

## KITCHER'S MODEST REALISM: THE RECONCEPTUALIZATION OF SCIENTIFIC OBJECTIVITY

**Antonio Diéguez**

Forthcoming in W. J. González (ed.), *Scientific Realism and Democratic Society: The Philosophy of Philip Kitcher*, "Poznan Studies in the Philosophy of the Sciences and the Humanities", Amsterdam: Rodopi, 2010.

### **Abstract**

In *Science, Truth and Democracy*, Kitcher moderates the strongest ontological realist thesis he defended in *The Advancement of Science*, with the aim of making compatible the correspondence theory of truth with conceptual relativity. However, it is not clear that both things could be harmonized. If our knowledge of the world is mediated by our categories and concepts; if the selection of these categories and concepts may vary according to our interests, and they are not the consequence of the existence of certain supposed natural kinds or some intrinsic structure of the world, it is very problematic to establish what our true statements correspond to. This paper analyzes the transformation in Kitcher's realism and expounds the main difficulties in this project. Finally, a modality of moderate ontological realism will be proposed that, despite of keeping the spirit of the conceptual relativity, is strong enough to support the correspondence theory of truth.

### **KEY WORDS**

MODEST REALISM, CONCEPTUAL RELATIVITY, CORRESPONDENCE THEORY OF TRUTH

The last two decades have been a period of deep changes in the realm of the philosophy of science. Not only its hegemony among meta-scientific disciplines has been challenged by the sociology of science and, in general, by the social studies of science, but the objective that guided it from the beginning of its academic institutionalization with the Vienna Circle in the 1930's —i. e. the

attempt to provide a global understanding of the aims, methodological rules, and differential epistemological traits that characterizes scientific knowledge— has been seriously disputed. An increasing number of authors seem to agree with Feyerabend that, from the axiological, methodological or epistemological point of view, there is nothing of interest that is common to all particular sciences. Hence, generic philosophical discourse about science is decreasing in recent publications to the benefit of specialized approaches about some concrete disciplines, especially economics, psychology and biology.

I do not believe that this generic discourse is destined to disappear. On the contrary, I rather think that there is room enough and sufficient tasks ahead for the development of a general philosophy of science. Actually, two of the more discussed problems in the recent philosophy of science, i.e., the problem of rationality in the choice of theories and the debate about realism, are part of this generic discourse- They have attracted and continue to interest many important philosophers of science. But, what is undeniable is that the panorama of the philosophy of science has now become more complex, more diverse, and also more diffuse than previously. Thus, for better or for worse, one does not find anymore figures as all-embracing and influential in other branches of philosophy or even in the sciences as Popper, Kuhn, Lakatos and Feyerabend.

Nevertheless, if we had to mention present-day philosophers of science who are in line with this great tradition that marked the character of the philosophy of science during the second half of 20<sup>th</sup> century, Philip Kitcher would be doubtless among them. Philip Kitcher is a philosopher of science that knows in some depth diverse scientific disciplines and has made important contributions to the philosophy of particular sciences, like mathematics and biology, being as well the author of *The Advancement of Science*, an extremely influential book on the general philosophy of science, published in 1993. Kitcher is well-known for his works on the philosophy of mathematics (particularly in his book *The Nature of Mathematical Knowledge*, 1983); for his criticism of creationism (in *Abusing Science*, 1982), intelligent design (in *Living with Darwin*, 2007), and sociobiology (in *Vaulting Ambition*, 1985); and for his reflections about the possibilities opened up for humans from biotechnology (in *The Lives to Come*, 1996). Among his more characteristic theoretical contributions are the following five: (1) his conception of scientific explanation as unification (cf. Kitcher 1981; 1989a), (2) his defense, from the perspective of a naturalist approach, of a moderate realism, and his retorts to the main antirealist arguments (such as the pessimistic induction and the underdetermination) (cf. Kitcher 1993a, chaps. 5, 6 and 7; 2001a, part I; 1993b; 2001b; 2002a), (3) his criticism of relativist and constructivism positions in Science Studies (cf. Kitcher 1993, chap. 6; 1998; 2001a), (4) his defense of pluralism about biological species and about the units of selection, together with his criticism of genetic

reductionism in biology (cf. Kitcher 1984a; 1984b; 1989b; Kitcher and Sterelny 1988), and (5) his analyses of the social and political background of the research agenda in present science (cf. Kitcher 2001a; Kitcher and Flory 2004).

Kitcher's general philosophy of science connects with the so-called "naturalistic turn" in philosophy of science (cf. Callebaut 1993; Zamora Bonilla 2000). That is to say, his philosophy tries to study science from an empirical basis, instead of doing it from immutable and supposedly universal epistemological preconceptions. He takes advantage, therefore, of the resources and results of some empirical sciences (particularly cognitive sciences, biology, and economics) in order to develop the pertinent epistemic models about scientific practice. The aim of this approach is not to provide a logical reconstruction of scientific theories or of scientific explanations, in the neopositivistic way; nor is it to offer a general view of the relation between the contents of theories and empirical evidence, as if this relation were context-independent; or to give an account of some allegedly timeless canons of rationality. Kitcher names ironically all of these aspirations, easily identifiable in the epistemological tradition, "The Legend." Rather, the objective is to understand how scientists really act when they have to make important decisions about the theories that they sustain. It is precisely this attention to the real *scientific practice* that places Kitcher's philosophy close to what can be called the "pragmatic turn" in the philosophy of science (also well represented by Ian Hacking (1983) and Andrew Pickering (1995)). The questions concerning the aims and values of scientific research, and the ways in which they condition scientific decisions and are related to democratic ideals, are of central importance in Kitcher's work.

My objectives in this paper are to analyze the main theses of the realism that Kitcher defends in *The Advancement of Science* (abbreviated as *AS*), and to evaluate the transformation this realism undergoes in his later book *Science, Truth, and Democracy* (abbreviated as *STD*). I will argue that, although these transformations are important, they are nevertheless those that can be strictly outlined when some aspects of science as social activity are faced from the position of a moderate realism. Finally, I will expose some internal ontological troubles that arise within Kitcher's realist proposal and offer a possible solution.

### **1. Kitcher's Realism in *The Advancement of Science***

Let us start by explaining what Kitcher means by realism. In *AS* (p. 127) he sums up his realist view of science with the following words: "scientists find out things about a world that is independent of human cognition; they advance true statements, use concepts that conform to natural divisions, develop schemata that capture objective dependencies." Two basic components

of scientific realism are to be appreciated in this characterization: the *ontological* and the *epistemological*. According to the first, there is a world independent of any act of knowledge; according to the second, science could provide us with an adequate knowledge about the characteristics of this independent world. Kitcher goes even further and claims that scientists provide “true statements” (in the sense of truth as correspondence). His realism is, then, also a *semantic* realism, and on this point he follows Popper’s interpretation of Tarski’s work. Kitcher is one of the philosophers who believes that Tarski rehabilitated the traditional notion of truth as correspondence; a notion that would be based on daily use (cf. Kitcher 1993a, pp. 128-133; and, for an explanation of the components of scientific realism, Diéguez 1998). In addition, some pages before, Kitcher indicates that the scientific community might pursue both epistemic and non-epistemic aims; being the obtaining of significant truths one fundamental (and permanent) epistemic goal. So, at least in part (although in an important part), scientific progress is measured by the elimination of falsehood in favor of significant truth (cf. Kitcher 1993a, pp. 94, 117 and 120).<sup>1</sup> This concept of significance, outlined in this work, will play a fundamental role in *Science, Truth, and Democracy*, where it will be developed substantially.

However, Kitcher’s most explicit characterization of realism can be found in a work titled “A Plea for Science Studies,” written five years later. He presents there the main claims—which he considers uncontroversial—of “the Realist-Rationalist Cluster” (Kitcher 1998, pp. 34-35):

1. In the most prominent areas of science, the research is progressive, and this progressive character is manifested in increased powers of prediction and intervention.
2. Those increased powers of prediction and intervention give us the right to claim that the kinds of entities described in scientific research exist independently of our theorizing about them and that many of our descriptions are approximately correct.
3. Nonetheless, our claims are vulnerable to future refutation. We have the right to claim that our representations of nature are roughly correct while acknowledging that we may have to revise them tomorrow.
4. Typically our views in the most prominent areas of science rest upon evidence, and disputes are settled by appeal to canons of reason and evidence.
5. Those canons of reason and evidence also progress with time as we discover not only more about the world but also more about how to learn about the world.

---

<sup>1</sup> In “Knowledge, Society, and History,” Kitcher writes: “We mark the difference between those propositional states that accurately represent reality from those that do not by dividing the pertinent propositions into those that are true from those that are false. The notion of truth as correspondence is this not some peculiar metaphysical extravagance but part of an important way of thinking about ourselves and others,” (Kitcher 1993b, p. 166). Nevertheless, he concedes to Kuhn that the notion of truth cannot be applied to whole theories, (cf. Kitcher 1993a, p. 129).

As can easily be noted, this brand of realism has a clear Popperian flavor and it is even stronger than that proposed by some of Popper's heirs. It is not a naïve realism, but a critical and fallibilist one (thesis 3). It is also plainly rationalist (thesis 4), although, unlike Popper's realism, it is open to the possibility that canons of rationality may change historically (thesis 5).

Despite the fact that Kitcher considers these five theses to be uncontroversial, he acknowledges in a footnote that the sense of thesis 2 has been legitimately disputed by some philosophers, such as Hilary Putnam, Arthur Fine, Nelson Goodman and Richard Rorty. He thinks that for these authors the problem with thesis 2 is not that it cannot have some acceptable sense, but that traditionally it has been loaded with questionable metaphysical additions. As for the authors, such as the antirealists from the field of *Science Studies*, whose rejection of the realist theses is more global, they have to take on the burden of proof. They have to show, with sound arguments, why the former theses are to be discarded. Indeed, Kitcher sees realism as the natural attitude that human beings develop spontaneously in their cognitive dealings with the world. If the antirealist attitude proliferates in philosophy, it is because it is erroneously believed that there are arguments against realism that cannot be satisfactorily answered.<sup>2</sup> Seemingly, with this elucidation, Kitcher wishes to separate semantic antirealism (as that proposed by pragmatists and Putnam's internal realism) from the radical antirealism coming from constructivist and relativist positions. If the former may be useful in order to discard some exaggerated claims of the realist position, the latter simply has no sufficient argumentative ground. Not surprisingly, Kitcher ends up by accepting in *STD* some ideas of the first type of antirealism.

Thesis 1 and thesis 2 jointly constitute the basis of a central argument adduced by realists and, consequently, one of the main targets of antirealist criticism. It is the sort of argument known as 'inference to the best explanation', in this case applied to realism as an explanation of scientific success. The argument is as follows: the enormous predictive, explanatory, practical... success of science is a surprising fact that requires an explanation; the best explanation of such success is to suppose that the entities referred by scientific theories do exist and that these theories formulate (approximately) true claims about them (as realism holds); therefore, realism should be accepted. Sometimes the argument is strengthened, as Putnam managed at the initial realist phase of his intellectual trajectory, with the claim that otherwise, scientific success would be 'a miracle' (cf. Putnam 1978, p. 19). That would imply that realism is not only the best explanation of scientific success, but actually the only sensible one.

---

<sup>2</sup> Kitcher thinks that the advantages of realism are decisive, so that the burden of proof lies with the antirealists (cf. Kitcher 1993a, p. 132). But in "Real Realism: The Galilean Strategy," (2001b, p. 171), Kitcher gives up the conviction that realism is the default position, the common sense position; and he holds that, after overcoming the antirealist arguments, positive arguments should be provide in favour of realism.

Bas van Fraassen (1980, pp. 19-40; 1989, pp. 142-150), Larry Laudan (1981, pp. 19-48), Michael Levin (1984, pp. 124-139) and Arthur Fine (1986, pp. 114 and ff.) have expressed the main criticisms to this argument. In *AS*, Kitcher focus his attention on answering Laudan, since he believes that Laudan offers the best antirealist strategy, but he also makes some critical references to van Fraassen's constructive empiricism (cf. Kitcher 1993a, pp. 150-157; see also 2001a, pp. 19-24; 1993b; 2001b).<sup>3</sup> Laudan's argument against realism is known as 'the pessimistic induction'. It appeals to the history of science; and its central idea is very simple: approximate truth and correct reference cannot be, as realists claim, the best explanation of scientific success, for there can be found in the past (and not in a very distant past) empirically successful theories whose central terms failed in their reference, i. e., they referred to entities considered nowadays as non-existent, like the phlogiston, the ether, the crystalline spheres, the caloric, the vital force, etc. If the realist is right, then how could these false theories have been successful in the past? They do not merely include some inaccuracies or make some specific mistakes, but are grounded on the postulation of completely non-existent entities.

Kitcher responds to this argument by denying that historical record supports that pessimism. In principle, as Michael Devitt (1984) points out, there are reasons to maintain just the opposite. In Devitt's view, if a conclusion should be drawn by inductive reasoning from historical cases, there are more reasons to draw an optimistic conclusion. There have been an increasing number of cases of approximate truth and preserved reference in theoretical terms (from the point of view of current science), at least if we consider recent historical periods. However, since no one counted the number of referring and non-referring theoretical terms in the historical record, it is nothing more than an impression. On the other hand, even it were true that the amount of theoretical terms that keep their reference surpass the amount of non-referring terms, that would still leave unexplained the success of the theories mentioned by Laudan, and it would not explain why the persistence of the currently postulated theoretical entities makes its existence more credible than the existence of the entities postulated in the past, some of which have survived for a long time. This is the reason that impels Kitcher to look for a more forceful counter-argument.

He considers, then, a more plausible way to get an optimistic conclusion from the historical record. Throughout the history of science, successive theories (or explanatory schemata, statements, etc.) appear closer to the truth than previous ones from the perspective of some other

---

<sup>3</sup> Michael Levin's criticism consists of arguing that truth has no explanatory capacity. Kitcher responds in his (2002a). Van Fraassen's criticism aims to show the fallacious character of the realist argument. The point is that, either the argument presupposes what is to be proved—that the hypothesis that best explains a fact is true for that very reason—, and therefore it is a circular argument, or it excludes, without justification, the possibility that all explanations of scientific success we have at the moment are unsuitable and, consequently, the realist explanation would just be the best one in a bad lot of explanations, without merits enough to be accepted. As far as I know, Kitcher has not answered this criticism, even though he has argued against constructive empiricism in general.

later theory. That authorizes us to suspect that our current theories will appear also in the future closer to the truth than do past theories. However, this proposal still seems too weak to Kitcher, for it assumes a generalized fallibilism for which the history of science gives no motive. We are not equally subject to error about everything. About some things we have obtained a firmer knowledge than about others. For that reason, “instead of a blanket pronouncement to the effect that our current theories are probably wrong, it would be far more instructive to investigate the stability of various components of practice in various fields.” (Kitcher 1993a, p. 138). It should be unveiled by reviewing the history of science and also with the aid of the cognitive sciences “where we are most likely to make mistakes and where we are legitimately confident.” (Kitcher 1993a, p. 138, note 16).

Accordingly, in order to be convincing and undermine realist optimism Laudan’s argument would have to show not only that in the past there were non-referring and still successful theories, but also that among the parts of these theories responsible for their success there were some considered false from a realist perspective, i. e., some non-referring parts. The reason is clear: “it is not enough to conceive a theory as a set of statements and distribute the success of the whole uniformly over the parts. One has to see how the statements are *used*.” (Kitcher 1993a, p. 143). Laudan’s pessimistic induction fails, then, until it can be shown that the phlogiston, the crystalline spheres, the ether, and the other non-extant theoretical entities were essential elements in the success of the theories to which they belonged. The facts seem to indicate, however, that things were not like this. The postulation of ether, for example, was no more than a heuristic resource without any function in the predictive success of Maxwell’s electromagnetic theory.<sup>4</sup> In this respect, Kitcher distinguishes within the scientific practice between the “working posits” and the “presuppositional posits.” The former are the referents of theoretical terms that occur in problem-solving schemata; the latter are the entities that apparently have to exist if the solutions to the problems provided by the theory are true. Laudan would have shown, at best, that presuppositional posits are suspect, but he would not have proved anything about the working posits.<sup>5</sup>

Kitcher also enlarges in *AS* on the answers to other well-known realist arguments: the theory ladenness of observation and the underdetermination of theories by evidence.<sup>6</sup>

---

<sup>4</sup> This argument has also been unfolded and illustrated with concrete historical cases by Stathis Psillos (1999, chaps. 5 and 6).

<sup>5</sup> Kitcher returns to this problem in *STD* (cf. Kitcher 2001a, pp. 16-19). However, apart from the replacement of the expression ‘working posits’ by ‘idle wheels’, he does not add anything new to his arguments, although there he insists about a point he have previously adduced (in Kitcher 1997a, p. 299), to wit, it is far from clear that the theories mentioned by Laudan were successful in some scientifically important sense (i.e. in their predictive and practical capacities).

<sup>6</sup> Both arguments are also answered in very similar terms in “A Plea for Science Studies” (Kitcher 1998, pp. 38-41), and in *STD* (Kitcher 2001a, pp. 14-15 and chap. 3). In *The Advancement of Science*, Kitcher faces another important antirealist challenge, although not in great detail: the incommensurability problem. He thinks that his notion of

The thesis of the theory ladenness of observation is not properly an antirealist thesis. However, antirealists make frequent use of it, since, in a radical interpretation, may lead to antirealism. In effect, if all observation is theory-laden, and one assumes that observation is the basic touchstone to choose among theories, then the choice always will be made from the assumptions of a theory. That is interpreted by the antirealist as a recognition of the impossibility of an objective choice among theories, and, in extreme cases, as an acknowledgement that the notion of objective reality is completely non-functional and, therefore, senseless. In this extreme interpretation, what we call ‘objective reality’ is something constructed by our conceptual schemes or by our social practices. Kitcher points out two possible strategies in order to respond to this use of the theory-ladenness thesis. The first one is Fodor’s strategy, which shows that “the plasticity of observation has been greatly overrated” (Kitcher 1993a, p. 225). The other strategy is to resort in the problematic situations to shared beliefs for the resolution of the conflicts. In that case, the interpretation of the same stimuli by scientists with different theoretical backgrounds need not be different. After analyzing by means of this second strategy the dispute between Galileo and the Aristotelians about observations with the telescope —and rejecting Feyerabend’s claims on this topic—, Kitcher concludes: “[W]e can recognize the dependence of observation and of observational reporting on background cognitive states without abandoning the thesis that the propensities acquired by specialists are observational skills. Trained professionals can demonstrate their virtuosity in ways that are appreciable by the laity, and so turn back the challenge that their alleged skill is merely masquerade.” (Kitcher 1993a, p. 233). In other words, even though it is accepted that observation is theory-laden, there are several ways of improving it (e. g. by making it more accurate, more detailed, less prone to error, etc.), and everybody can recognize these ways of improved observation as the best in a context.

Therefore, the theory-ladenness of observation does not prevent us from considering some observations as better than others. The conflict between incompatible observational reports need not be unsolvable in a rational and objective way. The error of many antirealists, like Kuhn, is that they jump from the very reasonable thesis (although disputed by Fodor and some others) that our perception of nature is dependent on our theories, to the less justifiable claim that *nature itself* is

---

reference potential can explain how the reference is partially preserved through the changes of theories. Therefore, it neutralizes the objection raised by incommensurability to realism, according to which it is impossible to see the changes of theories as a progress towards a truer conception of reality if the reference is not maintained in these changes. The idea behind the notion of reference potential is that terms do not refer in a homogeneous way. There are several heterogeneous possibilities in which scientific community may fix their reference in each concrete case. Although, as Kuhn held, after a change of theory the reference potential has changed, it is always feasible to recognize the reference of some tokens of a term, and offer a translation of these tokens into the language of the new theory. We cannot find, for example, something equivalent to ‘phlogiston’ in the post-Lavoisier chemistry, but we can say that in some cases ‘dephlogisticated air’ was employed by Priestley referring to oxygen (cf. Kitcher 1993a, pp. 78 and 97-105; 1982b, pp. 337-359). For a criticism of this position, see Niiniluoto (1999, pp. 131-132) and McLeish (2005).



dependent on our theories. Certainly, we do not have an Archimedean point (the God's Eye point of view in Putnam's terms) to establish beyond doubt that there is a match between our theories and reality. All access to reality are linguistically (or, in the traditional version of this argument, mentally) mediated, and, therefore, as Popper saw, they are theoretically mediated. But, this need not lead to deny an independent reality, or to deny that we can get access to it in more and more adequate ways (cf. Kitcher 1993a, pp. 131-132).<sup>7</sup>

As for the underdetermination argument, Kitcher thinks that it may be of some logical interest, but it has little to do with the actual scientific practice, wherein one hardly finds two completely equivalent theories in their observational consequences but different in their non-observational postulates. If the prior scientific practice is taken into account, the serious empirically equivalent rivals of a given theory with respect to the same evidence are few in number. Furthermore, the contribution of new evidences may eventually dissolve this rivalry.<sup>8</sup>

Kitcher also follows other critics of underdetermination, like Laudan, when he adduces that the consistency of a theory with the empirical evidence is one thing, and quite another is the support this evidence gives to such a theory. There may be two theories equally consistent with a set of evidences and, nonetheless, one of them may be better supported by these evidences than the other. Finally, in Kitcher's view, the epistemological costs of maintaining a rival hypotheses and dropping the other could be, in a certain moment, too great —and even inadmissible—. As it would have happened if, for example, some version of geocentrism empirically equivalent to

---

<sup>7</sup> As we said in a previous note, in "A Plea for Science Studies", he returns to the issue and he strengthens his arguments, although this time he exonerates Kuhn from any responsibility: "As Thomas Kuhn [...] clearly saw, the fact that concepts and categories are involved in observation doesn't mean that the content of experience is determined by them or that we cannot be led by experience to reconceptualize the phenomena. Nor does it imply that we are somehow 'cut off' from the world or that the only world we can talk about must be 'constructed'. [...] In perception, we are in causal contact with physical objects, and although this contact is mediated by our *having* certain kinds of psychological states ('perceptions', 'representations'), we do not perceive by *perceiving* those states. [...] So it would be more accurate to say not that the world is shaped by our categories but that our representations of the world are so shaped and that the shaping is open to empirical investigation." (Kitcher 1998, pp. 38-39). The same idea appears in *STD* (cf. Kitcher 2001a, p. 15).

<sup>8</sup> In *STD* Kitcher considers the possibility of finding complete and permanent empirical equivalences between rival theories (cf. Kitcher 2001a, pp. 34-35). He concludes that it would be necessary to analyze each particular case. Sometimes such theories might be two linguistic formulations of the same theory (as in the case of Schrödinger's wave mechanics and Heisenberg's matrix mechanics), on other occasions it might not be possible to make a scientifically based decision. In this work (p. 36), he concedes more importance to the problem. He does not see it anymore as a problem of scarce interest out of the philosophical realm, but he stresses its existence in scientific practice. Nonetheless, he remarks that it has less extent than it is supposed. It is not difficult to point out some cases in which it is not conceivable a serious and empirically equivalent alternative to an accepted theory. "What's —he asks— the supposed rival to the hypothesis that the typical structure of DNA is a double helix with sugar-phosphate backbones and bases jutting inwards?" He adds (p. 32) also a new argument to show that, in case of real situations of complete and permanent empirical equivalence, the proponents of underdetermination could not maintain their theses. According to several proponents of underdetermination, particularly some sociologists of science, granted that scientific theories are always underdetermined by empirical evidence, the decision between a theory and its empirically equivalent rival will be always grounded on social and political preferences. Now then, to be coherent, the proponents of underdetermination should also accept that the thesis that a theory supports certain social and political values better than its rival is also empirically underdetermined. Therefore, the appeal of these non-epistemic values would not endow the choice with rationality.

Copernican heliocentrism had been maintained by someone in post-Copernican astronomy (cf. Kitcher 1993a, pp. 247-256).<sup>9</sup>

Summing up, the realism Kitcher displays in *The Advancement of Science* is a strong realism, as far as it goes beyond the minimal ontological realism in the style of the entity realism defended by Ian Hacking, Nancy Cartwright and some sociologists of science. Kitcher's realism accepts the epistemological thesis that we can know reality in an adequate way; it is committed to the correspondence theory of truth; and it even claims that science provides us with many truths. There is, however, a stronger modality of realism that Kitcher does not reject, but neither does he openly embrace it. This modality holds that nature has its own causal structure (including natural kinds and objective dependences among phenomena), which is fixed independently of human cognition, and that our conceptual schemes are able to carve up this structure into its joints. Implied is that there can only be one correct way of representing a certain part of the world. True enough, Kitcher does not adhere either to the rival Kantian position, defended by Sellars and Putnam's internal realism, which sees the structure of the world as a subject's projection. Surprisingly, he presents his realism as compatible with both alternatives (cf. Kitcher 1993a, pp. 169-173).

In spite of that, Kitcher tones down in some ways the forcefulness of this realism. He notes, for example, that changes in scientists' individual and collective practices —particularly changes in the identification of significant problems, and in the acceptance of explanatory schemes and statements— do not depend only on dictates of empirical evidence (on “encounters with nature,” in Kitcher's terms), but also depend (although not in an exclusive way, as some sociologists assume) on the “conversations with peers.” Social factors have a role in the scientist's decisions, but they are not determinant factors. Kitcher proposes an intermediate stance between extreme internalism and extreme externalism:

The position that the sociologists attack is the extreme suggestion that social determinants make no difference: given the same inputs from asocial nature there will be the same modifications of consensus practice, no matter what the social structure. They counterpose the extreme view that inputs from nature are impotent, or at least impotent with respect to the framework of consensus practice: given the same social structure, there will be the same modifications of consensus practice, no matter what the inputs from asocial nature, and, in particular, the

---

<sup>9</sup> In “A Plea for Science Studies,” he sums up in a few words his position about the underdetermination problem: “Duhem started a line of thought that enables us to see that there is no instant rationality in science, but it's wrong to conclude from this that there are not context-independent standards of good reasoning that, when applied to increasingly comprehensive experiences, resolve scientific debates. In the early phases of the chemical revolution, phlogistonian could offer alternative analyses of the chemical reactions that Lavoisier viewed as showing the absorption or release of oxygen. As the number of findings increased, it became more and more difficult —and ultimately impossible— to find any consistent and unified way of treating all the reactions.” (Kitcher 1998, p. 40).

framework of consensus practice will remain unaltered. I claim that there is a vast, unexplored middle ground between these extremes. (Kitcher 1993a, p. 164).

In Kitcher's view, only detailed historical studies could indicate to what extent social factors and the inputs from nature contribute to such decisions. From his rationalist position, half way between the two mentioned extremes, the closure of scientific debates is mainly the result of arguments and not of social factors; but he concedes that often this is a long and complex process and even the arguments might occasionally not have carried the greater weight in the decision (cf. Kitcher 1993a, pp. 162, 169 and 201-202).

## **2. The Change to a Modest Realism: *Science, Truth, and Democracy***

*Science, Truth, and Democracy* introduces substantial changes with regard to the realism presented in *The Advancement of Science*. According to Kitcher, the fundamental change was the acceptance of pluralism. The basic premise of pluralism sustains the idea that the world can be represented in very different ways for scientific purposes, without any of them ever being a complete representation. Besides, pluralism claims that, although all the representations which conform to nature are consistent, the representations accepted by scientists in any stage in the history may be inconsistent (cf. Kitcher 2002b, pp. 570-571). In other words, the pluralism defended by Kitcher in *STD* is the negation of metaphysical realism as it was defined by Hilary Putnam, and especially its commitment to a God's Eye View. Or as Kitcher explains, it is the negation of the thesis that "[t]he world comes to us prepackaged into units, and a proper account of the truth and objectivity of the sciences must incorporate the idea that we aim for, and sometimes achieve, descriptions that correspond to natural divisions." (Kitcher 2001a, p. 43).<sup>10</sup> But Kitcher's proposal differs from Putnam's internal realism, since it does not give up the correspondence theory of truth. This union of the conceptual relativity defended by Putnam with the correspondence theory of truth approximates Kitcher's modest realism to the critical scientific realism that the Finnish philosopher Ilkka Niiniluoto has been elaborating on for more than two decades (cf. Niiniluoto 1984; 1999).<sup>11</sup>

---

<sup>10</sup> In one of the more recent formulations, Putnam characterizes metaphysical realism as follows: "[T]here is a totality of Forms, or Universals, or "properties," fixed once and for all, and [...] every possible meaning of a word corresponds to one of these Forms or Universals or properties," (Putnam 1999, p. 6).

<sup>11</sup> In *STD* (p. 205) Kitcher explicitly says his realism is similar to those of Richard Miller and Jerrold Anderson; and in "Real Realism: The Galilean Strategy" (note 72) he claims that perhaps Putnam's last works are close to his modest realism. As it is well known, Putnam has rejected his former conception of truth as warranted assertability in ideal epistemic conditions.

But this is not the only novelty Kitcher introduces in *STD*. In my opinion there are other important ones: (1) the insistence on the diversity and contextual dependence of the aims of scientific research, (2) the development of the notion of ‘significance’ as a contextual notion, (3) the greater weight given to the practical interests in science, (4) the admission that truth and knowledge are not absolute values, but are to be integrated with other non-epistemic values, and, finally, (5) the search for a “well-ordered” science, that is, a science compatible with democratic ideals.

From this short enumeration of novel contributions, it can be easily inferred that, compared to the realism offered by Kitcher in *The Advancement of Science*, the modest realism of *STD* tries to incorporate the most reasonable contributions made from the antirealist point of view and from the social studies of science. In “A Plea for Science Studies” (Kitcher 1998, p. 36), Kitcher conceded that the antirealist side also had in its favor some theses well supported by all we know about scientific practice. Such theses form what he calls “the Socio-Historical Cluster,” as opposed to the previously quoted “Realist-Rationalist Cluster”:

1. Science is done by human beings, that is, by cognitively limited beings who live in social groups with complicated structures and long histories.
2. No scientist ever comes to the laboratory or the field without categories and preconceptions that have been shaped by the prior history of the group to which he or she belongs.
3. The social structures present within science affect the ways in which research is transmitted and received, and this can have an impact on intratheoretical debates.
4. The social structures in which science is embedded affect the kind of questions that are taken to be most significant and, sometimes, the answers that are proposed and accepted.

Obviously, the most distant point from the realist position defended in *AS* is the end of thesis 4. In fact, the rest is recognized, one way or another, in *AS*.<sup>12</sup> A scientific realist has no difficulty in accepting that social structures affect the setting of a research agenda, that is to say, “the kind of questions that are taken to be most significant”; but it is a horse of a different color for the realist to concede that social structures affect “the answers that are proposed and *accepted*,” especially if it means that what answers are justified or valid will depend on the social structures. For a realist, social factors and non-epistemic values may affect the context of discovery and the context of

---

<sup>12</sup> Even though in *The Advancement of Science* (p. 6) Kitcher described his central aim as “to probe the notions of progress and rationality, dear to Legend’s champions” (balancing them with the contributions of some critics), later he seemingly prefers to describe it as an attempt to understand the form in which the social aspects operate: “[I]n *The Advancement of Science*, I try to show the intricacy of the reasoning processes that figure in major scientific debates and to construct a formal framework for understanding how various kinds of social institutions, social relationships, and personal aspirations can play a positive role in the genesis of new knowledge,” (Kitcher 1998, p. 45).

application, but they cannot affect the context of justification without undermining scientific objectivity (cf. Koertge 2000).<sup>13</sup>

However, Kitcher does not seem to be prepared to give to the external influences an important role in the context of justification. He writes: “the practical demands and the history of research standards also help determine what will count as acceptable solutions, specifying, for example, the precision that an answer must achieve if it is to be applicable.” (Kitcher 1998, p. 37). Social factors play, then, a very limited role in the *acceptation* of proposals by the scientific community: they can modify the rigor of acceptance criteria, so that a hypothesis which passes these criteria in a historical context will not pass them in another context, yet they cannot themselves dictate these criteria. As already pointed out, Kitcher made clear in *AS* that social factors do not determine the closure of scientific debates. On the contrary, debates are “ultimately closed through the articulation and acceptance of decisive arguments.” Arguments should be, then, “the *principal* source of power,” so that “in a competition between the social factors and arguments leading in a contrary direction, the acquisition of power should be more affected by the arguments. (Social factors may retard a decision, but never reverse it).” (Kitcher 1993a, pp. 201-202).<sup>14</sup> Nothing said in *STD* denies these claims.

But let us begin at the beginning. Of course, Kitcher’s modest realism, as it is displayed in *STD*, agrees with the realism offered in *AS* on the postulation of a mind-independent reality which can suitably be known by human beings, as well as on the acceptance of the correspondence theory of truth. Kitcher coherently refuses the Kantian-constructivist thesis that the world is something we construct by means of our conceptual or linguistic frameworks. What we construct are our representations, not the world. We have direct access to a mind-independent reality able to be known; although Kitcher admits —without making clear the apparent conflict which arises— that the constructivist may be right when he claims this access is always mediated by our concepts. For that very reason, modest realism must deny the distinction between phenomenon and noumenon. It is not that we know the noumenon through the phenomenon, or that we know the

---

<sup>13</sup> A realist may admit that social factors affect not only the research agenda but also “the contents of the beliefs of scientists,” (Niiniluoto 1999, p. 268, note); but then it is a question of the content of the *beliefs* of *individual* scientists. A realist could also admit that the considered hypotheses are constrained by non-epistemic values that foster biases for or against some hypotheses. What cannot be carried out by such factors, according to the realists, is to determine what contents the *scientific community* *accepts* or considers justified.

<sup>14</sup> For a criticism, see Shanahan (1997) and Solomon (1995). For the reasons mentioned, Solomon accuses Kitcher of being closer to “Legend” than he would like to admit. In her view, “scientists tend to be persuaded about a theory not when they have made a balanced consideration of all its merits and demerits, and judged it superior to all alternatives, but for more ‘sullied’ reasons such as the theory’s superior performance in their own area of research, their own collection of supportive data, peer pressure, bandwagon effects, graduate school training, etc.” (Solomon 1995, p. 215). So, any philosopher who denies this wants to preserve the “Legend”. Anyway, Solomon interprets correctly Kitcher’s position when she sees that Kitcher’s open attitude to social factors does not go as far as to dilute the “merits and demerits” of theories in “more sullied reason,” whatever that may mean.

noumenon directly; we just directly know a reality with which we interact and whose properties we are sometimes able to detect successfully (cf. Kitcher 2001a, pp. 14-15, 16 note, and 25-28; 2001b, pp. 183-184).

However, the modest realism of *STD* is also in several aspects more sophisticated than the realism introduced in *AS*. It assumes, for instance, that the world can be conceptualized in very different ways; that the truths about it can be expressed in very diverse languages; and, therefore, that there is no privileged language or conceptual scheme able to give the only correct description of the world. Just like Putnam's conceptual relativity, Kitcher's modest realism holds that "there is thus no determinate answer to the question, 'How many things are there?'" and no possibility of envisaging a complete inventory of nature." (Kitcher 2001a, p. 45; cf. 2001b, p. 196). But, this does not mean that there are no mind-independent entities. There is a mind-independent reality, but it is not a labeled reality. It is not a reality categorized and structured in a fixed form, irrespective of our interests and concepts.<sup>15</sup> Nevertheless, epistemological relativism, so popular among constructivists, is not justified either. Not every classification of the world is as valid as any other. Depending on our interests and aims, there will be more suitable languages and, then, more suitable classifications. None of them —and the relativist is right about that— could ever be considered the most suitable from an absolute point of view, i. e., in all the contexts and for any purpose. But, some classification could be considered the most suitable in some concrete context. In short, there is not a perfect language that will give us the authentic classification of true natural kinds, because any classification is always relative to some interests and aims; however, for some purposes it can be shown that certain classifications are better than others. This can be illustrated with the way in which present biology contemplates biological species. Sometimes they are distinguished according to morphological criteria (as in the case of plants with asexual reproduction), other times according to their reproductive isolation (as in the case of twin species), and still in other situations according to molecular criteria (as in the case of bacteria and viruses). The division into species depends, then, on scientists' interests in each situation, but in each context a division is preferable to others (cf. Kitcher 2001a, pp. 45-49).<sup>16</sup>

---

<sup>15</sup> Niiniluoto holds the same: "THE WORLD [that is, the mind-independent world] does not contain self-identifying individuals, but can be categorized into objects in several alternative, overlapping ways relative to conceptual schemes. For example, depending on the choice of a suitable conceptual framework, THE WORLD can be 'sliced' or 'structured' to a system of momentary events, mass points, physical systems, etc." (Niiniluoto 1999, p. 222).

<sup>16</sup> It is important to note that Kitcher's pluralism about the species concept is not a conventionalist thesis, but a realist one. Conventionalists think that the diversity of species concepts is a good reason for denying the reality of such a category. What we call 'species' is a very different thing in every case, and its distinction from other taxa is problematic (cf. Stanford 1995; Ereshefsky 1998). On the contrary, Kitcher's pluralism holds that there are different species concepts, all of them legitimate, but each concept only is properly applied to some groups of organisms, and for each group just one of these concepts is suitable. Species are real classes of individuals related by interesting biological relations. Now then, these relations are diverse and can be selected in different ways, so that there is not a single way of generating taxonomic classifications (cf. Kitcher 1984b).

Kitcher tries to dissipate the difficulties in understanding this intermediate position between metaphysical realism and relativistic constructivism with the help of a classical image in the recent literature about realism: the image of map-making.

The history of map-making —he writes— illustrates the modest realism with which I began. Consider some of the maps of our planet offered by the geographers of the past, maps of the entire globe. Later maps appear superior to earlier ones in two major respects. First, they include entities that were previously omitted, the New World and Australasia being the most striking examples. Second, their depictions of the spatial relations among the entities commonly represented are more accurate; the margins of the various countries follow actual coastlines more closely. We make these judgements without believing that *any* of the maps ever produced is completely accurate, even while admitting the possibility that earlier maps might occasionally deliver a more accurate representation of some local features, and that the kind of convergence we appreciate visually need not be monotonic. (Kitcher 2001a, p. 55).

Stephen Toulmin was one of the first authors who used the metaphor of scientific theories as maps, and curiously enough he did it to defend a kind of instrumentalism (cf. Toulmin 1953, chap. 4). More recently, it has been used by Ronald Giere to defend a more modest realism than Kitcher's version (although very similar to it in some aspects) (cf. Giere 1999).<sup>17</sup>

Maps are always partial, selective, and possess conventional elements. They can offer only a perspective of reality based on our aims when we elaborate them. Depending on such aims, some entities or others will be chosen as objects to be represented, and some reading conventions or others will be accepted. A political map will pick out aspects of reality very unlike those presented in a rainfall map, a road map, an economic map, or a geological map, and it will represent these aspects by means of diverse conventional signs. All these maps might refer to the same region; however, the representation they offer will be dissimilar. In addition, it is senseless to postulate an exhaustively complete and correct map. Not even the Borgesian map of an empire, which when unfolded, had the same extension of the actual empire, would be complete, since it would need to be selective in some ways. In other words, there is not a single correct way of making a map. But, as opposed to what Giere maintains, Kitcher thinks that maps can be described as (approximately) true or false. A map of Spain in which Madrid is nearer to Valencia than to Toledo, or in which the Guadalquivir River flows into the Bay of Biscay, would be not only an inaccurate map. It could be

---

<sup>17</sup> For instance, Giere does not accept that scientific theories are (approximately) true in the sense of the correspondence theory of truth. Giere also thinks the way in which maps represent reality may be used as an analogy to clarify the way in which theories and models in science represent it. However, in contrast to Kitcher, Giere does not see maps as true or false, but just as more or less accurate, more or less detailed, etc. Also, in contrast to Kitcher, Giere proposes, at least as a methodological rule, that we proceed in science as if the world has a single structure, which means that “the existence of conflicting applications of different types of models is an indication that one or both types of models fail to fit the world as well as they might” (Giere 1999, p. 83).

said to be a false map. Following the analogy, scientific theories are also partial and tied to conventional elements of representation which might change depending on the interests; nevertheless, even in that case, it would make sense to talk about the true or falsity of its assertions about the world:

Like maps, scientific theories and hypotheses must be true or accurate (or, at least, approximately true or roughly accurate) to be good. But there is more to goodness in both instances. Beyond the necessary condition is a requirement of significance that cannot be understood in terms of some projected ideal —completed science, a Theory of Everything, or an ideal atlas. (Kitcher 2001a, p. 61).

This quotation brings up an additional point that has great relevance in *STD*, although it was present in *AS* in an incipient way. Science does not aim to achieve the truth without more ado, but to achieve significant truths; just like maps do not represent for the sake of accuracy, but in order to be useful for some practical purposes. In science, however, significance is not a basically practical matter, as in the maps, but an epistemic one. What is important for science is to achieve epistemically significant truths. Now then, all the attempts carried out within the philosophical tradition to find a characterization of significance valid for all times and contexts have failed. The reason is that one cannot separate epistemic significance and the concrete practical concerns that guide the scientific community in given historical situations. The best proposal that can be found in the matter is, in Kitcher's view, the one that identifies epistemic significance with the capacity to offer objective explanations about nature: a significant truth would be a truth able to provide an objective explanation or an objective understanding of phenomena. Actually several aspects of explanations depend on the context, not only because the context says what things are to be explained, but also because it determines which explanations are satisfactory according to the audience. There is nothing like a 'significant-in-all-contexts-explanation'.

The contextual character of significance has important consequences in practice. There are no ideal atlases, ideal classifications of the world, or context-independent objective explanations. This implies, as we are told in the epilogue of *STD*, that "Nature is shaped by our past interests, its current configurations partially determine our present needs, and out of these needs grow our further attempts to solve problems we take to be epistemically and practically significant" (p. 199).

The following step in Kitcher's argumentation (to which the second part of the book is dedicated) seems then quite logical: if science looks for significant truths and the significance is not something that some truths possess intrinsically, but depends on the context, then we are entitled to establish what truths we want science to look for at each moment, i. e., what truths we consider more significant for our interests. The usual answer to this question has, hitherto, been



that the scientists themselves have to determine such matter. Kitcher thinks that in democratic societies this subject should not be exclusively in their hands, but neither should it be in the hands of outsiders, such as businessmen. A pure, value-free science does not exist; on the contrary, considerations about values—including non-epistemic values (moral, political, social... values)—are consubstantial to scientific practice. Moreover, truth and knowledge are not intrinsically good or beneficial things. If we take this for granted, it is hardly surprising that science is not currently well-ordered, and that it is abundant in conflicts that arise between scientific practice and the ideals of a democratic society. In a well-ordered science—to which we must aspire—the aims would be set by the interests of democratic society. Science should set significant truths, but the significance should be determined by the citizens' interests, ideally decided through the deliberation procedures of an illustrated democracy (people representative of diverse perspectives tutored by scientific experts). These interests should then set the scientific research agenda.

We will not consider here in more details Kitcher's proposal to get a well-ordered science (some critical reflections can be found in Brown (2004), Jasanoff (2004), and Mirowski (2004)). This is obviously a very difficult issue. Some authors have even questioned the desirability of its accomplishment. (Does a well-ordered science not run the risk of being a politicized science in the worst sense of the word? Is it not worryingly close to Bruno Latour's thesis that science is politics done by other means?). Nonetheless, in my opinion, this is an inescapable issue in view of the real situation of science in advanced societies and its increasingly complex relationship with the public. Kitcher has merit for having remarked on its importance for the philosophy of science and not only for the sociology of science (not to forget, however, antecedents such as John Dewey and Paul Feyerabend, to mention only the best known). In the following pages I would rather concentrate on some ontological aspects of modest realism that Kitcher draws in *STD* which I find particularly problematic.

### **3. The Hardship of Modesty**

Kitcher's modest realism is a sophisticated position which avoids many objections raised in the past decades to strong realism or to metaphysical realism in Putnam's sense. Nevertheless, modest realism has some troubles from the concessions it makes to antirealism. I will expose one which I consider particularly important.

As explained above, Kitcher tries to harmonize the correspondence theory of truth with the conceptual relativity defended for a good while by Putnam. It is debatable, however, whether both things are easily compatible. If our knowledge of the world is mediated by our categories and

concepts, if the choice of these categories and concepts varies in accordance with our interests — and it does not depend on the existence of natural kinds or an intrinsic structure of the world—, then it becomes problematic to establish what our true sentences correspond to. Do they correspond to a mind-independent world (a world devoid of an intrinsic ontological structure, in the event that we strictly assume conceptual relativity), or to a world structured by our categories and concepts?

The first alternative makes no sense, as Putnam indicated. There cannot be a correspondence between our sentences and a wholly unstructured reality.<sup>18</sup> So, we have to choose the second option. When we say that a statement is true, that means that there is a correspondence between this statement and the world as it is offered to us shaped by our languages or conceptual schemes. But, if we interpret Kitcher in this way, his modest realism has to face then the same problem as Kantian transcendental idealism in order to define the concept of truth. For transcendental idealism, our true statements cannot correspond to a noumenal world, but to a phenomenal one, and, therefore, to a world constructed by us. In spite of the lack of agreement by authors who have considered the issue, there does not appear to be a special impediment to understand the correspondence theory of truth in a non-realist form. This theory claims that a sentence is true if and only if what the sentence affirms corresponds to reality, but the theory leaves open how to conceive the reality. It might be understood as a mind-independent reality or as a reality constructed, or at least structured, by our mind. Not only Kant, but also by the early 20<sup>th</sup> century British philosopher J. M. E. McTaggart understood it in this last way. However, this alternative meet an objection. May we talk in such a case about ‘correspondence’? Would not it rather be a matter of mere coherence between different representations of the world: the coherence between the part of a world-version offered by a statement and the world-version constructed in some concrete act of experience? So it would be—in my opinion— if the world had no capacity for constriction about the versions or structures that we might build in our mind. A correspondence between our ideas and wholly mind-dependent facts would not be but a mere coherence between ideas. The key is, then, whether the mind-independent world has something to say or not.<sup>19</sup>

Certainly Kitcher—just like Kant, but more coherently<sup>20</sup>—resorts to a causal (and pre-conceptual) relation with the world, and, since this causal relation enables us to accede to the

---

<sup>18</sup> Nevertheless, for a suggestive exploration of this possibility, see Horgan and Potrč (2000).

<sup>19</sup> Not to perceive clearly that is, in my opinion, the main deficiency in Michael P. Lynch’s attempt to make the conceptual relativity compatible with the correspondence theory of truth. Lynch does not provide any explanation about how it is possible that the correspondence with the world makes our statements true if, on the other hand, there is no mind-independent fact (cf. Lynch 1998).

<sup>20</sup> As it was remarked by his early critics, Kant cannot coherently resort to a causal relation between the noumenal world and our cognitive faculties, since, according to his philosophy, the category of causality can only be applied to the phenomena, i.e., to the empirical realm. The complex problems presented by the notion of truth within Kantian philosophy can be profitably read in Palacios (1979). See also Putnam (1981), pp. 60-64.

world, he refuses the distinction phenomenon/noumenon. But, I am inclined to think that this viewpoint does not solve the difficulty. We could say that true statements correspond to a conceptually structured reality, with this reality being the result of applying our concepts and categories to an independent and unstructured world causally related to us. So, our true statements would be causally selected by the world itself. This causal interaction, however, would not be enough to establish a correspondence between our statements and an unstructured “something,” among other reasons, because that correspondence is not a causal relation (we can talk about a correspondence between a sentence and some future fact, which obviously is not causally related to us).

Niiniluoto discusses in some detail this question that also arises in his proposal of a critical scientific realism (cf. Niiniluoto 1999, pp. 224-226).<sup>21</sup> In order to solve it, he distinguishes between *unidentified facts* and *identified facts*. Unidentified facts are the facts contained in the mind-independent world, whereas identified facts are the facts belonging to a structured world-version. According to Niiniluoto, if a sentence is true in a world-version —if it is true about some identified facts—, then it is true in the mind-independent world as well —it is true about some unidentified facts. The reason is that every world-version is only determined by a chosen language and the mind-independent world, and after all “THE [mind-independent] WORLD does not change in any way when we give descriptions of it.” (Niiniluoto 1999, p. 219). The identified facts belong to the same world as the unidentified ones. They are not an intermediate between us and the world. They are just the unidentified facts as described relative to some language.<sup>22</sup>

Nevertheless, I think this recourse is really an implicit admission that the world has an intrinsic structure, although it is one in which facts —events or “states of affair” that might happen according to such structure— are not yet identified through human categories or concepts, and when identified, it may occur in several alternative forms. If not, why are not all descriptions possible? In fact, Niiniluoto admits that the world has a very basic intrinsic spatio-temporal structure and obeys natural causal laws. This structure does not come from our languages and conceptual schemes.<sup>23</sup>

---

<sup>21</sup> He is completely convinced that the problem can be solved: “Semantical realism is compatible with ontological pluralism: the non-epistemic correspondence theory of truth can be combined with the idea that objects can be individuated and identified in alternative ways through different conceptual systems,” (Niiniluoto 1999, p. 205).

<sup>22</sup> Niiniluoto also distinguishes between unidentified objects and identified objects (literarily *unidentified flying objects* (UFOs) and *identified flying objects* (IFOs)). Since UFOs form a part of THE WORLD, Niiniluoto finally admits that in some sense of the term ‘object’ (i. e., not self-identifying objects, “slices” of THE WORLD potentially identifiable in several ways) there are objects independent of our conceptual schemes (cf. Niiniluoto 1999, pp. 224-226).

<sup>23</sup> Cf. *Ibidem*. Niiniluoto also postulates a basic ontology consisting of mind-independent “property-individuals” located in space and time, such as the-brownness-of-this-table. These individualized qualities have been called ‘tropes’. The property of being brown would be the class of similar tropes, and, as a class, is a human construction. The objects would be the mereological sum of jointly occurring tropes, which can be selected in different forms (cf.

Neither for Kitcher nor for Niiniluoto is the mind-independent world completely malleable by means of our conceptual frameworks. We cannot impose on the world any arbitrary structure and hope that this imposition will always work in practice. Niiniluoto calls ‘factuality’ the resistance the world offers to the attempts to fit it into any conceptual scheme. But, if the idea that the world has a previous structure is rejected, it remains to be seen whether it can be satisfactorily explained why the world accepts some classifications and resists others. Maps are more or less accurate, more or less true. A map of the London Underground would not be of use for guiding us through the Paris Underground. In fact, all possible maps of the London Underground have to keep some structural similitude if they are to be effective: not every structure of lines and stations, of crossing and changes, will be valid. Kitcher claims: “Users of different schemes of representation may find it difficult to coordinate their languages. Properly understood, however, the truths they enunciate are completely consistent.” (Kitcher 2001a, p. 47). Accepting that such is the case, what could guarantee this consistency except the fact that these languages refer to the same reality, a reality with an intrinsic structure that does not depend on them?

It is interesting to know that, according to some psychological experiments, a visual stimulus wholly devoid of structure cannot be perceived. The existence of structured stimuli is needed in order to have a visual perception. This order must come from a somewhat structured reality, not from our mind; otherwise the experimental results could not be explained. So, an unstructured and homogenous noumenal dough would be simply imperceptible. This is not incompatible with the well-known fact that our perception, being selective itself, processes inputs which sensorial stimuli provide. The point is just that the structure of the world responsible for structuring the sensorial stimuli is not necessarily the structure we finally attribute to the world. Our mind has an active role, a constructive role if we prefer the expression. It elaborates the brute material received through the senses in more or less complex forms. But this construction is not built on the noumenon, but on a world with some ontological structure of its own (cf. Boulter 2004).

I think some light can be thrown upon this issue if we distinguish three forms of understanding ontological realism, that is to say, three alternative ways of specifying how a mind-independent world is to be conceived. I will call them ‘Platonic-Aristotelian Ontological Realism’ (PAOR), ‘Moderate Ontological Realism’ (MOR), and ‘Kantian Ontological Realism’ (KOR). This is, respectively, what they affirm:

---

Niiniluoto 1999, p. 30). It is not clear to me how to conceive the ‘tropes’ except as a certain type of intrinsic structure of the world. The sort of ontological realism defended by Niiniluoto and its differences from Putnam’s (and Quine’s) position can be seen in the lucid and detailed discussion in Pihlström (1996), cap. 4.

(PAOR): The world has an ontologically complete and categorically fixed structure (it is a ‘ready-made’ and labeled world consisting of self-identifying objects). This structure dictates exactly one true and complete description of the world.

(MOR): The world has a basic intrinsic structure compatible with several (and, perhaps, potentially infinite) impositions and classifications made by our conceptual schemes or linguistic frameworks. So, it tolerates many different versions, but all of them within limits defined by this structure. It is an unfinished structure and also inexhaustible from the point of view of human interests and capacities. It is not categorically ready-made and it does not dictate just one correct description.

(KOR): The world possesses no intrinsic structure. Any structure in any world-representation comes from our conceptual schemes, categories or languages.

(PAOR) is ingenuous, and hard to maintain given the pragmatists’ criticisms (especially Quine’s and Putnam’s criticisms), let alone the results of cognitive sciences showing the active role of mind—an embodied and largely social mind—in the knowledge process, and the creative character of our conceptualizing abilities (cf. Lakoff 1987; Martínez-Freire 1998). (KOR) has been Putnam’s position for some years and it appears it is sometimes defended by Kitcher. But, as I have argued, it is too weak to sustain a realism committed to correspondence theory of truth; and in this sense, Putnam is coherent when he gives up such theory in favor of a pragmatist conception of truth. (KOR) is not able satisfactorily to explain why the world is not completely malleable; why it resists some of the things we might want to make with it by means of our languages and conceptual schemes. Finally, from an evolutionary point of view (KOR) is less plausible than its alternatives: if every world-structure comes only from our mind, it would be difficult to explain the adaptive value of our cognitive capabilities, since the world would have no role in the success of these abilities in managing the environment. Consequently, I hold that Kitcher’s modest realism, like any other realism that refuses (PAOR), should accept (MOR).

(MOR) is not affected by many criticisms made against the dogmatism and the foundationalism of (PAOR), but it is still strong enough to explain why certain divisions and classifications fit into the world. We can offer only some world-versions, knowing that we cannot exhaust with them all the aspects of the world or to find the “Right Version.” But, given the basic ontological structure of the world, not all versions are possible or equally suitable. There are versions that fit easily into such structure, whereas other versions need to be forced to fit into it, and finally other versions *velis nolis* do not fit at all. In the words of Umberto Eco, it happens to the world just “like to the ox or the calf: in different civilizations they are carved up in different forms, so that the name of some dishes is not easily translatable from a language into other.

However, it would be very difficult to conceive a carving-up that offers together the extremity of snout and the tail.” (Eco 1997, p. 63). The ox and the calf do not dictate what carving-up must be done; their ontological nature is “pre-butcherish,” so to speak, and it tolerates different possibilities. Nevertheless, both have a structure that determines that no carving-up can include the snout and the tail in the same piece.<sup>24</sup>

#### 4. Conclusions

Kitcher’s modest realism, as displayed in *Science, Truth and Democracy*, is an attempt to integrate, from a moderate realist point of view, concerns about the democratic control of science. In preceding decades, these concerns were often linked to antirealist and even openly relativistic positions. Kitcher holds the need for a well-ordered science, to wit, a science whose aims are set by the interests of democratic societies established by means of informed (scientifically tutored) processes of deliberation. Nevertheless, in line with his realism, Kitcher does not admit that social interests can affect the epistemic validity of scientific results. On this point, his approach is opposed to that of social constructivism. Democratic deliberation affects the research agenda—the sort of problems considered significant—the resources to be assigned, the strategies seen as more appropriate, and the transfer of results to concrete applications. Not surprisingly, some supporters of social constructivism have considered—wrongly in my opinion—Kitcher’s proposal as a modality of what himself called “The Legend” in the philosophy of science.

However, from an ontological and semantic point of view, Kitcher’s modest realism has some internal difficulties. It is not clear whether Kitcher accepts a moderate ontological realism, for which the world has a basic intrinsic structure compatible with several conceptual or linguistic impositions or divisions, or he accepts, instead, a Kantian ontological realism, for which the world lacks an intrinsic structure and any structure comes from our conceptual schemes, categories or languages. We have argued that it is possible to sustain a correspondence theory of truth—which

---

<sup>24</sup> An author that, in my view, holds a position close to (MOR) is Alvin Goldman. That is quite manifest in the following quotation: “There are indefinitely many sorts of apparel that might be designed for the human body, just as there are indefinitely many categories, principles of classification, and propositional forms that might be used to describe the world. Although the body certainly has parts, it is not presorted into units that must each be covered by a distinct garment. It is up to human custom and sartorial inventiveness to decide not only what parts to cover, but what types of garments should cover which expanses of the body, and whether those garments should be snug or loose,” (Goldman 1986, p. 152). I also consider close to (MOR) the realistic contextualism exposed by Nicholas Rescher in *Nature and Understanding* (2000).

On the other hand, Niiniluoto’s distinction between UFOs and IFOs (see note above) implicitly situates him close to (MOR), since this distinction seems to imply that there are structures dependent on our conceptual schemes or languages (L-structures), but there is also certain ontic order. At that rate, the world would not be ready-made, that is, it does not impose on us a predetermined conceptual or linguistic order, but even so the UFOs have some own order. If things are understood in this way, Putnam’s claim that the mind and the world jointly make up the mind and the world could acquire a more precise sense.

is the theory of truth embraced by Kitcher— from the first kind of ontological realism, but not from the second. Therefore, modest realism makes sense only if it includes moderate ontological realism.<sup>25</sup>

*Universidad de Málaga*  
 Departamento de Filosofía  
 Campus de Teatinos  
 29071 Málaga (Spain)  
 diéguez@uma.es

## REFERENCES

- Boulter, S. J. (2004). Metaphysical Realism as a Pre-condition of Visual Perception. *Biology and Philosophy* **19**, 243-261.
- Brown, J. R. (2004). Review of *Science, Truth and Democracy* by Philip Kitcher. *The Journal of Philosophy* **11**, 599-606.
- Callebaut, W. (1993). *Taking the Naturalistic Turn*. Chicago: The University of Chicago Press.
- Devitt, M. (1984). *Realism and Truth*. Princeton: Princeton University Press.
- Diéguez, A. (1998). *Realismo científico. Una introducción al debate actual en la Filosofía de la Ciencia*. Málaga: SPICUM.
- Eco, U. (1997). *Kant y el ornitorrinco*. Barcelona: Lumen.
- Ereshfsky, M. (1998). Species Pluralism and Anti-Realism. *Philosophy of Science* **65**, 103-120.
- Fine, A. (1986). *The Shaky Game. Einstein Realism and the Quantum Theory*. Chicago: The University of Chicago Press.
- Giere, R. (1999). *Science without Laws*. Chicago: The University of Chicago Press.
- Goldman, A. I. (1986). *Epistemology and Cognition*. Cambridge, MA: Harvard University Press.
- González, W. J. (2006). Novelty and Continuity in Philosophy and Methodology of Science. In: W. J. González and J. Alcolea (eds.), *Contemporary Perspectives in Philosophy and Methodology of Science*, pp. 1-28. A Coruña: Netbiblo.
- Hacking, I. (1983). *Representing and Intervening*. Cambridge, MA: Cambridge University Press.
- Horgan, T. and M. Potrč (2000). Blobjetivism and Indirect Correspondence. *Facta Philosophica* **2**, 149-157.

---

<sup>25</sup> I am grateful to Antonio Regalado and Elizabeth Kline for his help in improving this paper.

- Jasanoff, S. (2004). What Inquiring Minds Should Want to Know. *Studies in History and Philosophy of Science* **35**, 149-157.
- Kitcher, Ph. (1981). Explanatory Unification. *Philosophy of Science* **48**, 507-531. (Reprinted in R. Boyd, Ph. Gasper, and J. D. Trout (eds.), (1991). *The Philosophy of Science*, pp. 329-347. Cambridge, MA: The MIT Press).
- Kitcher, Ph. (1982a). *Abusing Science: The Case Against Creationism*. Cambridge, MA: The MIT Press.
- Kitcher, Ph. (1982b). Genes. *British Journal for the Philosophy of Science* **33**, 337-359.
- Kitcher, Ph. (1983). *The Nature of Mathematical Knowledge*. New York: Oxford University Press.
- Kitcher, Ph. (1984a). 1953 and All That: A Tale for Two Sciences. *The Philosophical Review* **93**, 335-373. (Reprinted in Kitcher 2003).
- Kitcher, Ph. (1984b). Species. *Philosophy of Science* **51**, 308-333. (Reprinted in Kitcher 2003).
- Kitcher, Ph. (1985). *Vaulting Ambition: Sociobiology and the Quest for Human Nature*. Cambridge, MA: The MIT Press.
- Kitcher, Ph. (1989a). Explanatory Unification and the Causal Structure of the World. In: Ph. Kitcher and W. C. Salmon, (eds.), *Scientific Explanation*, pp. 410-505, Minneapolis: University of Minnesota Press.
- Kitcher, Ph. (1989b). Some Puzzles about Species. In: M. Ruse (ed.), *What the Philosophy of Biology Is: Essays for David Hull*, pp. 183-208. Dordrecht: Reidel. (Reprinted in Kitcher 2003).
- Kitcher, Ph. (1993a). *The Advancement of Science. Science Without Legend, Objectivity Without Illusions*. New York: Oxford University Press.
- Kitcher, Ph. (1993b). Knowledge, Society, and History. *Canadian Journal of Philosophy* **23** (2), 155-178.
- Kitcher, Ph. (1997a). An Argument about Free Inquiry. *Noûs* **31** (3), 279-306.
- Kitcher, Ph. (1997b). *The Lives to Come. The Genetic Revolution and Human Possibilities*, New York: Simon and Schuster.
- Kitcher, Ph. (1998). A Plea for Science Studies. In N. Koertge (ed.), *A House Built on Sand. Exposing Postmodernist Myths about Science*, pp. 32-56. New York: Oxford University Press.
- Kitcher, Ph. (2001a). *Science, Truth, and Democracy*. New York: Oxford University Press.
- Kitcher, Ph. (2001b). Real Realism: The Galilean Strategy. *Philosophical Review* **110**, 151-197.
- Kitcher, Ph. (2002a). On the Explanatory Role of Correspondence Truth. *Philosophy and Phenomenological Research* **64**, 346-364.



- Kitcher, Ph. (2002b). Reply to Helen Longino. *Philosophy of Science* **69**, 569-572.
- Kitcher, Ph. (2003). *In Mendel's Mirror. Philosophical Reflections on Biology*. New York: Oxford University Press.
- Kitcher, Ph. (2007). *Living with Darwin*. Oxford: Oxford University Press.
- Kitcher, Ph. and Flory, J. H. (2004). Global Health and the Scientific Research Agenda. *Philosophy and Public Affairs* **32** (1), 36-65.
- Kitcher, Ph. and K. Sterelny (1988). The Return of the Gene. *Journal of Philosophy* **85**, 335-358. (Reprinted in Kitcher 2003).
- Koertge, N. (2000). Science, Values, and the Value of Science. *Philosophy of Science*, supplement **67** (3), S45-S57.
- Lakoff, G. (1987). *Women, Fire, and Dangerous Things*. Chicago: The University of Chicago Press.
- Laudan, L. (1981). A Confutation of Convergent Realism. *Philosophy of Science* **48**, 19-48. (Reprinted in D. Papineau (ed.), (1996). *The Philosophy of Science*, pp. 107-138, Oxford: Oxford University Press).
- Levin, M. (1984). What Kind of Explication is Truth. In: D. Papineau (ed.), *Scientific Realism*, pp. 124-139. Berkeley: University of California Press.
- Lynch, M. P. (1998). *Truth in Context. An Essay on Pluralism and Objectivity*. Cambridge, MA: The MIT Press.
- Martínez-Freire, P. F. (1998). Epistemología con sujetos cognitivos. In: P. F. Martínez-Freire (ed.), *Filosofía actual de la Ciencia*, pp. 95-118. Málaga: SPICUM.
- McLeish, C. (2005). Scientific Realism Bit by Bit: Part I. Kitcher on Reference. *Studies in History and Philosophy of Science* **36**, 667-685.
- Mirowski, Ph. (2004). The Scientific Dimensions of Social Knowledge and their Distant Echoes in 20<sup>th</sup>-Century American Philosophy of Science. *Studies in History and Philosophy of Science* **35**, 283-326.
- Niiniluoto, I. (1984). *Is Science progressive?* Dordrecht: Reidel.
- Niiniluoto, I. (1999). *Critical Scientific Realism*. Oxford: Oxford University Press.
- Palacios, J. M. (1979). *El idealismo trascendental: Teoría de la verdad*. Madrid: Gredos.
- Pickering, A. (1995). *The Mangle of Practice*. Chicago: The University of Chicago Press.
- Pihlstrom, S. (1996). *Structuring the World. The Issue of Realism and the Nature of Ontological Problems in Classical and Contemporary Pragmatism*, *Acta Philosophica Fennica* **59**.
- Psillos, S. (1999). *Scientific Realism. How Science Tracks Truth*. London: Routledge.
- Putnam, H. (1978). *Meaning and the Moral Sciences*. London: Routledge & Kegan Paul.

- Putnam, H. (1981). *Reason, Truth and History*. Cambridge, MA: Cambridge University Press.
- Putnam, H. (1999). *The Threefold Cord*. New York: Columbia University Press.
- Rescher, N. (2000). *Nature and Understanding*. Oxford: Clarendon Press.
- Shanahan, T. (1997). Kitcher Compromise: A Critical Examination of the Compromise Model of Scientific Closure, and its Implications for the Relationship between History and Philosophy of Science. *Studies in History and Philosophy of Science* **28** (2), 319-338.
- Solomon, M. (1995). Legend Naturalism and Scientific Progress: An Essay on Philip Kitcher's *The Advancement of Science*. *Studies in History and Philosophy of Science* **26** (2), 205-218.
- Stanford, P. K. (1995). For Pluralism and Against Realism about Species. *Philosophy of Science* **62**, 70-91.
- Toulmin, S. (1953). *The Philosophy of Science*. London: Hutchinson.
- Van Fraassen, B. (1980). *The Scientific Image*. Oxford: Clarendon Press.
- Van Fraassen, B. (1989). *Laws and Symmetry*. Oxford: Clarendon Press.
- Zamora Bonilla, J. P. (2000). El naturalismo científico de Ronald Giere y Philip Kitcher. Un ensayo de comparación crítica. *Revista de Filosofía* **24**, 169-190.