDY, András (1970) – *Proportions, Prices and Planning: A Mathematical Restatement of the Labor Theory of Value*, Amsterdam, North-Holland.
- CHAMIZO, José A. (2007) – “Teaching Modern Chemistry through ‘Recurrent Historical Teaching Models’”, *Science and Education*, 16, 197-216.
- DEBREU, Gérard (1959) – *Theory of Value*, Monograph 17 of the Cowles Foundation, New York, John Wiley & Sons.
- DOS SANTOS FERREIRA, Rodolphe (1990) – “De Petty à Sraffa, ou le triomphe de l'Arithmétique Politique”, in *Sraffa trente ans après*, Richard Arena and Jacques-Laurent Ravix, eds., Paris, Presses Universitaires de France, 77-97.
- DOS SANTOS FERREIRA (1998) – “Launhardt and Hotelling”, *Recherches Économiques de Louvain*, 64, 97-109.
- DOS SANTOS FERREIRA, Rodolphe (2002) – “Aristotle's Analysis of Bilateral Exchange: An Early Formal Approach to the Bargaining Problem”, *European Journal of the History of Economic Thought*, 9, 568-590.
- DOS SANTOS FERREIRA, Rodolphe (2006) – “La filiation Cournot-Walras”, Paper presented to the Workshop *Regards croisés sur Léon Walras*, Association Internationale Walras, Université de Lausanne.
- DOS SANTOS FERREIRA, Rodolphe, EGE, Ragip (2000) – “The Net Product in the *Formule du Tableau Economique*: Lessons From a Formalism”, Paper presented at the *Fourth Annual Meeting of the European Society for the History of Economic Thought*, Graz.
- DOS SANTOS FERREIRA, Rodolphe, THISSE, Jacques F. (1996) – “Horizontal and Vertical Differentiation : The Launhardt Model”, *International Journal of Industrial Organization*, 14, 485-506.
- EKELUND Jr., Robert B., HEBERT, Robert F. (1990) – “Cournot and His Contemporaries: Is an Obituary the Only Bad Review?”, *Southern Economic Journal*, 57, 139-149.
- EKELUND Jr., Robert B., HEBERT, Robert F. (2002) – “The Origins of Neoclassical Microeconomics”, *Journal of Economic Perspectives*, 16, 197-215.
- FINLEY, Moses I. (1987) – “Aristotle”, *The New Palgrave, A Dictionary of Economics*, John Eatwell, Murray Milgate and Peter Newman, eds., London, Macmillan, 1, 112-113.
- GERRARD, Bill (1991) – “Keynes' *General Theory*: Interpreting the Interpretations”, *Economic Journal*, 101, 276-287.
- JAFFÉ, William (1974) – “Edgeworth's Contract Curve: Part 2. Two Figures in its Protohistory: Aristotle and Gossen”, *History of Political Economy*, 6, 381-404.
- LEIJONHUFVUD, Axel (1968) – *On Keynesian Economics and the Economics of Keynes*, New York, Oxford University Press.
- LOWRY, S. Todd (1969) – “Aristotle's Mathematical Analysis of Exchange”, *History of Political Economy*, 1, 44-66.

- MARX, Karl (1857) – *Grundrisse, Foundations of the Critique of Political Economy (Rough Draft)*, Translated with a Foreword by Martin Nicolaus, Harmondsworth, Penguin Books, 1973.
- PHILLIPS, Almarin (1955) – “The Tableau Économique as a Simple Leontief Model”, *Quarterly Journal of Economics*, 69, 137-44.
- SAMUELSON, Paul A. (1982) – “Quesnay's 'Tableau Economique' as a Theorist would Formulate it Today”, in *Classical and Marxian Political Economy, Essays in Honour of Ronald L. Meek*, Bradley and Howard, eds., London, MacMillan, 45-78.
- SCHABAS, Margaret (1992) – “Breaking Away: History of Economics as History of Science”, *History of Political Economy*, 24, 187-203.
- SCHABAS, Margaret (2002) – “Coming Together: History of Economics as History of Science”, in Roy Weintraub, ed., *The Future of the History of Economics*, Duke University Press, 208-25.
- SCHUMPETER, Joseph A. (1954) – *History of Economic Analysis*, London, George Allen & Unwin.
- SOUDEK, Josef (1952) – “Aristotle's Theory of Exchange: An Inquiry into the Origin of Economic Analysis”, *Proceedings of the American Philosophical Society*, 96, 45-75.
- WEINTRAUB, E. Roy (2002) – *How Economics Became a Mathematical Science (Science and Cultural Theory)*, Durham, NC, Duke University Press.