

ECONOMIC RHETORIC AND THE EXPLANATION OF SUCCESS

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1.- Introductory remarks: economic rhetoric in context

In one of the best critical analyses of economic rhetoric so far, Thomas Boylan and Paschal O'Gorman argue forcefully for the usefulness of distinguishing between what they call "local rhetoric", which they embrace, and "global rhetoric", which they rebuff (Boylan and O'Gorman, 1995, p.44. Also Cf. chap. 2: "Rhetoric/The abandonment of methodology?", pp. 36-60. The expression, "economic rhetoric", is theirs, p.38). As they put it, "[G]lobal economic rhetoric asserts that any philosophy of science which accommodates any method other than the standards of literature has no relevance to the philosophy of economics" (p.44). A main point here is that

"local, like global, economic rhetoricians fully acknowledge numerous limitations in Enlightenment empiricism and its novel articulation in twentieth-century logical positivism. None the less they do not accept the subsumption of economics under the umbrella of rhetoric understood as a literary means of examining conversations". (p. 45)

In short, "local economic rhetoric has no difficulty in acknowledging a limited role for rhetoric" (*ibid.*). But to come to grips with the essentials of global rhetoric in order ultimately to confront the radical rhetoric approach to economics, it is not enough to argue, as Boylan and O'Gorman do, for the indispensability of articulating a sound philosophical methodology of economics . Or so I shall try to show in this essay.

To start off, I should briefly explain why I propose to go beyond Boylan and O'Gorman's account of economic rhetoric, which is critical and much to the point. This is because I believe they fail to account for the status of economic rhetoric as a *research program*. Indeed, economic rhetoric ought to be seen as much a more ambitious doctrine than what Boylan and O'Gorman seem prepared to recognize. Whereas they write that one could, and should, be satisfied with building a "plausible framework to account for the actual methodological practice of economists" (p.36), they nevertheless also notice that, as a device or "model of self-understanding", (McCloskey's phrase: Cf. McCloskey , 1985: xix), the literary criticism of economic works (i.e., economic rhetoric) "will enable us to attain a clear understanding of the *success of economics* while simultaneously helping the field to mature" (Boylan and O'Gorman, 1995: 37; italics added).

It is therefore not quite accurate to state that the understanding of economics "as practiced by the professionals in the field [would remain] largely unchanged" (p.38) if economic rhetoric were to succeed in establishing its case for a complete and systematic replacement of philosophical methodology with literary accounts. Economic rhetoric wants to give economists better means toward the attainment of their ends: it proposes to create an inventory of the tools successful

economists have been using. These are the tools which can precisely account for economists' success and which should thus be taken as essential for junior and even senior economists to master if they hope to be successful in their professional work — and also, of course, if they aspire to fame and recognition.

Were it the case that economic rhetoric did not seek to gain broader influence amongst economists, and if it did not claim to be an improvement on the way economics has been and is still being done — if, in fact, it had no bearing whatsoever on methodological matters — then I, for one, would not bother to criticize it. I would simply consciously ignore its particular assumptions and remain totally indifferent to its general results. But to usurp the proper place of contemporary philosophical methodology as such is precisely what economic rhetoric is aiming at in the short, medium and long run, as McCloskey openly states (see McCloskey, 1985 and 1994). Economic rhetoric puts forward crucial claims and pretends to be able to explain why economics works so well, or at least so much better than philosophers of science are ready to concede, as a scientific enterprise. For that reason, philosophers of science have to be much more cautious in assessing it.

Now, Boylan and O'Gorman make a significant breakthrough in their co-authored book, where they criticize economic rhetoric using very sound arguments. Explicitly opposing McCloskey on crucial epistemological and methodological matters, Boylan and O'Gorman present as legitimate, for instance, the concerns about the scientific nature of economics (the question of finding a demarcation criterion). More generally, they take a stand on the fundamental need to distinguish between the context of discovery and the context of justification. Philosophy of economics, seen

as logical analysis applied to a specific domain of empirical and theoretical inquiry, is here fully reinstated in its own right as an evaluative and normative activity—and as useful to scientists first of all.

But unfortunately, the crucial claim of economic rhetoric, i.e. that rhetoric, using the tools of literary criticism, is in a better position than any normative methodology to explain the *success* of economics, lies untouched and uncriticised. This is why we need to go a step further in order to fully assess the core claim of this approach, i.e. the claim that it is a *kind of explanation*. It is this step forward which I propose to make here. Before we can do so, however, we must first take a small step backward in order to place this essay in context.

According to the vast majority of contemporary philosophers of science, the kind of empiricism assumed and justified by logical positivism until the beginning of the 1960s is a dead horse. Instead of looking for valid universal rules of method for all kinds of scientific research whatsoever, philosophers have been appraising with much more empirical precision the sociological and historical dimensions of science and are now all convinced, or so can it be maintained, that observation is theory-laden;¹ that the history of science shows no linear progress; and indeed, that scientific facts are both socially constructed and most of the time unintentionally discovered. In addition, they all seem to accept that theoretical explanations are

¹ In their book of 1995, Thomas Boylan and Paschal O'Gorman rightly assert that the theory-ladenness of empirical description is "[T]he truism of contemporary philosophy of science" (Boylan & O'Gorman, 1995: 3), and for them this epistemological thesis holds not only for natural science but for the explanation models of observable economic phenomena as well.

never known to be true, even when they are more or less robustly justified by observation and experiment or when they are acknowledged and agreed upon by professional scientists in the field.

Perhaps the crucial turning point away from what has been called "the standard view" or "the received view" was Thomas Kuhn's insistence that "objective factors" like consistency, simplicity and fruitfulness could not explain *by themselves* how theoretical choices were made inside scientific communities. Sociologists of scientific knowledge, more or less espousing the so-called "strong program", amply documented the idea that such theoretical choices often resulted from very polemical controversies, as in the case of Pasteur and spontaneous generation, and could produce far reaching social and ideological consequences, as in the case of Darwinism and the dynamics of evolutionary speciation by natural selection.

For better or worse, these sociologists of science saw themselves as articulating more forcefully, if not more dramatically, Kuhn's scientific and philosophical advance. Indeed, much more an *historian* than a *philosopher* of science, Kuhn was the first to stress that we should think of paradigm changes in the hard sciences not mainly in terms of frequent small and/or large scale revolutions but rather primarily in terms of the "conversion experiences" of scientists (Kuhn, 1962: 151). Examining the approach that replaced logical positivism (or "modernism" to use McCloskey's word), and trying to explain why the post-Kuhnian period in philosophy of science came about, Philip Kitcher says that "[instead] of an ordered abode of reason, science came to figure as the smoke-filled back rooms of political brokering" (Kitcher, 1993: 7).²

² But Philip Kitcher correctly argues that "[T]his was not the way in which Kuhn intended to go,

As a kind of research program, economic rhetoric can be seen as fully consistent with this historiographical-sociological turn. For instance, Boylan and O'Gorman take for granted that "economic rhetoric is a challenging and sophisticated philosophical thesis" which "maintains that economic models are really nonornamental metaphors" (Boylan & O'Gorman, 1995: 7). But in so far as the rhetorical approach to economics is seeking a sound (i.e. empirically true and logically valid) explanation of how economic theories form and/or transform themselves, one must check whether it is based on good arguments or flawed ones. I will argue below that economic rhetoric fails to explain the *success* of economics. Had I more space, I would also challenge McCloskey's view that only "insiders" can and should set the scientific standards in economics or in any other discipline (it seems to also be Kuhn's view for all disciplinary domains of natural science in general).

Rhetoric can be characterized as the analysis of discourse, in contrast with the empirical and theoretical study of language, which is the goal and task of general linguistics. Every book, chapter and article in most fields of intellectual inquiry, including the present paper, are undeniably pieces of rhetoric. They are, after all, written entities, and I have no objection to the effort to describe exactly how economists' arguments are written. Along with Boylan and O'Gorman, I recognize that economists and other scientists use metaphors, metonymies,

and, to the disappointment of some who have drawn inspiration from him, he has continued to insist on a set of commitments that scientists share, that cannot be articulated as rules but that function in distinguishing good reasoning from bad" (Kitcher, 1993: 7).

synecdoches and irony to express what they think more eloquently and, hopefully, more persuasively. Indeed, I have no complaint if the rhetorician wants merely to trap oxymorons and detect analogies in "economic conversation". But surely this is not all there is to economic rhetoric.

In fact, economic rhetoric involves a kind of reduction of knowledge to conversation. But just as we ought to reject the reductionisms of psychologism, sociologism and logicism, we must also reject what I am tempted to call 'rhetoricism'. This is not to reject the discipline of rhetoric itself, defined as the study of the art of eloquence and persuasion, but rather a peculiar doctrine—or worse, a doctrinaire point of view. For a paradigmatic example of traditional rhetorical analysis, one has only to read the works of Chaïm Perelman and Lucie Olbrechts-Tyteka, who together have opened up a new domain of research in philosophy, and whose works, incidentally, are also very highly praised by McCloskey himself.³ Rhetoricism, on the other hand, is much more than a merely descriptive analysis of the figures of style and literary devices occurring in "successful economic conversation". By analogy, we might say that economic rhetoric in McCloskey's sense is to traditional rhetoric as the works of the Edinburgh School in the sociology of science are to the Mertonian tradition.⁴

³ The most direct way of finding out the relevance of rhetoric for philosophy is to read Perelman's and Olbrechts-Tyteca's 1952 book (see Perelman & Olbrecht-Tyteca, 1952).

⁴ As for the strong program in sociology of knowledge, McCloskey, 1985 only refers to the works of Harry Collins, Trevor Pinch, Bruno Latour and Steve Wolgar. But in the 1994 book, the theoretical network of the sociology of science has been more extensively explored.

Rhetoricism is the doctrine that all scientific knowledge in economics amounts to mere literary know-how. It holds that the soundness and validity of economic argumentation reduces in fact to the literary success of economists' speech and the persuasiveness of their style — an idea that it is very difficult to accept, to say the least. And this, as far as I am concerned, is what is really at stake in economic rhetoric as a general doctrinal view.

Rhetoricism has thus to be seen as a broad theory of scientific knowledge. It is a sociological doctrine about what we should be ready to consider as the proper working of scientific communities—rhetorical networking. It is also a philosophical doctrine about what counts as knowledge in science: a doctrine about how belief comes to be collectively fixed in disciplinary communities; about the way in which scientific belief comes to be challenged and eventually reversed and transformed by the makers and users of this knowledge. Moreover, rhetoricism should be looked upon as a philosophical point of view because it conveys a conception of truth (McCloskey considers economic rhetoric as being interested merely in "truth with a small t", as opposed to philosophical methodology, who strives only for Absolute Truth) and seeks to take the place of the normative methodology worked out by past philosophers of science and economists, a methodology the legitimacy of which rhetoricism vehemently contests.

Clearly, we must decide in the end whether every interesting form of economic rhetoric must necessarily incorporate rhetoricism. I will leave this question open here. But what is certain is that economic rhetoric presents itself as a new methodology wherever scientific standards are concerned, for it wishes to systematically articulate the discursive operations economists tend to

use to prove their point in professional discussion. This is to say that economic rhetoric as such is part of a much broader enterprise, an enterprise intended to lead to a new understanding (or as McCloskey says, "self-understanding") of diverse scientific or disciplinary practices.

Come what may here, one has to confess that the rhetorical approach to economics has the immense merit of forcing us to raise some disquieting questions: What is and what should be the place of persuasion in our epistemic enterprise? Does scientific knowledge, and knowledge in general, amount to persuasiveness and nothing else? Should we strive for persuasion at all costs? Is it then possible that one might have to trade off persuasiveness against probity or trustworthiness? And what about warranted assertability, logical validity, factual truth? We can easily understand that every economist should do her best to convince others—this is trivially true. But is persuasiveness a criterion of truth? This, of course, is not a trivial question. It seems to me that economic rhetoric will only be satisfied when philosophers are ready to concede that the only access humans have to truth is through the rhetorical criterion, and that the only guide to truth we have is persuasion. We may take this to be what economic rhetoric is crucially about as a methodological research program.

3. The rhetorical explanation of success

Taking McCloskey's work as the paradigmatic exemplar of economic rhetoric, I shall now try to show that the rhetorician's arguments are not conclusive when taken at face value. What, exactly, does McCloskey want to explain? He wants to explain the "success" of economics as a science.⁵

⁵ McCloskey 's 1994 book starts with the following words: "Economics in its modern and

But who says that economics is, in fact, a successful kind of scientific knowledge? Certainly, many economists do, and McCloskey undoubtedly thinks that this is an already settled question. The only question that is not settled is how economists have managed to succeed. According to McCloskey, philosophical methodology cannot explain this tangible success, and economic rhetoric can. This, then, is what should be the subject of quarrels and wrangling by philosophers and rhetoricians.

Incidentally, explaining scientific success is not a marginal topic in the contemporary philosophy of science: on the contrary, it is a very central one. As Laudan has shown, this is precisely the source of the fundamental opposition of realists and non-realists of all brands (instrumentalists, constructive empiricists, etc.). One of the crucial questions raised here is whether the core theoretical terms of an empirically successful theory need to refer to something physically "real". Following Laudan (see Laudan, 1981), the theoretical terms of an "empirically successful" theory in physics or chemistry do not need to refer to "reality". Laudan gives numerous examples of this. For instance, Laudan considers the ethereal theories in the 1830s and 1840s — i.e. scientific theories referring to something like an "electrical fluid, a substance which was generally assumed to accumulate on the surface rather than permeate the interstices of bodies" (Laudan, 1981: 26) — and shows that such theories explained many empirical phenomena (for instance, the attraction of oppositely charged bodies, the behavior of the Leyden jar, the similarities between atmospheric and static electricity, the role of heat in chemical reactions, the conduction and radiation of heat, etc.). Those theories, which of course we now regard as flawed and false for the

mathematical form has grown into a brilliantly successful science. Unquestionably, it has" (McCloskey, 1994: xi).

reason the substance called 'ether' does not exist, were nonetheless thought to be very *convincing* at the time. Even Fresnel's prediction of a bright spot at the center of the shadow of a circular disc ("a surprising prediction which, when tested, proved correct", Laudan, 1981: 27), was based on an optical ether theory. Philosophers and historians of science say that those theories were *considered* true or near the truth at the time, but of course they would never say that these ethereal theories *were true at that time and that now they are false*. If I understand McCloskey properly, economic rhetoricians would say just that, which is odd.

Now, Laudan's interest in these matters is quite different from the problem being discussed in this essay: Laudan wants to show that the realist's claim is incorrect insofar as he himself can demonstrate that "a part of the historical success of science has been success exhibited by theories whose central terms did not refer" (ibid.:27). But by the same token, Laudan shows that those inadequate theories were nevertheless "rhetorically efficient", so to speak. They were adopted, i.e., believed to be approximately true. Moreover, Laudan shows that those theories were "rhetorically efficient" *because* they were for some time epistemically successful, *but not the other way around*. Laudan's historical analysis can therefore also be used to show that when the ethereal theories came to be less 'efficient' on epistemological grounds they faded away and were replaced by more successful ones. This latter group of theories came in turn, unsurprisingly, to be considered by scientists in the field to be more *acceptable* than the former *because* they were more efficient at explaining observable phenomena. For that matter, these theories came, over time, to be more widely *accepted*.

Laudan writes about the "empirical success" of theories in the natural sciences. Philosophers in general are preoccupied with understanding what can and should count as scientific knowledge in any domain of inquiry whatsoever. Rhetoricians also seem to be interested in scientific success. But what exactly is to count as the (epistemic?) success of an economic theory is never explicitly discussed in economic rhetoric. Nevertheless, rhetoricians want to account for what they take to be the success of economics. And a case could be made that even more traditional economists preoccupied by methodology were looking for ways to account for epistemic success of their own scientific strategy of research. Milton Friedman, for instance, took for granted in the 1950s that economic science had met "with dramatic success" the test of predictive fruitfulness (Friedman, 1953: 33). As is well known, his critics never granted that this was so. Think now of the socialist calculation debate or the Keynes/Hayek debate of the 1930s: we usually say that at the time Lange won over Mises and the Austrians, and that the Cambridge Keynesians won over Hayek and LSE. It is not clear what contemporary economists would now hold to be true. But one thing is clear, that socialism has been defeated as an economic theory and liberalism looks to be overcoming state interventionism (or statism) almost every where on Earth. It is surely not acceptable to think that at one time socialism was a true or an adequate economic theory which gradually became false *because* economists came to the view that it was inadequate as an economic model. Surely the epistemic failure of socialism is not reducible to a somewhat dramatic change or disturbing turn in the conversation economists have been conducting over the last fifty years or so. Surely there is more to it than that, something more *empirical*.

But there lies the rub. For what, precisely, is the rhetorician's yardstick or measure of success for economics? The initial temptation is to say that there is none. For the rhetorician, this success is

in a sense a starting point, a situation to cope with, something like a given to be explained. This supposed 'fact' of the success of economics as a scientific enterprise is perhaps the most tangible dividing line between philosophers of economics and professional economists. It is, indeed, for many practicing economists a hard unchallengeable fact. This is so true that McCloskey, for instance, is not ready to discuss the matter with anyone who seems to call it into question, such as Mark Blaug, Daniel Hausman, Uskali Mäki for example, and especially Alexander Rosenberg.⁶

But again, one must ask what signs point to the success of economic science, for this is not a trivial question. And as Boylan and O'Gorman have insisted, the measure of scientific success is multifaceted. There cannot be only one mark of success here. As they say:

The aim of chess is to checkmate one's opponent: this determines what counts as success in playing that game. Similarly, the aim of science is what is to count as success in that enterprise, which may be pursued by individuals or encouraged by government or private sector institutions for any number of motives or ends. (...) Scientific activity, however, is rather complex in comparison with a game like chess. It is more like the activity of maintaining a ship at sea than playing chess in that it entails a multiplicity of aims, i.e. what counts as success is a multiple-criteria, rather than a single criterion, affair. (Boylan and O'Gorman, 1995: 161)

Moreover, we should be clear whether what we are seeking is a measure of *epistemic* success or something quite other, like professional recognition or public esteem, a distinction which cannot be found in the rhetorical approach to economics. The reason the rhetorician thinks she can avoid

⁶ See Blaug, 1987 and 1991. See also Mäki, 1988a, 1988b and 1993. But see especially Rosenberg, 1988a, 1988b and 1992b.

this line of questioning is that she takes for granted that economists who are Nobel Prize winners, or who are recognized by their peers as being successful economists, really *are* successful. But this is sheer presupposition and faulty amalgamation. What is problematic here is that the only thing that seems worthy of explanation for economic rhetoric is how exactly economists manage to be effectively persuasive. And it seems we should be satisfied with that and not bother about anything else. For, as long as epistemic success is what we want to understand, the only thing we need to understand and explain is the bare rhetorical fact that professional economists have convinced each other successfully, and have also convinced outsiders to the field, that at least some of their arguments are sound and empirically adequate. Only philosophers steeped in methodological prejudice would contest this as the hallmark of success for economics as a professional scientific practice.⁷

From the rhetorical perspective, it seems out of order to hold that the fact that a particular economic model gives an *accurate description of the empirical world* can account at least partly for its scientific success. Moreover, this conceptual framework does not even welcome the very idea that one should in general ask what *methodological criteria* provide a basis for saying that a certain economic theory should be considered worthwhile. But of course one could want to ask, for example, whether what John Muth says about rational expectations can hold water. Or one could want to criticize what Robert Solow writes about the production function. In the same vein, one could also want to thoroughly examine the soundness of the counterfactual argument

⁷ See McCloskey, 1989b, p. 5. McCloskey then comes to the final solution: "Methodology and Epistemology spoil conversations; let's get rid of them" (McCloskey, 1994: 184).

with which Robert Fogel proves that the building of railways has only contributed to a small degree (2.5%) to the increase of the American GNP. All three debates, to take only three examples of which McCloskey has done some very interesting rhetorical analysis⁸, have been taken up and could be pushed still further on epistemological and methodological grounds by economists as well as by philosophers. But unfortunately, this is precisely the kind of philosophical work economic rhetoric opposes.

The rhetorical approach takes it for granted that all in all the writings of economists have been very persuasive and influential and then asks which literary devices they have used to successfully, i.e. *convincingly*, articulate their views. What we are supposed to discover in this way is that economics is a kind of rhetorical activity, and that as such it can only be grasped by applying the conceptual tools of literary theory to the "published conversation" economists have together been conducting for quite some time now.

Indeed, the rhetorician's line of reasoning seems to be the following: economists are talking to each other, they are writing and publishing, they are mutually exchanging viewpoints and arguments to persuade themselves that their explanations and models of economic phenomena are the right ones. They may also be trying to find out which models and explanations will be accepted over the long run as worthwhile and reliable. Economists have a "conversation" and in

⁸ See "The Rhetoric of Scientism: How John Muth Persuades" (chap. 6 of McCloskey, 1985, pp. 87-112). See also "The Master Tropes Rule Economics: The Case of Robert Solow" (in McCloskey, 1985, pp. 83-87). And finally see also "The Problem of Audience in Historical Economics/Robert Fogel as Rhetoric", (chap. 7 of McCloskey, 1985, pp. 113-137).

this conversation they pursue these ends using every kind of rhetorical tool they find useful. Anything—anything that is judged by them to be ultimately efficient—is worth trying, and *in this sense*, anything goes: economists are ready to try anything that they think could be conclusive, that is, persuasive. For that matter they don't care at all about what philosophers of science might say concerning the illegitimacy of a particular explanatory scheme (for instance one that is *ad hoc* and, as such, untestable or unfalsifiable). They do not give a damn whether methodologists claim that in a particular economic theory or another, one of line of reasoning is circular or generally invalid by reference to logical syntax. Economic rhetoricians simply do not care whether epistemologists criticize some economic views for being assumed *a priori* to be valid instead of being empirically formulated, or whether, for that matter, they look to be purely analytical, that is, tautological, which could indicate that economics is content to be a kind of "mathematical politics" (Rosenberg 1992a).

For economic rhetoric, the only thing that counts *or should count* for economists is to convince their peers, to be eloquent enough for their audience, and to effectively persuade members of the scientific community of which they are part. If they argue convincingly, then they argue correctly, and they need not bother about anything else. They can set aside any other methodological constraints such as those, for instance, which philosophers of science would impose upon them. That is, they can do so as long as they keep up their good rhetorical work, i.e. as long as their discourse is effective. And they will be right to regard their work as scientifically acceptable as long as it is effectively agreed upon by the only ones to whom it is being offered, that is, the community of professionally trained economists.

Here then is the key claim of economic rhetoric: if we are right to think that for economics (as for all other sciences, be they natural or social), success in knowledge means persuasiveness in conversation, then the causes of success can themselves only be rhetorical. This is why it will be helpful to systematically identify each and every literary device that has been used by successful economists — especially, for instance, by Nobel Prize winners. And, of course, this is exactly what McCloskey has consistently been doing at least in the books published in 1985 and 1990. But what could be wrong with that, if indeed economics should be assimilated with literature?

Something has gone wrong in this argument. For, even if we were to grant that rhetorical success is all there is in science, something I am not ready to concede at all, we would still have to consider that the rhetorician's inference is a *non sequitur*. Why is that? Suppose for a moment that we grant that John Muth is a successful economist (by rhetorical standards I mean: Muth has been persuasive and has been generally recognized and praised by his peers). Suppose also that McCloskey's literary description of Muth's 1961 paper is empirically reliable and exhaustive, so that we can legitimately think that all of the rhetorical devices used by John Muth have been properly registered and correctly accounted for in this discourse analysis. Would that be enough to conclude that this particular set of literary figures is what caused Muth's reputation, and could we indeed consider that the rhetorical components of Muth's paper globally constitute *the causal factor* that explains his alleged scientific or professional success? It would absolutely not do so, for this question is actually an empirical matter, and rhetorical analysis of texts and discourses is not sufficient to *empirically* establish that economists have been effectively persuaded of the soundness of Muth's bold theory *exclusively* or *in particular* in virtue of the tropes he has been using. There is a *non sequitur* here. It is not because we can

figure out how exactly a particular scientific paper has been written by someone that we have grounds to argue that this is why the theory was accepted as sound after all. The conclusion evidently extends much farther than the premises would allow.

But there is a more general point to make here. For what is at stake is whether economic rhetoric constitutes a conceptual framework sufficiently encompassing to fully allow the logical and linguistic analysis we have to perform if we are thoroughly to understand the proper workings of scientific discourse. But in fact it does not, and a quick glimpse at the Searle/Vanderveken general theory of speech acts will suffice to show this.⁹ One must stress not only that "the way in which something is said is clearly distinct from what is said" (Boylan and O'Gorman, 1993: 51) but, further, that the *illocutionary* component of any speech act is quite distinct from and, as it were, absolutely irreducible to its *perlocutionary* dimension. The act of saying something about the surrounding world and the act of moving someone to believe what is said are two distinct dimensions of a declarative or assertive speech act. In general, the "logic of illocutionary acts" has been proposed precisely to take the double nature of propositions into account. This means, in short, that the propositional content of any assertive statement, including a statement in economic science, must meet very specific requirements if it is to work properly, logically speaking. And in particular, it shows that the meaning of any declarative sentence coming from an economist, as far as it expresses a proposition, can only be grasped, assessed and further

⁹ See especially Searle and Vanderveken, 1985. As a research program, however, Speech Act Theory began with Austin, 1962 and was first further developed by Searle, 1969. For a better understanding of how this theory relates to general pragmatics, see Searle, Kiefer and Bierwisch, eds., 1980.

validated in relation to its *truth conditions*. But more generally, it also means that there are specific *success conditions* which must be met by linguistic behaviours, for instance when discussing a particular matter (for example, making assertions, raising questions, or a mixed set of different kinds of speech acts) or when trying to move an audience and ultimately convince it by appealing to people's intelligence. The success conditions of such behaviours are of a different nature than the truth conditions of the propositional content which is transmitted by the speech acts. Furthermore, the first set of conditions is irreducible to the second, and vice versa. But economic rhetoric seems indiscriminately to mix these two dimensions of speech acts.

In the end, social and professional success amongst one's peers is not the same thing as epistemic success. And of course, even if we grant that knowledge is partly articulated by using what Max Black has called "nonornamental metaphors" (Boylan and O'Gorman, 1995: 43), it is nonetheless illegitimate to reduce knowledge, and scientific knowledge in particular, be it in physics or in economics, to a literary machinery of this kind. The logic of inference, be it deductive or statistical, is surely at least as indispensable for elaborating and also probing scientific arguments as the mastering of tropes. Nothing is to be gained and much is to be lost by saying that economics is, after all, the same as poetry, or that it is a specific kind of literary prose and that it is only analyzable if one treats it in the way one would treat science fiction or any other literary genre.

4.- Concluding remarks: economic rhetoric is misguided and leading economists astray

As I have shown, economic rhetoric seems on one hand to imply that ideas work in science—and as such can be said to be "scientific"—if and only if they convince a scientific community or at least the majority of those identified as scientists practicing in the field. On the other hand, what I call "rhetoricism" includes the claim that what is persuasive in science is what is true for us at the moment and for the time being, and takes for granted that there is nothing else to look for. This obviously means that persuasion is the only epistemic goal of science, and that persuasiveness works as the only possible criterion of truth.

Unfortunately, economic rhetoric as articulated especially in McCloskey's post-modernist vision of the philosophical methodology of economics is based on a clearly defective and deceiving argument. This rhetorical approach to economics, as energizing as it may be for economists in search of greater recognition and gratitude, is strongly biased with regard to the philosophical analysis of science.

Of course, economic rhetoric could, as a research program, be launched and pursued without all this negative talk about the traditional philosophy of science. This is true unless the very goal of McCloskey's rhetoric—rhetoricism—is precisely to get rid of all those vexing questions philosophers raise about economics as being only a putative kind of scientific knowledge. It should be obvious that neutralizing philosophical questions will not help anyone find new ideas about how to appraise the scope and limits of what Israel Kirzner has called, some thirty-five years ago, "the economic point of view" (see Kirzner, 1960). On the contrary, it will merely

increase the intellectual malaise of philosophers vis-à-vis a scientific discipline which they are trying very hard, and more than ever in the last twenty years or so, to understand deeply and without complacency.¹⁰

Even though I am straightforwardly accepting, contrary to Alexander Rosenberg for instance, that rhetorical analysis can be somewhat illuminating when the time comes to scrutinize how economists, and for that matter other social scientists, try to argue convincingly, I am not ready to concede that a good argument (i.e. a valid and sound one) is one that, if anything, actually persuades, be that for a short or a very long period of time. I cannot, and we should not, accept the thesis that *an argument is scientifically acceptable if and only if it actually convinces the majority of the thinkers forming a particular disciplinary scientific community.*

The history of ideas, and especially the history of science and philosophy, teaches us over and over again two things running counter to this thesis: 1) we know that an argument can be convincing and still be fallacious and unreliable; and 2) we know that an argument can be true and still not be believed by anyone. We cannot say, for instance, that arguments *believed to be true*, even by all mankind and throughout human history, are *effectively true*. Wittgenstein would say that, in the scientific language game, the syntax of the word "true" works differently than this. Yet it is what rhetoricism teaches and preaches.

¹⁰ In the English speaking world, we can date the very beginning of a specialized philosophical methodology of economics to Alexander Rosenberg's first book (see Rosenberg , 1976). But in the French speaking world, it goes back to Gilles-Gaston Granger's tremendous and masterly book published in 1955 (see Granger, 1955). But for reasons yet to be explained this book never played the seminal role it could and should have been playing.

In so far as economic rhetoric uses persuasiveness as a kind of litmus test of scientific legitimacy in economics and, as it were, as *the* new criterion of demarcation, it is misguided. This is not only because it is illogical to think that an effectively convincing argument is necessarily an acceptable one (to be *accepted* is not the same thing as to be *acceptable*) but also because we now have good reasons to think that we will never be able to formulate any general and universal criterion of demarcation whatsoever which could once and for all serve to identify legitimate science as opposed to pseudo-science. But because economic rhetoric argues that no demarcation criterion is methodologically operational while at the same time seems to be arguing that rhetorical persuasiveness has to be seen as the only kind which is operative and, for that reason, really counts, our only choice in the end is to oppose economic rhetoric as a research program for it clearly leads the practising economists astray .

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