Ce **texte** paru comme **Cahier** no **93-14** a été présenté le 28 juin 1993 au 20e congrès annuel de History of Economics Society tenu à l'Université Temple (Philadelphie). L'auteur remercie Lawrence Moss, Robert Nadeau et Bruce Toombs de leurs très utiles commentaires ainsi que le CRSH et le Fonds FCAR pour leur aide financière.

Ronald Coase on Methodology

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Ronald Coase is usually considered anything but a methodologist. Thus, it is not surprising that, in the introduction to "How Should Economists Choose?", which is the only paper Coase wrote on a methodological topic, he readily confessed his relative ignorance of philosophy of science, candidly observing that "Words like epistemology do not come tripping from my tongue" (HSEC, 6). However, given the importance of this Nobel Prize winner's contribution to the renewal of theoretical thinking in economics, everyone should admit that his infrequent reflections on methodology, however crude they might look to methodologists, undoubtedly merit close consideration. Whatever methodological clarification these reflections might procure, it is surely worthwhile briefly to analyze them, with the hope of understanding a little better some dimensions of Ronald Coase's way of thinking and, more specifically, of emphasizing some implications of the close relationship between his methodological views and his radical conception of the market. With this in mind, I will first illustrate how far these methodological views seem, at first glance, to be dominated by a fundamental commitment to a straightforward realism; I will then show that they are instead subordinated, in a more complex way, to what I have just called Coase's radical conception of the market and, in conclusion, I will point out some questions raised by this situation.

How to be faithful to the real world?

If there is such thing as a leitmotiv with methodological content in the writings of Ronald Coase, it is surely his constant reaffirmation of the importance for economists of never departing from the real world. Thus, when assessing Coase's views on methodology, it is tempting to characterize them by referring to this pervasive dimension of his thought. For example, he flatly rejects the notion of utility maximization (ECD, 208; FML, 2, 5) mostly because "the rational utility maximizer of economic theory bears no resemblance to the man on the Clapham bus or, indeed, to any man (or woman) on any bus" (FML, 3-4). Among the economists of the past, his heroes are Smith and Marshall, the economists to each of whom he has dedicated one of his two main contributions to the history of economic thought (ASVM, MM). There is no doubt that his admiration for them is largely due to the fact that both pay particular attention to concrete situations. Coase notes, for example, that "Adam Smith would not have thought it sensible to treat man as a rational utility-maximiser." (ASVM, 545) Similarly, he maintains that Marshall emphasized induction because "his aim was to understand the working of the real economic system, a system whose operations we could observe in the factories, in the streets and in the homes of ordinary people." (MM, 28) Furthermore, he presents this latter economist as a "great collector of economic facts" drawn from "visits to factories" and "questioning businessmen and workers" (*ibidem*).

Coase was so convinced of the importance of investigations similar to those he appreciated in Marshall's work that he wrote a very carefully documented paper about the management of lighthouses intended to show that the way so many economists used this example was totally misleading and without resemblance with "what happened in the real world". Indeed, when looking at the work of his fellow economists, Coase was constantly irritated by the superb "detachment from the real world" (FML,23) of those who freely and misleadingly used examples like lighthouses, but also like sparks from locomotives (PSC, 29-39; FML, 22-23) or like orchards and bees (FML,29) without conducting serious "investigation of how the economy actually operates" (FML,28-29). Coase's concern with such "detachment" explains his disdain for what he describes as "blackboard economics" (FML, 19, 28), focussing in this very suggestive expression his main objection to the abstract models devised by so many economists. For example, he objects to the ingenious analysis proposed by Joan Robinson in her *Economics of Imperfect Competition* , by saying that it permits one to "cover the blackboard with diagrams [...] without the need to find out anything about what happened in the real world" (NF-M, 51).

At this point, one could possibly be tempted to describe as paradoxical the fact that, in his most famous paper, "The Problem of Social Cost", this enemy of blackboard economics was the initiator of theoretical developments which were later reformulated as the so-called "Coase Theorem" -- a theorem which implies highly abstract assumptions about a world without transaction costs. But it is easy to show this paradox to be only apparent since, whatever the misinterpretations concerning the Coase theorem, it is clear that, while Coase referred to a world without transaction costs, this was to better emphasize the fact that, "in the real world", transaction costs *are* everywhere. Even in this highly theoretical paper, Coase strongly denounced the frictionless neoclassical world of perfect competition.

In "The Nature of the Firm", the other paper on which his considerable notoriety is based, this dedication to the "real world" is still less equivocal. When the occasion was offered to him to reassess this paper in three largely autobiographical lectures given for its fiftieth anniversary, in 1987, Coase described in great detail how its essential ideas had matured when he was in the United States on a year-long scholarship, spending his time "in factories and offices rather than in the writings of economists" (NF-M, 52). In this paper, Coase's explicit objective was to provide a definition of the firm which was not only as "manageable" as the usual neoclassical definition, but which was also "realistic in that it correspond[ed] to what [was] meant by a firm in the real world" (NF-T, 18). In his 1987 reassessment lectures, he drew attention to the importance of this last remark, which he integrally maintained and even emphasized: "In interpreting the argument of my article it is necessary to keep in mind that my aim was to discover a ‘realistic assumption’" (NF-M, 52).

If this preoccupation for realism was such a constant feature of his various papers, it is not surprising to observe that in the above mentioned single paper which he devoted to methodology, Coase did not hesitate, as he proudly recalled in his 1987 autobiographical lectures, to take a stand against Friedman's famous thesis on the realism of assumptions. Against Friedman, Coase underscores the importance of realism in assumptions, because it "forces us to analyze the world that exists, not some imaginary world that does not." (HSEC, 7). In his 1987 lectures, he repeats this condemnation of Friedman's thesis because "what this comes down to is that when economists find that they are unable to analyze what is happening in the real world, they invent an imaginary world which they are capable of handling" (NF-M, 52). In this connection, Coase does not hesitate to appeal to the good judgment of economists: "Faced with a choice between a theory which predicts well but gives us little insight into how the system works and one which gives us this insight but predicts badly, I would choose the latter and I am inclined to think that most economists would do the same" (HSEC, 6). And to vindicate his case, he suggests that those economists would not be sympathetic at all to a theory that would predict very efficiently the American government's policies on oil and natural gas but that would do so by assuming that the "aim of the American government was to increase the power and income of the OPEC countries and to reduce the standard of living in the United States" (HSEC, 7). For Coase, it is important that theories gives us valuable insights and such insights have to be based on realist assumptions and not on paranoid or incredible suppositions like the one on which this fantaisist theory is based.

While they are remarkably consistent, however, the features of Coase's thought emphasized so far can hardly be said to be very significant from a methodological point of view, with the possible exception of this last argument about the choice between a theory which predicts well and a theory which provides insight. It is not clear whether the priority he gives to the real world constitutes a methodological position rather than an economist's strong commitment to do a serious job. There is little doubt that, for Coase, economics is a serious matter and that it does not sound very serious to refer to the case of lighthouses without first checking how lighthouses work in the real world. Nor does it seem very serious to devise policies with the help of complex diagrams on the blackboard but with little regard for their actual implementation, or, for that matter, to build a theory on assumptions without being bothered by the fact that everyone acknowledges their falsity. But an economist's firm conviction that his science is nothing but a serious attempt to understand the real economic world is not sufficient as such to transform this economist into a full fledged methodologist.

What makes a discussion of Coase's methodology more rewarding, however, is the fact that, at least in the paper I have described as his only methodological paper, Coase develops more original, if not more defendable, methodological theses. One notable aspect of his rejection of Friedman's thesis concerning the realism of assumptions is the following. Most of those who reject Friedman's thesis — Hutchison, Koopmans, Samuelson and many others — base this rejection upon their desire to *increase the empirical content* of the theory, by indirectly testing the assumptions instead of only the predictions. Since Coase was so committed to a view according to which economists' statements should faithfully reflect what is going on in the real world, one might expect him also to insist on the crucial importance of testing all aspects of a theory. But oddly enough, Coase seems to reject the very usefulness of testing a theory in economics. More precisely, in his view, assumptions should be realistic, but this requirement does not imply that testing plays any role in a theory's acceptance. If Friedman's paper was taken as a target by Coase, it was more because it pretended to characterize economics as a so-called "positive" science related to the real world only through a testing process, than because of its refusal to *test* assumptions. For Coase, an economist's agreement with a theory ought not to be based on testing processes that tend generally to be inconclusive. But, in such a context, in what sense can a theoretician be faithful to the real world?

Testing ideas through the market

Coase mentions (HSEC, 15) that, at a meeting held before the publication of Friedman's paper in 1953, he had himself had occasion to debate with Friedman rather informally and unsuccessfully. It was only when he met Thomas Kuhn later in Chicago that Coase grasped clearly the very meaning of his own position. Consequently, in his own methodological paper of 1982, he refers to one of Kuhn's well-known theses to claim that the adoption of an economic theory is never based on quantitative tests. [################: Attention through a telephonic conversation in Fall 1997, Th. Kuhn said me that this relativistic interpretation of his position about Kuhn was unfair to him. So, Revise the text with rereading Coase'paer related to Kuhn#####################] Everyone knows that Kuhn's thought is subject to divergent interpretations. Thus, it is interesting to note that Coase emphasizes the most relativistic dimensions of Kuhn's arguments when he concludes that "quantitative methods do not appear to be used in the natural sciences in a way essentially different from the way they are used in economics" (HSEC, 15). Clearly, Coase could not have derived such a conclusion from Kuhn without underestimating the importance of some aspects of Kuhn's argument. Indeed, while abundantly illustrating the fact that scientists normally do not reject a theory when faced with discrepancies betweeen the mesurements observed and those that are required by the theory, Kuhn maintained that "experience has repeatedly shown that, in overwhelming proportion, these discrepancies disappear upon closer scrutiny." (ET, 202) Coase, who himself quotes this very passage, claims that there is good reason for economists -- as for physicists, according to Kuhn -- simply to ignore these discrepancies, but he does not discuss the crucial question of whether these discrepancies will normally disappear in economics as they do in physics. Indeed, Kuhn observes on the next page of the quoted paper that "sometimes, as has often happened in chemistry and astronomy, more refined experimental techniques or a finer scrutiny of the theoretical approximations will eliminate the discrepancy entirely". It is true that some discrepancies "repeatedly defied analysis" (ET, 203), but Kuhn characterizes such discrepancies as "anomalies" precisely because their persistance does not correspond to what is "normal". In any case, according to him, "few anomalies resist persistent effort for long" (*ibidem*) and, when they do, far from being placidly neglected by physicists, they generate a "scientific crisis" whose solution requires what Kuhn calls "a scientific revolution".

Coase ignores these dimensions of Kuhn's thought and rather construes this thought as a theory of choice (among scientific theories) suggesting that quantitative and qualitative studies aim not only "at enlarging the understanding of those who believe in the theory but also at attracting those who do not believe in it and at preventing the defection of existing believers." (HESC, 17) I have no doubt that Kuhn's analysis can be (and has often been) interpreted in such a way and my aim here is not to discuss Coase's rather one-sided interpretation of Kuhn. I will rather underline the fact that Coase's methodological conclusions unexpectedly pointed to what was to become a leitmotiv for Don McCloskey during the next ten years: "the definiteness of quantitative studies enables them [economists] to make their point in a particularly persuasive form" (HSEC, 17). Given this role of persuasion, Coase completed his methodological paper with a few sociological considerations underlining how theory choice can be influenced by institutional and ideological factors and by the sheer maximization of self-esteem.

Now the problem with such relativistic views is not that they come from somebody who placed so much value on close relations between ideas and the "real world", since, in one sense, they derived precisely from keen attention to what was going on in the real academic world. The problem is rather that these relativistic views are presented as the essential conclusion of a paper entitled "How Should Economists Choose?" This is a problem because, since he does "not believe that, for the most part, economists could act in any other way" (HSEC, 15), Coase's answer to the question discussed in his paper is that economists are right to choose a theory the way they actually do, *i.e.* without waiting for any empirical test of the theory. But if such is the case, on what ground is it possible to identify from among theories those that correspond most closely to what is going on in the real world? For example, Coase is very critical about the way the respective theses of Chamberlin and Robinson were adopted by economists without reference to any testing. He observes about these theses: "They certainly gave us a lot to put on the blackboard [...] They seemed to give us a better understanding of how a competitive system works, but whether this was really so is another matter" (HSEC, 12). How can one denounce blackboard economists in the name of the real world, while maintaining that economists are right to adopt a blackboard theory without checking if it corresponds to the real world since, given Coase's relativistic conlusions, they simply cannot do otherwise? At this point Coase's methodological approach seems to be trapped in a contradiction — at least in an apparent one — for which an explanation is clearly needed.

To understand how an economist so devoted to empirical agreement with the real world has been attracted to such an extent by an ultra-kuhnian relativism, one needs only to have a closer look at the terms in which this relativism is expressed. Coase emphasizes the "persuasive" power of quantitative theories upon their potential "believers" because, according to him, they "perform a function similar to that of advertising and other promotional activities in the normal product market" (HSEC, 17). And he quickly explains this view: "what we are dealing with is a competitive process in which purveyors of the various theories attempt to sell their wares" (*ibidem*). For Coase, whether it is a new economic theory, a new social policy or a new technique of production, any *idea* must be treated as a good in a market.

Actually, Coase adopted a similar approach in two other papers. The very title of one of them "The market for goods and the market for ideas" points directly to such a view. While discussing the question of freedom of speech in the context of a debate on the First Amendment, Coase proposes a parallel between these two "markets". According to him, consistency requires that to the same degree that government intervention is judged appropriate in the market for goods (to protect consumers from fraud, for example), some government intervention, even if this implies some limitation of freedom of speech, should also be appropriate in the market for ideas (to protect people from being deceived). The other paper, entitled "Economics and Contiguous Disciplines", discusses the invasion -- exemplified in Gary Becker's works -- of economics in the other social sciences. For Coase, this phenomenon is a clear case of competition between ideas. More precisely, it is a case of competition between various methods or approaches to occupying fields defined by the subject matters of other disciplines. According to Coase, such a phenomenon should be analyzed with the tools appropriate to analysing markets.

It would be beyond the scope of the present paper to discuss these two texts which are not *stricto sensu* of a methodological character, but alluding to their characteristic approach helps us to understand that, for Coase, it is not at all unnatural to construe a *methodological* question as an *economic* one. I suggest that this is precisely what he did in his 1982 methodological paper. In fact, he acknowledges this in his conclusion where he writes, "Paradoxically, the approach to the methodological problem in economics that is likely to be most useful is to transform it into an economic problem." (HSEC, 21). For him, indeed, the economic theories to be "advertized" are "wares" in a "market" in which it is important "to leave economists free to choose" (HSEC, 18) as he says with an allusion to another of Friedman's theses towards which he was much more sympathetic (see HSEC, 6). This clearly explains Coase's somewhat surprising relativism, but paradoxically, there is no doubt that, for him, such a position was nothing but a sheer extension to methodology of his desire to keep in touch with the real world. Coase, indeed, would not have to be pressed very hard to characterize standard methodological literature -- including Friedman's 1953 paper which he considers too "normative" -- as blackboard methodology. To keep closer to the real world, he finds it more appropriate to describe what economists *actually* do when "choosing" a theory, and, in this context, he proposes his own "kuhnian" analysis of the *market* for economic theories.

But the crucial question to raise here is "can theories be treated as goods exchangeable on a market?" The answer to this question depends naturally on what one considers to be a good and what one considers to be a market. Neo-liberal economists like Friedman, Stigler, Becker, Posner, Demsetz and Coase did much to extend the notions of "good" and "market". Roughly, I would say that, for them, a market exists whenever there is some amount of (costly) bargaining over some control of something that can be construed as a good. Alternatively, one might say that a market exists where it is possible to capture gain from trade. Indeed, there is no trade without some implicit bargaining and without costs, and there is no gain without some kind of control over something which is usually called a good. In "The Problem of Social Cost", Coase analyzes as if they were potential markets situations involving "harmful effects" on other parties (like cases of pollution), and explains the fact that these markets are not actualized by referring to the prohibitive transaction costs that would have been required to realized the transactions involved. Whatever the objections that can be (and have been) raised against this type of analysis, the idea of treating such a situation as a market is meaningful because both parties could, in principle, capture gains from trade or, alternatively, because it is *at least conceivable* that costly bargainings might take place over (beneficial) control of a (pollutable) good like air, water or cultivable land.

The case of *ideas*, however, is much more problematic. Should ideas be construed as goods? Certainly, the notion of a "market for ideas" is a legitimate and interesting metaphor, but it is only a metaphor, since in such a potential "market", there is no such thing as an economically meaningful exchange, an economically meaningful bargain or an economically meaningful trade. In a metaphorical sense, ideas are exchanged indeed, but they are not traded one against another like goods are traded in a market. The process of persuasion can surely be compared to a costly process of bargaining, since some kind of dicussion and competition is involved in both cases. However, while this process concerns ideas that, like any good, have utility, it does not concern the *control* of this good in any economically meaningful sense. Proprietorship over an idea is quite meaningful in the context of patents and copyrights, but academic *persuasion* construed as a bargaining process has nothing to do with such proprietorship. The grasp of an idea can be assimilated to a type of control resulting from successful persuasion, but the control gained by such persuasion is not exerted by the persuader who already grasped the idea before starting to persuade. It is true that the persuader-bargainer can gain, if successful, some extra amount of prestige or, at least, of self-esteem, but, in such a construal, the good traded is no longer the idea, but the prestige.

Put otherwise, if you persuade me that your view is correct, both of us have captured some gain (I know something new and you have the satisfaction of having convinced somebody), but this gain was not captured from trade in any meaningful sense. It is true that there was between us a kind of trade or exchange in the sense that we have exchanged ideas or arguments, but the above gains were not captured *from* such a trade. If your view was very convincing, I might have been convinced by it without providing you any alternative idea or counterargument; in such a case, I would have been seduced by your argument (and convinced that I had gained very much by this new knowledge) and you would probably receive still more satisfaction from the situation. Clearly the respective gains do not come from trade but from the transmission of the idea. It is meaningful to refer to gains from trade when gains result from a free exchange in which both parties are ready to proceed because they get more than they lose by proceeding, but in the present case, it would not be meaningful to say "if you agree to give me your idea, I will agree to abandon in your favor some amount of persuader's satisfaction" since it is the very fact that I accept your ideas that provides you with some (if any) amount of satisfaction. If such a construal is correct, you should rather be interested to "give" me all of your ideas in the hope that I shall accept them, but such a description can hardly characterize a market, unless we (absurdly) interpret ideas as negative goods!

However, this discussion, which aims to show that an exchange of ideas cannot fruitfully be assimilated to a market situation, could look exceedingly byzantine if one maintained that Coase's view were simply that valuable ideas — like valuable goods — were selected by market processes. Only the best ideas would resist a free competition, which would tend to discard the less valuable ones. In such a construal, a market would no longer be defined by bargaining, exchange or trade but, more macroeconomically, by competition and selection. But such a interpretation would be still more problematic since it would confuse the epistemological truth of an idea with the sociological acceptance of an ideology. Just as a market does not necessarily select the most valuable goods but only the most satisfactory given the tastes of the agents, so a market for ideas would select the most *advantageous* ideas, given the tastes and the interests of the people involved.

While it is doubtful that Coase was championing this type of epistemological utilitarism when he refered to the market for ideas, these considerations permit us to touch on both the strength and the weakness of Coase's methodological analysis. Indeed, the notion of a market for ideas can be fruitful insofar as it is interpreted as a market for any advantage (like domination or prestige) attached either to the discovery or to the fruitful exposition of an idea, but the analysis of such a "real world" phenomenon is typically *sociological* and in no way *epistemological.* In his paper, Coase raised a standard methodological (or epistemological) question and turned in the middle of the paper to a sociological one, ingeniously attacked with the economist's tools that he has dramatically contributed to extend. Such a shifting of gears is surely not illegitimate in a paper each of whose parts contributes to an elaboration of the ideas of a great economist on the respective topics covered in each part. However, those who are interested in Coase's views on methodology are left with the job of reconciling the two basic methodological intuitions that dictate his rejection of Friedman's positivistic approach, the first being an instinctive realism and the second an ultra-kuhnian relativism (or a methodological liberalism). These two intuitions are not necessarily incompatible but, unfortunately, their methodological compatibility will never be established, as Coase apparently expected, by a sociological analysis of a market for ideas.

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