CONVERSATION, REALISM, AND INFERENCE

REVISITING THE RHETORIC VS. REALISM DISPUTE

Jesús Zamora-Bonilla

UNED, Madrid

e-mail: jpzb@fsof.uned.es

> But wait. Before you go, look here over at the blackboard. I'v got a sweet diagram of an Edgeworth box that shows the mutual benefit from intellectual exchange. Now suppose to start with we make the assumption that both parties are self-interested…
>
> Deirdre McCloskey, *Knowledge and Persuasion in Economics*, p. 363.

1. THE RHETORIC-REALISM DEBATE: ARE WE ALL RHETORICAL REALISTS AFTER ALL?

For someone who entered the field of philosophy of economics at the beginning of the nineties, the dispute about (or the quarrel between) 'rhetoric' and 'realism', whose main protagonists were being Donald (later Deirdre) McCloskey and Uskali Mäki,[1] was certainly one of the hottest topics. Being myself by that time profoundly immersed, on the one hand, in something like the Finnish approach to scientific

[1] McCloskey (1985) and (1995), Mäki (1988), (1995) and (2000).

rationality, and most particularly contributing to the exciting (though now dismally languishing) ‘verisimilitude programme’, but on the other hand struggling for giving a significant role to the subjective views and biases of flesh-and-bone scientists in the construction of the concept of verisimilitude, I found myself, like (I guess) many people in the really interesting intellectual disputations, with a painfully divided heart on this issue. To a high extent, I think this has also been the fundamental attitude of the two main participants in the debate: both McCloskey and Mäki have being constantly trying to make sense of the arguments and positions they were criticising, and, though this unavoidably led many times to what from the other field had to be seen as a misunderstanding, it had also the consequence of approaching the rival positions by means of each party ‘metabolizing’ some of the rival’s ideas. After all, this is one of the ways in which sound intellectual practices flourish and grow. In a nutshell, at the beginning of the debate mounted by the publication of McCloskey’s *The Rhetoric of Economics* (1985) there were a lot of noise due to the fact that, at least since the times of Plato and Aristotle, the term ‘rhetoric’ had been used in two different senses, one positive and one negative. The positive, or at least neutral sense (exemplified by Aristotle) is that according to which rhetoric amounts to *everything* that can be done in order to *persuade* an audience; in this sense, rhetoric *includes* logic, as well as anything else we could call the ‘canons of rationality’, but is obviously not limited to this. The negative sense (exemplified by Plato) explicitly took the term ‘rhetoric’ to refer to those means of persuasion *that are not* logically (or ‘rationally’) valid. No need to insist in the fact that this negative sense is the one that has become more common in ordinary usage, and hence, McCloskey’s and other defenders of ‘economic’ or ‘scientific rhetoric’ tended to be interpreted (and not to a lesser extent because of the delectation these authors showed in debunking such ‘myths’ as –upper case– ‘Truth’, ‘Objectivity’, ‘Realism’, or ‘Rationality’) as telling that economic, or, in general, scientific knowledge is ‘just’ the result of a set of persuasion strategies not related at all with the objective truth of the claims of which economists or scientists were actually persuaded. The Mäki-McCloskey debate served basically to make it clear two points. First, that ‘rhetoric’ was basically (and especially when the use of the ‘bad’ relativistic rhetoric in the pro-rhetoric proclaims was discounted) being understood in the broad, positive sense, i.e., the sense referring to the inescapable fact that economic or scientific practices and discourses aim at persuasion. Second, that persuasion strategies are not only constrained by contingent and interest-driven rules (that would just reproduce the power status of each individual or group participant in the ‘conversation’), but also by some ‘transcendental’ norms exemplified by some kind of Habermasian *Sprachethik*, including the goal of expressing the facts as they are –given the limits of what we can know and express–, and the norm commanding not to consciously deceive others, though these norms are usually implemented in different ways in different contexts. So, the most likely representation of the debate’s outcome is that *now* we all agree (or so I assume) that science is a field of persuasion, and that truth and objectivity are, in some relevant form, mandatory (though not all-determining) constrains of the persuasion strategies that *should* be employed.

A recent exemplification of this conclusion is Uskali Mäki’s paper on the rhetoric in the neuroeconomics debate. There, Mäki illustrates the use of some strategies that he explicitly identifies as ‘rhetorical advantages’ (i.e., legitimate references to standing scientific standards), and other ones that can be considered ‘rhetorical excesses’ (i.e., something more related to “salesmanship promising”), but that nevertheless might be ‘justified’ taking into account the social and cultural contexts of the disciplinary situation, mainly because

> in contemporary science, such social success is a prerequisite for epistemic success. The epistemic potentials of an emerging research programme cannot be actualized (nor estimated) without mobilizing massive academic resources in its support. Such resource mobilization requires overcoming the resistance of prevailing disciplinary conventions
>
> Maki (2010), p. 114.

Regarding the other side of the debate, consider for example the following statement by McCloskey:

> we are all realists *of one kind or another* (…) we are realists of whatever sort because we all want to be able to use the rhetorical turn, 'Such and such is *really* the case, true'. We want to write history, for example, *wie es eigentlich gewessen*
>
> McCloskey (2003), p. 334.

Taking into account this proclaim (I shall discuss in a moment the 'anti-realist' tinge McCloskey immediately adds to it), we could even re-read the title of the paper in which it appeared ("You shouldn't want a realism if you have a rhetoric") as meaning that "If you have a rhetoric, then you *already have* a realism" (or "all the realism you need").

Nevertheless, from my point of view all this 'rhetoric of science' debate[2] has left some very important questions unanswered, for example: Why are some persuasion strategies successful, i.e., why is it that people happen to be actually convinced by them –instead of simply ignore them? What is the connection between the use of certain rhetoric strategies and the actual attainment of other goals (i.e, besides a scientist being successful *at* persuading someone, is she also successful in reaching –*thanks* to this persuasion– those goals for which the persuasion was an instrument, and what are the factors determining this latter type of success)? Why is a rhetoric strategy more successful in certain circumstances than in others (i.e., can we get some understanding, not of something like a universally valid rhetoric, but of the factors that explain that certain particular rhetorics function better or worse in certain contexts)? Most of my own research during the last years has been devoted to illuminate some or other corners of these intriguing questions, through the construction of a family of game-theoretic models depicting some epistemic interactions between scientists in which these were mostly interested in persuading their colleagues of the validity of a scientific claim. I feel, however, that by my giving too much attention to the persuasion or 'rhetorical' side of those interactions (and in spite of my explicit assertion on the contrary), many readers may have come to the conclusion that these models were just another assault against the fortress of scientific rationality and objectivity. I have devoted a recent paper[3] to explain in exactly what sense my game-theoretic approach to scientific persuasion serves as a defense, rather than a criticism, of (lower-case?) objectity and rationality, but I want to take this opportunity to explore and clarify the connection of this approach with the question of scientific realism, another (if not the most important one) of the topics in which Uskali Mäki's work has concentrated.

[2] See, e.g., Gross (1990) and Pera (1994).
[3] Zamora Bonilla (2010).

So, just in order to finish to framing my discussion about realism within de rhetoric debate and its unanswered questions, let me comment a few more of the rhetoricians' statements on the topic. McCloskey, for example, in the same paper just quoted, says "I'm denying that that there is a timeless Good Argument for anything" (p. 331). Well, though not intending to fire from their departments those Platonist logicians or mathematicians that think and defend the universal validity of many logical arguments, I would also accept McCloskey's claim, at least in the sense that, whether there is such a timeless Good Argument is probably *irrelevant* to the working science; but who cares? The problem is not whether this exists or not, but whether we may find an explanation of why argument A is taken as valid in context C, and better if it is a *general* theory or model, serving to explain, from the peculiarities of arbitrary contexts and arguments, why certain arguments are more likely to be taken as valid in certain contexts than other arguments, or than in different contexts. Below in the same page, McCloskey adds "a science is a class of objects and a way of conversing about them, not a way of knowing Truth now and forever", but this is certainly not the whole truth: science is *not only* that, for there are many 'ways of conversing' about the same objects, and most of these 'ways' are not considered by us as 'science'; the question, hence, is what is characteristic of the *kinds* of conversations that we call 'science', not as a universal demarcation criterion, but just as a description of our use of the term? For example, people talking in the bar about the financial crisis are just 'having a conversation about a class of objects', but what they do is (or so many of us think) qualitatively different from what happens within a university seminar on macroeconomics. Professional economists employ a *specific* way of 'conversation' probably because they think that *by doing so*, the conclusions they will reach will have some *properties* that will make them different from, and *more valuable* than, the outcomes of canteen chatter. I do not see any reason to consider that it is an illegitimate question the one that asks what are those *properties* the claims that are the outcome of a scientific conversation may have (or may lack), and that scientists may consider as the *reason* that make some ‘ways of conversing’ *more desirable and appropriate* than others. I also do not see any reason to doubt that these properties may often, if not always, be of some *epistemic* nature. And furthermore, I do not see any reason to consider as an illegitimate question the one that asks whether particular scientists or scientific communities are *wrong* about their reasons to think the way they think about this question, i.e., whether it can be the case that, while they believe that by using such and such criterion to decide if a solution to a given problem is right, they are nevertheless mistaken in the sense that this criterion is actually not efficient in selecting ‘right’ solutions (according to *other* elements of the notion of ‘rightness’ those scientists themselves have, or according to what we may justifiably think that should be taken as ‘right’). After all, McCloskey herself has pretty much conducted precisely this kind of research when criticizing some of the practices of modern economists.[4]

Or consider the qualms expressed by McCloskey to the ‘rhetoric of “how such and such has *really* been”’, quoted a few paragraphs above: : “the *really*”, she says, “refers to our rhetoric. It refers to our persuasions about the world, not directly to the world itself” (*op. cit.*, p. 334). But, if we are interested at all in having a *distinction* between how things ‘really’ are and how things ‘seem’ to us, or between our firmly believing a proposition and the proposition being true, i.e., if we just happen to have the *notion* that we can possibly be *mistaken* in believing something that we believe, then, the *aim* of using ‘the rhetoric of such and such being really so’ is to help us discriminate between those argumentative procedures that lead more often to *mistakes* and those that

[4] See, e.g., McCloskey (1997), and McCloskey and Ziliak (2008).

do it less often. Of course, mistakes can only be recognized through their clashing with other beliefs, but, why not simply accept *any* belief, no matter how conflicting it is with other ones we or other people have? Why scientific communities bother in defining and maintaining argumentative 'sound' strategies, instead of randomly extracting what they must accept from an urn containing all the possible claims about the class of objects they study? A little bit above McCloskey had written "even 'objectivity' has a necessarily social definition"; well, alright, but this does not *only* mean that each community has its own criteria to decide when a claim is acceptable as 'objective', but it also means that some *properties* of scientific outputs (claims, data, theories, models, and the like) are valued by the members of that community as a signal of their being objective, and that scientific research is or may be *socially organized* in such a way that those outputs having those properties are most likely produced than other outputs lacking them. So, realism is always small-case realism, in the sense that *it is the flesh-and-bone scientists who are interested in discovering theories or laws that are 'really true'*, and philosophical elucidation about realism amounts basically to try to *make sense* of what in the hell all this thing means, and how do those damn scientists manage to be so good at it![5]

[5] Before finishing this section, I do not resist the temptation to quote one of the big names in the 'anti-realist' movement, the North-American philosopher Richard Rorty. In a book with interviews to him (Rorty and Mendieta (2005)) we read, for example, that "if we take care of making sure people can say what they belief (…) 'truth will take care of itself'" (p. 15), and that "'truth' is a name for what is most likely to come to be believed under Habermasian ideal conditions of communication" (p. 112). But this should be taken as empirical *predictions*, not as definitional *explications*. Note, e.g., the 'likely' in the last quote: it entails that *sometimes* we end up with *false* claims even under Habermasian ideal conditions; if this is so, is their being 'false' just another way of saying that they are fewer?; but, if the *only* thing we know about those false claims is that they have been reached under such ideal conditions, why conceptually separate them from those conclusions we have reached in similar circumstances *and happen to be true*?; hence, why not say simply that truth is *everything* that is concluded under Habermasian conditions (and not merely 'what is more likely concluded')? So, given that this is not the case, i.e., given that what it is said is that freedom of speech, of research, etc., is simply *more* conductive to truth than to error, or more conductive to truth than other institutional or cultural settings, it might in principle be scientifically investigated whether this is the case or not, or exactly in what conditions it is. For example, in Zamora Bonilla (2006a) I argue by means of a game theoretical anaylisis that *unconstrained* 'freedom of speech' would be detrimental for the attainment of epistemic values in science.

2. REALISM WITHOUT RHETORIC.

I turn now to other aspect of the 'realism in economics' debate in which Uskali Mäki has been engaged in the last decades, one not directly connected with the issue of rhetoric. No doubt, the most famous methodological thesis amongst economists is the one that Milton Friedman popularised more than half a century ago, a thesis according to which it is not important that most of the assumptions of an economic model (perfect competition, complete information, infinite divisibility of goods and money, and so on) are obviously *false* as description of reality: the *only* important thing about economic models is that they provide *successful predictions* (for example, about the evolution of price levels, quantities bought, interest rates, etc.).[6] The fact that those assumptions fail to satisfy the criterion of –to use Friedman's perhaps unfortunate word– 'realism', is just the price to be paid for the mathematical tractability of the models, so that we can actually manipulate these in order to derive specific predictions. In spite of the attacks Friedman's thesis suffered during the first decades after its publication, its success between practicioners of economic science was so astounding that it has become commonplace to use it as a conversation stopper when the 'unrealism' of some aspects of a model is pointed out, particularly within the New Classic school; it seemed, so to say, that anti-realism or instrumentalism had become something as the natural epistemological position of economic science. The situation changed in the nineties, basically through the (radically different) philosophical programmes of Uskali Mäki and Toni Lawson.

One nice place to locate the most relevant aspects of this new realism debate is the criticism launched at those programmes by Daniel Hausman (1998), a paper in which the

[6] See Mäki (2009) for both Friedman's original methodological piece and a set of reflections upon its impact on the philosophy and methodology of economics.

author tried to show the irrelevance of realism as a philosophical thesis for economic science. Hausman's argument is the following: In the first place, the debate about realism in both in classical and contemporary philosophy of science has centered about the question of the objective existence of the *unobservable entities* that some empirically successful theories presupposed (*e.g.*, atoms, electrons, quarks, black holes, or genes); within this debate there were two paradigmatic positions: on the one hand, that of Hilary Putnam, who asserted that it would be a miracle to assume that modern physical, chemical or biological theories might be so empirically successful if the entities they assumed (or something close enough) did not exist at all; on the other hand, the position of Bas van Fraasen, who noted that, since by definition we cannot observe unobservable entities, in accepting a theory we must be satisfied with accepting the claim the theory has empirical success, and just to *ignore* the question about the real existence of its unobservable part. In the second place, Hausman shows that economic theories do not refer, typically at least, to 'unobservable entities'. It is true that some things in economics might be taken as 'unobservable': Cobb-Douglas utility functions, macroeconomic aggregates, or the ontological correlates of the different equilibrium concepts. But all these concepts do not *refer* to entities that are 'unobservable' in principle (or only observable with the help of sophisticated observation devices), but to things that are plainly 'observable' and whose existence is nearly unquestionable, only that the economic theories refer to them through strongly idealized or simplified descriptions. So, Hausman concludes, discussion on 'realism' in economics is not relevant, as long as everyone taking part in a theoretical economic debate accept the real existence of the entities the theories refer to. 'In economics, everybody is a realist', would Hausman's general conclusion be.

What about the specific realist approaches of Lawson and Mäki. Starting with Lawson,[7] his 'critical realism' is based, amongst other interesting ideas, in the claim that reality is not made up just by (empirically detectable) 'facts', but also by underlying structures, powers, mechanisms and tendencies, that only manifest themselves in the form of strict empirical regularities within certain specific 'closed systems', for example, within an experiment, and rarely 'in the wild', because 'open systems' are prevalent in nature and society (a point, by the way, similar to the one made by Nancy Cartwright). The goal of science, in general, and of economics in particular, is, according to Lawson, to discover those 'deep' structures and mechanisms. New Classical Econonmics, according to him, has committed basically two serious mistakes in connection with this: first, its positivistic affiliation, which takes as essential to scientific research the discovery of empirical regularities and to scientific theory its consisting in the mere systematisation of regularities; and second, the exclusive use of what Lawson calls the 'deductivist' method, i.e., the one consisting in the construction of formal models, derived from a set of assumptions (like rationality, maximisation, equilibrium, and so on) that are never put under question. The main criticism Hausman puts forward to Lawson's view is, first, that economists, including the new classical ones, attempt in fact to find and explain the *causal mechanisms* that generate the phenomena we observe in economic experience; second, that they are explicitly conscious of the fact that these mechanisms *do not* produce strict regularities (like in natural sciences), particularly due to the fact that economic systems are very unstable and complex (i.e., they are subjected to many mechanisms simultaneously); but, third, that economists never admit that these mechanisms are 'occult' or 'underlying' in the sense that they might refer to *unobservable* entities, but they are only so in the sense

[7] See, e.g., Lawson (1997).

that their discovery demands a big effort of *abstraction*, in order to conceive the economic system (or an important subsystem of it) 'as a whole'.

Another criticism of Lawson's realism has been presented by Wade Hands, showing that, in fact, new classical economists follow *exactly* the type of scientific method preached by Lawson! (i.e., that of imagining causal mechanisms that lead, as a result, to the economic facts we observe):

> Lawson and the neoclassical economists can have exactly the same view of the type of things (real underlying causes) that one should be looking for in economic science, and yet disagree totally about what those real underlying causes are.
>
> Hands (1999), p. 182.

We might even add, to use Lawson's own terms, that those economists are *just* looking for mechanisms 'facilitating' certain types of actions (e.g., economic policy measures), though those mechanisms have not had till now the opportunity to actually work in the real world. For example, general equilibrium theory does not affirm that the economy *is* a system of perfect competition; rather, it tells that, *if* we removed the factors that actually hinder the economy from being such a system, *then* we would have an economic system that would be efficient in certain specific sense. Information economics provides a further example: the point of this theory is not to describe what facts do really happen in the world (this was nicely asserted by Akerlof in his classic 'Market for lemmons'), but to inform us about what kind of contracts *would have* efficient effects if they *were* established in specific circumstances of asymmetrical information.

Another possible criticism to Lawson's critical realism is that his contrast between the 'transcendental' or 'abductive' method defended by him, and the 'deductive' method that he assumes neoclassical economists are following, is deceptive. The Kantian expression 'transcendental method' refers to the search for the 'necessary conditions of possibility' of something; for example, how the world *must* be so that experimental science is possible, or how the structure of the economic system *must* be so that we can observe what we happen to observe in it. This is *not* equivalent at all to 'abduction', which consists just in *inventing* a hypothesis in order to explain a set of phenomena or facts. Abduction is, so, another denomination of the good-old-fashion *hypothetico-deductive* method, whereas the transcendental method correspond to an *aprioristic* way of research. A little bit of formalism will help to better understand the distinction: in the abductive method, the connection between the theory, T, and the empirical facts, E, that we want to explain by means of the theory, is (if successful) $T \rightarrow E$ ("if the theory were true, these facts should be observed"), whereas in the transcendental method the relation (if successful) is $\neg T \rightarrow \neg E$ ("if the theory were *not* true, then we *could not* observe these facts"), which is logically equivalent to $E \rightarrow T$ ("if the facts are as they are observed, then this theory is necessarily true"). Hence, in the transcendental method you attempt to deduce the theory from the empirical facts, whereas in the abductive method you try to deduce the empirical facts from the theory. Lawson's proclaims in favor of a 'transcendenal' method seem to imply, hence, that he is thinking rather in the hypothetico-deductive method, mainly when he asserts that the results of applying the former are always fallible and revisable (whereas truth is that in the real transcendental method would only deliver truths that follow necessarily from our data, so, the only way of 'revising' a conclusion in the transcendental method is by showing that our deduction has been mistaken, or that the empirical data were incorrect). But, being this so, the conclusion is that *the method Lawson privileges is* *not really different from the method he criticizes* (the 'deductive' method), for this one also consists in deriving conclusions from hypotheses, and testing afterwards those conclusions against the available data. Taking this into account, the important aspect of neoclassical economists that Lawson seems to *want* to criticize is not that they derive conclusions *in a deductive* way from their theories' assumptions, but the fact the these economists *are not willing to reject these assumptions* disregarding what is the result of the comparison of the conclusions with the empirical data; i.e., the problem is not that new classical economists follow a 'deductive' method, but that they are *dogmatic*. The problem is that it is not clear at all that alternative schools in economics or social science (schools that Lawson praises as 'realist'; e.g., Marxism, post-Keynesianism, institutionalism…) are *less dogmatic* than the mainstream, for they seem to be equally 'unwilling' to reject their own theories' fundamental assumptions under the light of empirical evidence.[8]

Uskali Mäki's approach to realism in economics[9] is philosophically stronger and makes more justice to the peculiarities of social science, and of economics in particular. The most essential aspects of his approach are the following. In the first place, Mäki insists in separating *'realism'* (as a *philosophical thesis* about the most appropriate interpretation of scientific knowledge, or as a *scientific value*, i.e., the claim that the goal of science is to discover or to approach the truth about the world and the really existing things) from *'realisticness'* (as a *property* that scientific models, hypotheses or theories may have or may lack, i.e., their describing more or less correctly the truth or the really existing things). In the second place, Mäki distinguishes *'idealisation'* (as a deliberately *exaggerated*, and hence false, representation of an entity or system) from *'isolation'* (as the omission to take

[8] Another debatable thesis of Lawson's theory, though less direcstly connected to the realism issue, is that new classical economics dispossesses individuals from their freedom of choice, for the assumptions of that school would 'force' individuals to act in a determinate way (that one which happens to maximize their expected utility functions). I think this criticism is also not tenable, because *any* social theory must give an *explanation* of what people do, and this amounts to identify an action as the one that will most probably happen given certain circumstances. Renouncing to have an explanation of social facts is really to renounce to have scientific knowledge of human social behavior.

[9] See, e.g., Mäki (1994).

into account some aspects of reality that are not considered relevant or important, though without asserting that these aspects do not actually exist; an 'isolation' assumption, or a model based in one, is, hence, not necessarily false, but simply *partial*). In the third place, Mäki introduces the notion of *'commonsensible'*, as different from 'observable': whereas an entity is observable depending on its relation to our perceptual capabilities, Mäki's neologism refers to things that are not really (or not necessarily) observable in strict sense, but that we ordinarily take as objectively real in our daily social behavior (things such as 'firms', 'preferences', 'government', 'money', and so on). These conceptual distinctions allow Mäki to express the following *realist* claims about economics: first, the goal of economic science is to discover the *essential aspects* of the working of the economic systems (aspects that our theories describe thanks to conceptually isolating them); second, in order to attain this goal, it is often necessary to work with theories that contain *false assumptions* (idealizations), but different types of falsities play different roles, and in general, they serve to illustrate the working of different mechanisms; third, economic theories refer to commonsensibles, and hence, the question about non-observational terms is not relevant; and last, but not least, economists should study the causal processes involved in the working of the real economic processes (*'the way the world works'*), in a similar way as to what Lawson proposes.

Going back to Hausman's critique, we may say that, blinded by his attachment to the unobservability question as the most central one to realism, Hausman ignores that one important idea for realist philosophers is that the goal of science is *truth*, and not only empirical success; in the case of physics and other natural science, this goal is approached thanks to the hypothesis that unobservable entities exist, but these are an instrument in the attainment of the real goal (truth, or approximate truth). Stated otherwise, what counts for the realist is not essentially whether what scientific theories talk about *exist* or not, but whether what they say about the world is *an illuminating and accurate description of how the world really is*. From this point of view, Lawson's and Mäki's criticisms to some economic theories must be taken as a denunciation of the failure of those theories in describing the *real working* of the economy. The instrumentalist methodology derived from the spreading of Friedman's slogan succeeded in the economists' becoming immune to such kind of criticisms for some decades, but now it can be doubted that New Classical economics has had a predictive success so outstanding that either it is a clear signal of the approximate truth of their assumptions about the real economy's working mechanisms, or it is irrelevant whether these assumptions are basically right in the light of a clear predictive success. So, it seems that a sensible recommendation for economic practice from realist approaches is that it is time to take seriously the effort of devising and using more truthlike (in Popper's sense) assumptions for economic theories (for example, assumptions based on observed behavior –in the way of behavioral economics-, or on less idealized statistical hypotheses –in the way of substituting representative agent assumptions by hypotheses about distributions of agents' types-, or on multiple-equilibrium causal processes based on evolutionary or network mechanisms).

A more recent criticism of the realist approach in the philosophy of social sciences, and which is relevant for the view of scientific knowledge I shall outline in the next section, has been launched by Isaac Reed,[10] who, unfortunately, chooses as his main target the transcendental realism inspired by Roy Bhaskar and exemplified by Tony Lawson, and also tends to identify 'good' social science with just the more hermeneutic paradigms. As According to Reed, realism is an inadequate philosophy for the social sciences, mainly because the social formations do not depend so much on universally valid mechanisms, than on contingent constellations of elements, and because meanings (which are always

[10] Reed (2008) and (2010).

subjective) are an essential constituent of social facts. Reed, however, attempts to escape the perils of post-modern relativism by distinguishing ‘ontology at the level of theory’ (“establishing a unified, abstract account of the fundamental mechanisms according to which social life works”, Reed (2008), p. 119), from ‘ontology at the level of explanation’ (“sociological explanations are ontological in the sense of making truth claims about aspects of the social world”, ibid.); the last assertion is later clarified in another paper:

> we need to separate “objectivity” from “naturalism.” Objectivity is a goal for social knowledge—to accurately represent the context of explanation or some piece of it. Naturalism is a means to this goal; it proposes to import into social science the relations between the context of investigation and the context of explanation that are perceived to be productive in natural science (… But) if meaning orients social action, then the only way to explain action is to develop theories that enable the interpretation of this meaning.
>
> Reed (2010), p. 37.

Hence, Reed proposes ‘interpretivism’ as an alternative to ‘realism’ and ‘naturalism’ in the social sciences, an interpretivism that would describe as the main goal of social research the maximum possible ‘overlap’ between the ‘meanings’ that inhabit and constitute the world of the *investigated* subjects, and the ‘meanings’ that constitute the theories of the *investigating* subjects (Reed, 2010, 36).

From my point of view, Reed proposal is plainly acceptable (though partial in his interpretation of social science, as I shall explain immediately), but not as a criticism of realism as a philosophical interpretation of the goals of social science,[11] for, in the first place, realism in the most general sense is, as we have seen, more committed with the truth of theories as a goal of research, than with 'universal causal mechanisms' as an ontological assumption (this is, at most, an ingredient of some brands of realism), and so, the idea of competing social theories partially overlapping their subject matter to a higher or lesser extent is plainly compatible with the notion of science as pursuing progress towards the truth, no matter how unstable or 'subjective' happens to be the subject matter. In the second place, realism is, according to this interpretivist view, not much a philosophical tenet, as it is a constituent of the values and attitudes of scientists themselves (as was explained in the first section). But, in the third place (and hence the partiality mentioned above), the social world is not only identifiable with the subjective understanding that social agents have of their situation, for there is *much more* to discover and understand in social facts than what the individuals pertaining in those facts understand (and not too seldom misunderstand) themselves: the actions of individuals are enchained in interactions and networks whose structure and consequences are often completely concealed to the agents, or these do grossly misrepresent them. These networks of interactions (that we may call 'mechanisms', not certainly 'fundamental', but rather 'emergent' ones) are a legitimate object of study for much of social science; they are of course more or less unstable and path dependent (i.e, strongly affected by contingent historical factors), but this does not preclude that social theories (and especially *models*) about the most general and abstract properties of these mechanisms may be of great help in understanding what is that is happening in the social world (or 'the way the world works', to employ Mäki's fortunate expression). And, as the 'overlap' between social scientific understanding and the social subjects understanding of the social world really goes in the 'reverse' direction (i.e., as

[11] Neither of naturalism broadly understood; cf. Zamora Bonilla (2011), in which it is shown how hermeneutics and naturalism can be made compatible.

social agents translate and appropriate social theories and models, more or less successfully), we shall also take into account the reflexivity and performativity issues that this interaction between both extremes will produce, i.e., how the people's 'knowing' of certain social science theories affects people's behavior and its actual consequences… but this is no point at all against realism, but just a recognition of the peculiarities of social *reality*!

3. REALISM AND INFERENTIALISM.

I shall devote this last section to describe how the kind of realism defended till this point coheres with the inferentialist model of scientific research I have been elaborating elsewhere. According to this model,[12] scientists try to persuade their colleagues (and others) of the acceptability of some claims. The problem is that, since each researcher is trying the same, and there seems to be no reason why it could be rational for one scientist to accept a claim proposed by another, instead of just proposing a new claim by herself. The 'solution' to this apparent paradox I have suggested is that the 'game of persuasion' must be played according to some rules, which in some cases make more or less compulsory for individual scientists to accept some assertions, given what assertions have been accepted by them before, or given what facts have publicly taken place; i.e., the assertions made by individual researchers are subjected to *inferential rules* (rules saying that, if you have accepted such and such, or such and such has occurred, then you must accept some further claims, or you must reject others). Imagine that each scientist is writing on a 'book' the assertion to which

[12] For a more complete version, see Zamora Bonilla (2006b).

she commits (i.e., to which she accepts that the inferential rules of her community are applicable), and so that the knowledge of what is written in each researcher's book is public; hence *the game theoretic nature of scientific research arises because each scientist's payoff depends on what is 'written' not only on her own book, but on the book of each other member of her community* (i.e., your payoff depends on what your colleagues say about what you say). This payoff is generated by three different channels: an 'internal score', an 'external score', and a 'resource allocation mechanism', all of which are determined by norms. In the first place, any scientific community will have adopted a set of *methodological norms* with which to assess the scientific value of any set of commitments (of any possible 'book'); the coherence of a researcher's book with these norms (or, more precisely, the coherence *her colleagues say* it has) will determine the *internal score* associated to that book. Second, and in contrast to the case of everyday language games, in science many propositions are attached to the name of a particular scientist, usually the first who advanced them; one fundamental reward a scientist may receive is associated with the fate that the theses (laws, models, experimental results...) proposed by her have in the books of her colleagues. This 'fame' is what I call here her *external score*. The combination of the internal and the external score associated to a book is its *global score*. Third, the community will work with a set of *rules for the allocation of resources* which will determine how much money, what facilities, what work conditions, what assistants, and so on, will be allotted to each scientist, depending on her global score. So, it is basically the effort to have a high internal score (i.e., of being coherent with the methodological norms of your community) what makes you accept or reject claims advanced by others, hence rising or lowering their external scores.

Realism, as an epistemic value, enters into the picture through the consideration both of the inferential norms that have been adopted by a scientific community, and of the preferences of individual scientists. Rules are not an absolute given, but we can assume that in each community they will have evolved according to the preferences of its members, and the more common a rule is across different communities, the more reasonable is the assumption that it reflects a common preference for scientists. After all, scientists as a group are free to establish the scientific rules they prefer, and, if they are rational, they will tend to establish the rules that (they think) will better promote their most preferred goals. What are these goals, hence? What this approach suggests is to build different models assuming some or other preferences and constraints, and see which models offer a picture more similar to real science. My own proposal has been that the utility function of the toy scientists populating these models may be seen as consisting of two main elements: a ‘social’ component, which is some function of the success of a researcher’s proposed claims in the ‘books’ of others (i.e., ‘recognition’), and an ‘epistemic’ element, which can be modelled as a kind of ‘empirical verisimilitude’ function (i.e., the scientists prefer, *coeteris paribus,* to be recognised by having discovered theories with a higher level of verisimilitude, and prefer to be governed by inferential rules that lead them to accept claims they think are highly verisimilar, than claims with a lower verisimilitude); basically, a theory has a greater ‘empirical verisimilitude’, the more supported it is by the highest possible amount of empirical information, or, if the empirical data are taken to confirm the theory, the more contentful the theory is.[13] Of course, this is just a hypothetical proposal, and it would be interesting to contrast the predictions of this model with those of other models based on

[13] One simple definition of empirical verisimilitude is the similarity between the theory T and the empirical data E (measured as $p(T\&E)/p(TvE)$) weighted by the content of these data ($1/p(E)$), so $Vs(T,E) = p(T/E)/p(TvE)$. See Zamora Bonilla (2002) to confirm how different functions based on this simple one to adapt it to more complex cases derive in ‘realistic’ methodological practice (i.e., practices that would reflect the ones of real scientists).

different assumptions about the preferences and constraints faced by scientists; till the date, however, no alternative models have been proposed.

The commitment to realism must be seen, hence, not so much as a philosophical thesis, but as a peculiarity of the values and (or) the practices of scientists themselves, who will deploy, or fail to do it, something like ‘revealed preference for realism’. In what of behaviours or choice propensities can this preference be manifest, assuming that my hypothesis about the epistemic part of scientists’ utility functions is approximately right? Here are some possible examples:[14]

1) Of two laws *confirmed* by the data (so that their posterior probability is 1 for both), the *stronger* (i.e., that one with lower prior probability) will be taken as preferable. Note that the stronger law can be taken as a more accurate and contentful representation of the facts.

2) Of two hypotheses that *explain* equally well the existing data, the *weaker* will be taken as preferable. This may sound paradoxical given the previous point, but the case is very different: no the statements are not ‘confirmed laws’, but *hypothetical* claims (models or theories); we do not know whether they are true, only that they successfully explain the data. So, in this case, to make a *stronger* claim is unsupported, and takes us further from what we *know* about the truth.

3) When scientists expect that new relevant data arrive, they tend to be more ‘instrumentalist’ (for they will take into consideration the *expected* value of the theories’ verisimilitude, and it can be proved that in this case adding a new empirical discovery that contradicts the theory does not decrease the latter’s expected value), whereas, if new relevant data are not expected, they will be more ‘realist’ (for in this case, the actual verisimilitude of the theories depends in a negative way of their failed

[14] See Zamora Bonilla (1996) and (2002) for the proofs.

predictions). Stated in other terms: when developing and testing for the first times a theory or family of theories, it constitutes no worry for researchers the acknowledgement that these theories are empirically false: only 'successes' count. Instead, when the field is 'mature', having 'less' falsity is a virtue. I have called this 'the Lakatos principle'.[15] This point shows that a 'revealed preference for realism' is not just a truism, but that there are behaviours that would reflect a different epistemic preference; in this case, realism is defended as an essential component of the scientists' epistemic utility functions because their 'instrumentalist' behaviour is shown to be a short term consequence of the *type of context* in which more realistic theories are looked for.

4) The verisimilitude of a theory may also be judged by researchers not directly on the basis of the 'literal claim' it makes, but of an appropriate *qualification* of it, i.e., one that takes into account that the theory is not trying to literally describe the world, but of giving an approximate, idealised, or otherwise 'suitably deformed' description, one that allows to consider what are taken to be the relevant aspects of the world, and also to make calculations and inferences easier. This gives place to much of what Uskali Mäki has been working on for two decades regarding the 'irrealism' problem.[16]

5) There are only three ways of increasing the value of a theory that explains well the data: first, discover new independent data that corroborate the theory (new empirical successes); second, increase the prior probability of the theory; and third, decrease the prior probability of the already explained facts.[17] The first of these ways allows us to observe a distinction between the role empirical data play in a realist and in merely empiricist or confirmationist approach: in the latter, there is no intrinsic reason why new empirical data are looked for; i.e., once we have some set of empirical data,

[15] Cf. the definition of 'theoretical progress' in Lakatos 1978, p. 66.
[16] See., e.g., Mäki (1994), and for a recent proposal, Mäki (forthcoming).
[17] I understand 'prior' as opposed to 'conditional', rather than as opposed to 'posterior'.

they generate a value for the posterior probability (or degree of confirmation) of the theories, and that's all; it can be the case that we happen to discover new data, but there is no reason based on our epistemic utility function that lead us to *want* to have more data or to actively pursue it. In the realist case, instead, discovering *new* data (i.e., new 'empirical truths') is a way of increasing the verisimilitude of the theories (if these happen to explain well the new data). But what about the second and third ways? Does not this 'changing prior probabilities' sound as exactly the opposite of honest realism? Well, let's see.

My point is that the fact that 'manipulating' prior (or 'unconditional') probabilities can affect the epistemic value of scientific claims, is an ideal place to observe the interconnection of rhetoric and realism. Note that the verisimilitude of a theory T that successfully explains empirical evidence E, equals $p(T)/p(E)$[2]. Hence, in principle, what a scientists attempts to do to perform well in a game whose rules promote the maximisation of that value, is to find a theory that *a)* has a high unconditional probability (i.e., its probability not taking into account the empirical data), and that *b)* from it we can deduce empirical facts whose unconditional probability is as low as possible. These two goals are usually in mutual conflict: in order to explain well many things, theories must have a *low* prior probability (this is a truism: if T entails E, then $p(T) \leq p(E)$), and this trade-off is basically what makes the game of science difficult and exciting! But this is a trade-off, i.e., a constraint, *not* a logical contradiction, and so, what is important is the theory's being as 'likely' as it can be, taking into account how 'unlikely' are the facts it explains. So, a way to make your theory more valuable from the point of view of your colleagues is to show by means of any plausible arguments why the principles and hypotheses in which your theory is based are not too unlikely (or are even a matter of fact!), and also to show that the facts the theory explain are extremely unlikely. Arguing for the strangeness of the facts you have been able to explain, and for the platitudeness of the ideas on which your theory is based, serves this to score higher in the valuations your colleagues make.

We might call 'rhetoric' this kind of arguments whose function is to modify the prior or unconditional probability of one statement.[18] They are rhetoric neither in the negative sense we saw in section 1 (for they are not 'irrational' or 'not logically valid'), nor in the positive sense (for they are only a *part* of the arguments scientists employ), but I think the restriction of the term 'rhetoric' to this type of arguments is more consistent with the common usage: arguments that are rationally appropriate, but that are not aimed neither as a logico-mathematical proofs, nor supplying empirical support. Many arguments in science (I would say, everywhere when there is an argument, but not a logical or empirical proof) are of this kind. But they are a consequence of small-case realism, i.e., of the fact that scientists look for theories and models that *seem verisimilar to them*. It also explains why, after a scientific revolution, a lot of work is done by scientists (and in this case, often also by philosophers) that consists in something like discussing 'metaphysical foundations', i.e., in showing that the principles of the new paradigm are not so strange after all, nor the ones of the old paradigm so obvious. Lastly, the existence and instrumental value of these type of arguments does not entail that *any* theory may have *any* epistemic value we want, rather on the contrary: whether an argument succeeds or fails to modify our, or our colleague's prior probabilities, is just a matter of fact, and so, *we cannot manipulate the epistemic value of a theory at will*. So, these types of rhetorical arguments are, in fact, an example of 'rhetoric at the service of realism'.

---

[18] In a different sense, I have used the term 'rhetoric' to refer to *the strategic use of language* (Zamora Bonilla 2006a).

In conclusion, the inferentialist model of science allows to make sense of small-case realism by showing what is exactly meant by saying that realism is not so much a philosophical problem as a value or goal for flesh-and-bone scientists. If philosophers try to oppose this move by considering that it goes too far, or not too-far, on the route-666 of relativism, the only reasonable answer is to invite them to consider their own epistemic preferences, and to show how would they *behave* in the pursuit of their conceived epistemic goals if they happened to be the scientists, i.e., what would they do in order to 'reveal' their epistemic preferences. Though, in the end, we must not forget that *de epistemicibus gustibus non est disputandum*. Or is it?

ACKNOWLEDGEMENTS

Financial support from Spanish Government's research projects FFI2008-03607/FISO and FFI2008-01580/FISO is acknowledged.

REFERENCES

Hands, W., 1999, "Empirical realism as meta-method: Tony Lawson on neoclassical economics", in Fleetwood, S. (ed.), 1999, *Critical realism in economics: Development and debate*, London, Routledge, 169-185.

Hausman, D., 1998, "Problems with Realism in Economics," *Economics and Philosophy*, 14, 185-213.

Gross, A. G., 1990, *The Rhetoric of Science*, Cambridge (Mass.), Harvard University Press.

Lakatos, I., 1978, ‘Falsification and the Methodology of Scientific Research Programmes’, in I. Lakatos, *The Methodology of Scientific Research Programmes. Philosophical Papers. Vol. 1*, Cambridge University Press, Cambridge.

Lawson, T., 1997, *Economics and Reality*. London, Routledge.

Mäki, U., 1988, “Rhetoric, economics, and realism: A rejoinder to McCloskey”, *Economics and Philosophy*, 4, 167-169.

Mäki, U., 1994, "Isolation, idealization and truth in economics", in Hamminga, B., and N. de Marchi (eds.), *Idealization in Economics*. *Poznan Studies in the Philosophy of the Sciences and the Humanities*, 38, 147-168.

Mäki, U., 1995, "Diagnosing McCloskey", *Journal of Economic Literature*, 33, 1300-1318.

Mäki, U., 2000, “Performance against dialogue, or answering and really answering: A participant observer’s reflections on the McCloskey conversation”, *Journal of Economic Issues*, 34.1, 43-59.

Mäki, U., (ed.), 2009, *The Methodology of Positive Economics. Reflections on the Milton Friedman Legacy*., Cambridge, Cambridge University Press.

Mäki, U., 2010, “When economics meets neuroscience: Hype and Hope”, *Journal of Economic Methodology*, 17.2, 107–117.

Mäki, U., forthcoming, “The truth of false idealizations in modelling”, in Humphreys, P., and C. Imbert (eds.), *Representations*, *Models and Simulations*, London, Routledge.

McCloskey, D., 1985, *The Rhetoric of Economics*, Madison, University of Wisconsin Press.

McCloseky, D., 1994, *Knowledge and Persuasion in Economics*, Cambridge, Cambridge University Press.

McCloskey, D., 1995, "Modern Epistemology Against Analytic Philosophy: A Reply to Mäki," *Journal of Economic Literature* 33, 1319-1323.

McCloskey, D., 1997, *The Vices of Economists; The Virtues of the Bourgeoisie*. Amsterdam, University of Amsterdam Press.

McCloskey, D., 2003, “You shouldn’t want a realism if you have a rhetoric”, in Mäki, U., (ed.), *Fact and Fiction in Economics*, Cambridge, Cambridge University Press, 329-340.

McCloskey, D., and S. Ziliak, 2008, *The Cult of Statistical Significance: How the Standard Error Costs Us Jobs, Justice, and Lives*. Ann Arbor, University of Michigan Press.

Pera, M., 1994, *The Discourses of Science*, Chicago, The University of Chicago Press.

Reed, I., 2008, “Justifying Sociological Knowledge: From Realism to Interpretation”, *Sociological Theory*, 26.2, 101-129.

Reed, I., 2010, “Epistemology Contextualized: Social-Scientific Knowledge in a Postpositivist Era”, *Sociological Theory*, 28.1, 20-39.

Rorty, R., and E. Mendieta, 2005, *Take Care of Freedom and Truth Will Take Care of Itself: Interviews with Richard Rorty*, Stanford, Stanford University Press.

Zamora Bonilla, J. P., 1996, “Verisimilitude, Structuralism and Scientific Progress”, *Erkenntnis*, 44:25-47.

Zamora Bonilla, J. P., 2002, “Verisimilitude and the Dynamics of Scientific Research Programmes”, *Journal for General Philosophy of Science*, 33, 349-68.

Zamora Bonilla, J. P., 2006a, “Rhetoric, Induction, and the Free Speech Dilemma”, *Philosophy of Science*. 73, 175-93.

Zamora Bonilla, J. P., 2006b, “Science Studies and the Theory of Games”, *Perspectives on Science*, 14, 639-71.

Zamora Bonilla, J., 2010, “What games do scientists play? Rationality and objectivity in a game-theoretic approach to the social construction of scientific knowledge”, in Suárez, M., M. Dorato and M. Rédei (eds.), *EPSA Epistemology and Methodology of Science. Launch of the European Philosophy of Science Association*, vol. 1., Dordrecht, Springer, 323-332.

Zamora Bonilla, J., 2011, “Rationality in the social sciences: bridging the gap”, in Jarvie, I., and J. Zamora Bonilla (eds.), *The SAGE Handbook of Philosophy of Social Science*, ch. 38. London, SAGE.