

The Rhetoric of Economics
by
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Abstract

McCloskey's "Rhetoric of Economics" states the importance of rhetoric and analyzes the poverty of rhetoric in mainstream economics. Particularly interesting is the criticism of the "modernist methodology" and of the existence of a prescriptive methodology in itself. Analysis of the rhetoric is according to McCloskey the way to recover a sound scientific research in economics. The debate raised by Deirdre N. McCloskey's book is in part reported at the end of this work.

1. Introduction

Economics is a weird science. It deals with humanity as well as emotions, instincts and brains. It deals with all these things not one by one, but in a system where each human being is more or less free to make choices. This may be the reason why economics is such an interesting issue, and it is certainly the reason why economics is such a difficult issue; as the American physician Murray Gell-Mann once said "think how hard physics would be if electrons could think"¹. How to deal with all that?

Mainstream Economics treats economics as a hard science. By leaving out emotions, instincts and humanity, economics is the study of how the economic system would work if its elements (humanity in general) were perfectly rational and often isolated. The representative agent, normally used in macroeconomics, is a maximizing agent which represents the whole system - this means that the economic system is supposed equivalent to one maximizing agent. Interactions are eliminated from the picture with all the factors which make the economic system so complex. Economists

¹ Murray Gell-Mann, in Scott Page 1999, Computational Models from A to Z, Complexity, vol. 5 (1) pp.35-41

impose assumptions and deduce conclusions following a strict methodology (see McCloskey 1998). The question that arises is if the mainstream methodology is the only one that can be used. Mathematics and statistics have been essential in the development of economics, but now they are almost the only instruments accepted by the academic community. Assumptions must certainly be made to study reality, but it seems that the methodology is so strict that often hypotheses are made to fit the instruments and not to explain reality! It seems that economists want to explain what happens in a world that can be formalized, not in the real world. There are many ways to study economics, and mathematics is a wonderful instrument, but not the only one. Mathematics can definitely not replace arguing/reasoning, and it cannot be an aim in itself; it has to remain a tool, and as all tools it must be used when it is needed. A revolutionary work on methodology in economics is "The Rhetoric of Economics" by D.N. McCloskey, whose ideas can be a trigger for an interesting debate in the academic community.

2. Rhetorics

In contemporary language the word rhetoric has a bad meaning. It generally refers to "empty speech", to politicians or sellers who want to deceive voters or customers using well made discourses with the aim of hiding the truth and convincing people. This is only part of the truth about rhetoric. As McCloskey (1998) put it "Science is an instance of writing with intent, the intent to persuade other scientists, such as economic scientists. The study of such writing with intent was called by the Greeks rhetoric" (McCloskey 1998, p. 4). The art of arguing is fundamental for knowledge; it is the way in which scholars communicate their thoughts so that other scholars can understand and profit from them. In the scientific community communication thus increases knowledge. Rhetoric can be defined as a reasoned way to argue,

which gives clarity of meanings and arguments, and “rhetoric is exploring thought by conversation” (McCloskey 1983, p.483). According to McCloskey, rhetoric is widely used in economics (many examples are listed in “Rhetoric of Economics”), but it is unanalyzed; yet, it is important that scientists use rhetoric in a skilled way because style and contents are not separable. The way researchers present their work is almost as important as its contents. McCluskey gives as an example Muth's “Rational Expectation and the Theory of Price Movements”. This paper is now extremely important to the entire rational expectation theory (which is central in most papers on macroeconomics nowadays). However, “the paper took a long time to be recognized as important because it was badly written” (McCloskey 1998, p.52). Writing a good paper means to convince the community about your arguments and widely spreading the produced knowledge. The important characteristic of communication is that it needs standards, it needs words, or tools, which are understood and accepted by the community: “Even in the most narrowly technical matters economists have shared convictions about what makes an argument strong” (McCloskey 1998, p.100). This means that the scientific community (of economists in this case) has developed standards about what is convincing and what is not. The problem occurs when those standards are “convictions which they have not examined, which they can communicate to graduate students only tacitly, and which contain embarrassment to the official rhetoric” (McCloskey 1998, p. 100). The standards themselves have to pass through the community, as they need to be analyzed and verified. “Most economists believe that once you have reduced a question to numbers you have taken it out of human hands. That's where the rhetoric of quantification goes crazily wrong” (McCloskey 1998, p.100). According to the rhetoric of quantification, widely used in economics, all that is written in numbers is clear and true. The problem is that numbers have meaning only because humans give meaning to them. Four is big or small depending on what we are talking about; four is a number which has a meaning because it is

thought by a brain, and its relative magnitude depends on what that brain is referring to. The question "How large is large?" applies to any quantitative argument, and, as McCluskey puts it, no one can answer it. There is no general rule which can answer the question; it cannot be answered using mathematics. Mathematics without a brain which controls the tool is useless. According to McCloskey, economics, and particularly econometrics, have made a big mistake by not facing its rhetoric of importance. What is convincing in mainstream economics is the statistical significance of a number which is a strong argument for economists' standards: "The abuse of the word significant, in connection with statistical arguments in economics is universal. Statistical significance seems to give a criterion by which to judge whether a hypothesis is true or false. The criterion seems to be independent of any tiresome consideration of how true a hypothesis must be to be true enough. [...] Table of Student's-t cannot properly nourish a science" (McCluskey 1998, p.113). This long citation clarifies the point made by McCluskey. The tool is not only used, it is abused. Statistical significance has an important meaning within statistics and has to be used in economics when needed, but it must be acknowledged that there is a difference between *statistical* significance and *scientific* significance. The latter is not a mathematical output, it is made by concepts and *scientific conversation* between scholars, or, as McCluskey puts it "The point is to have standards of arguments, to go beyond the inconclusive rhetoric provided by pseudo scientific ceremony in most of modern economics of hypothesize, fit, significance-test, publish" (McCluskey, p. 115). Mainstream econometrics (and economics) often confuses *statistical* and *scientific* significance.

3. Modernist methodology

The "mathematization" of economics was a good idea. Mathematical metaphors can avoid confusion, both during the

research and in the communication of the results. Formalization of the problems gives a scheme in which the researcher can argue. A shared way of arguing helps communication, and as indicated above, communication within a scientific community is the reason itself for the existence of such a community. This gain has a cost: "along with their new mathematical way of talking the economists adopted a crusading faith, a set of philosophical doctrines that makes them prone now to fanaticism and intolerance. [...] In the way of crusading faiths the doctrines have hardened into ceremony, and now support many monks, bishops, and cathedrals" (McCluskey 1998, p.140). The faith in question is composed of all the quantifying enthusiasm we can observe today in economic papers. Everything is written in mathematical terms, independently of the argument and the results. McCloskey calls this the "modernist methodology" and defines it as follows: "modernism views science as axiomatic and mathematical, and takes the realm of science to be separate from the realm of form, value, beauty, goodness, and all things unmeasurable" (McCloskey 1998, p.142). Modernist methodology tries to formalize problems in mathematical terms, to make reproducible experiments, to use the experimental implication as the only basis for refusing or accepting a theory and to make predictions (Kelvin's Dictum: "When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind", see McCluskey p.143). Modernist methodology is not at all fitted for economics. Economics cannot be defined as a measurable science and it can be formalized only in some cases; experiments are close to being impossible, and we all know how the predictions in economics work – or don't work. In order to test empirically an economic theory, you have to collect data, carry out a statistical analysis and make numerous collateral assumptions. In economics, falsification is not cogent and predictions are not possible. Moreover, as McCluskey stresses "A modernist methodology consistently applied would stop advances in economics" (McCluskey, p.154). The greatest objection to modernism is that it imposes a rule bound methodology. Faith that becomes blind with time is a

bad advisor. Scholars are not allowed to choose the tool they think is more appropriate for their quest. They must formalize the problem, impose a hypothesis and write down an elegant equation. Then assumptions are made to fit the methodology with no regard to the research question. A representative agent is used because it permits a closed mathematical form - but is it the economist's aim to produce a closed mathematical form or to understand the economic system? No one can claim that a closed mathematical form is in itself a good explanation of the economic system. However, in a standard economic "conversation", a closed mathematical form is a very strong argument. Mathematics is a wonderful instrument, when it is needed. Using a rule-bound methodology the researcher cannot use and justify the best way to answer a given research question. McCloskey does not say that chaos is the best way to proceed in science, but she claims that neither extreme is the best way.

Mathematics, which is nearly the only tool used in mainstream economics for its demonstrability, explicitness and objectivity, is in itself a human science created by human beings, who have sometimes come to the wrong conclusions (Hersh, 1997). The objectivity of economics built on mathematics is exaggerated and overrated (McCloskey 1998). First of all due to the peculiar feature of economics, but also because mathematics itself and the modernist method of pure deduction founded on mathematics is not infallible. In economics, the deduction method applied to everything is simply not possible. It should be stressed that the same deduction method used in mathematics can lead to mistakes. The existence of infallibility is an illusion. Descartes sustained the ideal Euclidean method: "Start from evident axioms, proceed using infallible deductions" (Hersh, 1997). But in his mathematical research "Geometry", he did not apply this rule (Hersh 1997). In ancient Greece, during the Renaissance and in the following centuries, mathematicians used to acknowledge two different methods: the "synthetic" and the "analytic" one. Euclid used the synthetic method: axioms, deductions and theorems. This

is also the method the mainstream economists pretend to be using today. The analytic method consists of analyzing a problem and finding the solution (Hersh 1997). It is generally called the "heuristic" approach. Descartes insists on the synthetic method but uses the analytic method in "Geometry". He firmly believes in the infallibility of mathematics, but he still makes errors using the method he claims to be perfect (Hersh 1997). In my opinion an infallible methodology does not exist in mathematics and in economics it is totally unthinkable.

Any methodology that is lawmaking and limiting is objectionable; "what distinguishes good from bad in learned discourse, then, is not the adoption of a particular methodology, but the earnest and intelligent attempt to contribute to a conversation" (McCluskey 1998 p.162). Descartes, who was making a revolution in his times, advised researchers to respect no authority whatsoever: neither Aristotle nor religion (Hersh 1997). Conversation, in this context, can be thought as smart arguing, as the attempt by highly qualified economists to convince other highly qualified economists about theories, models or facts. It is in the everyday experience, that talking helps thinking, and *sometimes* drawing a graph or formalizing a problem helps thinking, too - and this is still part of an "earnest and intelligent attempt to contribute to conversation". What is needed is to understand and to communicate understandings to the rest of the world: "the overlapping conversations provide the standards. [...] There is no need for philosophical lawmaking or methodological regulation to keep the economy of intellect running just fine" (McCluskey 1998, p.163) and the crucial point is "our ability to engage in continuous conversation, testing one another, discovering our hidden presuppositions, changing our minds because we have listened to the voices of our fellows" (Amelie Oksenberg Rorty cited in McCluskey 1998, p.163). According to McCluskey, rhetoric is the way to get out of the modernist maze.

McCluskey invites rhetoric not to abandon mathematics in favor of a "flowery language": "The good rhetorician loves care,

precision, explicitness, and economy in argument as much as the next person. [...] It is not an invitation to irrationality in argument. Quite the contrary. It is an invitation to leave the irrationality of an artificially narrowed range of argument and to move to the rationality of arguing like human beings" (McCluskey 1998, p.168). For McCloskey "Methodology claims prescience in scientific affairs. The difficulty with prescience is that it is exactly 'prescience', that is, knowing things before they are known, a contradiction. Methodology entails this contradiction. It pretends to know how to achieve knowledge before the knowledge to be achieved is in place" (McCloskey 1998, p.186). In this sense rhetoric is antimethodology: "rhetoric is not a new methodology, it is antimethodology. It points out what we actually do, what seems to persuade us, and why" (McCluskey 1998, p.184). To conclude, the study of rhetoric in economics is important because "economists should become more self-conscious about their rhetoric, because they will then better know why they agree or disagree, and will find it less easy to dismiss contrary arguments on merely methodological grounds. [...] Nothing is gained from clinging to the Scientific Method, or to any methodology except honesty, clarity, and tolerance. Nothing is gained because the methodology does not describe the science it was once thought to describe, such as physics or mathematics; and because physics and mathematics are not good models for economics anyway" (McCloskey 1983, p. 482). A theory has to be reasonable, persuading and interesting, but this does not imply mathematical formalization.

The debate

The most important consequence of McCloskey's book is that it has started a "conversation" among economists. Economists for and against McCloskey's analysis have written papers explaining the reasons for their position. That is exactly what is healthy in a scientific community: arguing. I have selected three papers: Butos (1987), Caldwell and Coats (1984), Hoppe (1989).

Butos (1987) agrees with most of the ideas proposed by McCloskey. Yet, he has concerns about McCloskey's advocacy of "anything goes" (methodologically) and what he considers a trivializing of the nature and significance of rhetoric.

McCluskey's antimethodological approach has been accused of irrationalism both by Popperians and by economic methodologists (Butos 1987). The accusation of irrationalism was rejected by McCluskey herself in the second edition of her book (the one analyzed in this work) by arguing that what is actually irrational is to use an artificially narrowed range of arguments, to stay within a strict methodology just because it is the mainstream methodology (see p.9). Butos (1987) reinforces the argument saying: "Popperians [, who] claim that scientific activity must be constrained by objective methodological rules if the growth of knowledge is to occur, provides no guidance for rational rule selection and no criteria for rule assessment [...]." (Butos 1987, p. 297). The quest to find a solid and certain knowledge base has failed. The pure deductionist approach to knowledge needs a base of certainty on which to build the chain of deductions. This solid base has not been found in mathematics (Hersh 1997), and in knowledge it is certainty still a chimera. According to Butos (1987), the problem is not that antimethodology implies irrationality; the problem is that methodology is "necessary". In my opinion, this is the most interesting part of Butos' analysis. Methodology is seen as an emergent property of the scientific community: "I have tried to show that science, like other kinds of social phenomena, may be understood as an (evolving) spontaneous order. While innovation and novelty are essential characteristics of such order, it has also been suggested that they operate within a context of constraint. In science, constraint is exercised by abstract rules, indeed, traditions, that inhere within the scientific community. Such traditions, even if they are tacit, provide a context within which research essentially defines itself; they represent the scientist's inheritance, desired or not, wise or not, from which escape is virtually impossible. This

means that scientific activity takes place within a community to which the researcher, no matter how revolutionary, must reconcile himself. In short: 'anything' will not go." (Butos 1987, p.298). I agree that methodology is an emergent property of the scientific community, but I fail to understand how such an argument can be made against McCloskey's ideas. Following Butos, McCluskey's contribution should be considered as part of the process that produces or evolves the emergent property. Moreover, if we consider the emergent property as evolving, the current methodology is only the contingent form of the property, and there is no reason for which this property cannot evolve towards the rhetorical analysis McCluskey proposes. McCluskey is not proposing a total absence of constraints. Indeed, the constraints are made by what is "reasonable and persuasive"; constraints which are made by (or emerge from) the scientific community: we are persuaded by "what persuades well educated participants and justly influential people in our field" (McCloskey 1983, p. 512). McCluskey wants to go beyond prescriptive constraints. Rhetorical analysis *can* be (not *has to* be) the emergent way of producing knowledge. The complex approach to the evolution of the methodology is, in my opinion, a very useful way to look at the phenomenon, but I think it is wrong to imply from this a given form to the emergent property. The present form of the emergent property is not the necessary form.

According to Butos (1987) the definition of rhetoric used by McCloskey is too weak: "I claim that rhetoric not only is a way of knowing, but also that all knowing (and hence science) is rhetorical" (Butos 1987, p.299). McCloskey's definition confines the domain of rhetoric to form and deprive rhetoric of any epistemic value (Butos 1987). In this case Butos is reinforcing rhetoric as a good way to produce knowledge. To conclude, Butos (1987) praises McCluskey's work as an "addition to an increasingly influential literature critical of positivism in economics" and as a "provocative beginning that beckons others to follow".

The point of view exposed in Caldwell and Coats (1984) can be understood by simply reading the following citation: "McCloskey is right in claiming that modernism is dead. [She] is wrong in maintaining that methodology is dead". Caldwell and Coats (1984) maintains that the collapse of modernism does not imply the death of methodology: rhetorical analysis is just one of the many possible responses. According to Caldwell and Coats (1984) McCluskey's first error is that she neglected to include the post modernist literature about the methodological issue in economics. The second error, and in their opinion the most serious one, is that McCluskey argues that any method is "arrogant and pretentious" while her work "is filled with proposals for improving economists' practice!" - a contradiction in their eyes. However, the most disturbing fact according to Caldwell and Coats (1984) is the absence of a methodology: "How do we tell good rhetoric from bad? [Her] nostrums that well educated participants should know the difference and that the justly influential will show us the way are uncharacteristically conservative [...]. Who are the justly influential? What distinguishes them from the unjustly influential, or unjustly non-influential?" and so on. Caldwell and Coats want rules; but how do we tell good rules from bad rules? Butos (1987) rightly points out that rules emerge from the community exactly as the influential economist emerges from the community. The criticism expressed by Caldwell and Coats (1984) does not seem to be the right way to defend methodologies. The study of the methodological literature is interesting, but the last points are quite weak. Proposals do not mean prescription and a rational foundation of science has still not been found (Hersh 1997). As human products, the presence of rules has the same value *a priori* as the absence of rules. It is the scientific community who decides if a rule is good or bad, in economics as in mathematics. It is inside the human mind that the rules (or absence of rules) have consequences. If rhetorical analysis is worse than a given methodology, it should be shown by using other arguments. Caldwell and Coats do not seem convincing to me.

Hoppe's paper (1989) is a very tough criticism of "Rhetoric of Economics" as it can be deduced from the title: "In defense of Extreme Rationalism". Indeed, a part from a long explanation of how badly McCluskey presents her points, Hoppe's very disagreement is philosophical. He is convinced that "the perennial claims of rationalism remain unchallenged by this most modern, relativist attack: the claim that there exists a common ground on the basis of which objectively true propositions can be formulated; the claim that a rational ethic objectively founded in the nature of man as actors and talkers exists; and finally, the claim, only somewhat indirectly established in the previous argument and still to be substantiated, that one can know certain propositions to be objectively true *a priori*, (that is, independent of contingent experiences) as they can be derived deductively from basic, axiomatic propositions whose truth cannot be denied objectively without running into a practical contradiction, that is, without presupposing in the very act of denial what is supposedly denied (so that it would be literally impossible to undo the truth of these propositions)." (Hoppe 1989, p. 7). Unfortunately Hoppe does not reveal the source of the "objectively true propositions", nor the propositions themselves. I would like to answer with a citation from Butos (1987): "Unless one has solved the problem of induction (that is, succeeded in justifying an ultimate rational authority) intellectual honesty requires one to accept the principle that no rational basis exists for belief. As a result there is [...] paradoxically a 'rational' basis for irrationalism" (Butos 1987, p.296). I fail to understand what "objectively true" is in this context, and Hoppe is in my opinion showing a solid faith rather than a solid rationalism. Indeed, as Butos (1987) points out in the previous citation, the very rational thing to do is to accept our inability to see the Truth, with a capital T, or as McCloskey puts it "what is in the mind of God". The ideal objective of a scholar is Truth (if it exists!), but the practical objective is to pursue "varieties of rightness other than truth"

(Nelson Goodman in McCluskey 1998, p. 180). For McCloskey, if a theory “is persuasive, interesting, useful, reasonable, appealing, acceptable, we do not also need to know that it is True” (McCluskey 1998, p. 180). The sense of this citation is not that Truth doesn't exist, but that Truth (if it exists) is not reachable. In economic terms, we have to aim at a second best, since optimality is too far away. This citation is very disturbing for Hoppe: “If there is nothing like truth based on common, objective ground, then all of the preceding talk can surely not claim to say anything true.[..] One cannot argue that one cannot argue” (Hoppe 1989, p. 3). In my opinion one cannot argue that something is True, but one can argue that something is persuasive, interesting, useful, reasonable, appealing, acceptable.

Conclusions

“The Rhetoric of Economics” is a very interesting book. It proposes revolutionary ideas against a methodology that seems to have reached its limits and during the 15 years elapsed between the first and the second edition, the anti-modernist party has grown. New concepts are treated in the economic literature, even if this is happening very slowly. To cite one, the complexity approach to economics is gaining more and more ground. I definitely agree with McCloskey that a deeper analysis of the rhetoric in economics is required, and that the modernist approach has reached its limits. It is important to understand that not all problems can be analyzed using mathematics and statistics, and that these instruments must be used with care when they are appropriate. In my opinion the problem is rooted in the religious faith in the methodology. Faith in science does not make sense. As McCluskey puts it: “Modernism is influential in economics, but not because its premises have been examined carefully and found good. It is a revealed, not a reasoned, religion.”

The view of the methodology as an emergent property, as

proposed by Butos (1987) is very interesting. In this context rhetoric presents interesting aspects. For sure it is not a prescriptive methodology, but, as mentioned also above, the presence of standards related to what is reasonable, persuasive, interesting, useful and acceptable means that in any given period, there are constraints in the scientific community. In my opinion, McCloskey's idea of scientific development is not without constraint, but it contains evolving constraint. When a scholar decides on an issue for his research, and even more important, when a scholar wants to persuade the community that his theory is correct, he/she will use what is in that period considered to be strong arguments. In view of this, I think that the present methodology is merely an expression of the present standards of what is a strong argument, and that the deviations from the mainstream economics that we observe today are simply the evolution of the argumentative standards. Starting from the idea that the modernist methodology is "necessary", as it has emerged from the scientific community, "The Rhetoric of economics" is part of the evolution towards new standards or new definitions of what is reasonable and persuasive. However, "The Rhetoric of Economics" is even more important in that it suggests that the scientific community should be aware of being part of an evolutionary system. Using a deep rhetorical analysis it is probably possible to evolve faster and understand the development. When scholars believe that they have found the right - and not only an interesting - method, experience has shown that it is difficult to move on and to accept criticism related to the method itself. A correct rhetorical analysis does not imply a prescriptive law, but the law of persuasion and reasonable arguments. By knowing how it works, it will work better. We may never actually find the Truth, but we need to know that we are sincerely trying to move towards the Truth, and this movement is impossible if we believe that we have already arrived.

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