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In a preliminary investigation of the complex and uneven territory covered by the relationship between science and values, it is important to distinguish different ways in which values can enter into the natural and social sciences. Very often these differences have been conflated, to the detriment of our understanding of the geography of the territory.

I contend that science's value dependence has at least *four* different forms, which give us four different roles that values play in the scientific endeavor: (1) values functioning as selectors among different fields of investigation; (2) values functioning as selectors among alternative, empirically equivalent theories or hypotheses (these are often referred to as *epistemic values*); (3) values functioning as "regulative" ideas of science, that is, as indicators of the place and meaning that the scientific enterprise as a whole should have in our culture, in society, and in our life in general; (4) values functioning as guides to the application of our scientific knowledge and technology to practical decision making. Under the fourth role I classify cases in which the piece of knowledge to be applied is highly reliable as well as cases in which our knowledge is instead partial and limited, generating nonnegligible "inductive risks" in the sense introduced by Carl Hempel (1965) and further developed by Heather Douglas (2000).

By reviewing what I regard as the main problems in each of these different types of science's value dependence, I offer a *general survey of the territory while stressing the importance of keeping epistemic and nonepistemic values separate*. The cases in which they cannot be easily separated are those in which our knowledge is quite unreliable. My use of the word *importance* implicates me as an advocate of separating epistemic values from nonepistemic ones, a position I intend to defend

against current attacks. By *epistemic values* I am referring not just to the evidence-hypothesis relationship or to values that are conducive to *truth*[1] but more generally to those aims that are usually regarded as capable of furthering our knowledge, like "understanding" or "explaining," to the extent that these two are independent of each other. I use *nonepistemic* to refer essentially to values that are ideological, economical, or political (like feminism, sexism, Marxism, fascism, capitalism, and racism), or ethical, environmental, esthetical, or religious.

Sometimes such nonepistemic values are grouped under the misleading term *social values.* Here, however, I will simply take for granted that all the typically epistemic values I will discuss (consistency, experimental accuracy, explanatory power, and so on) are *socially shared*, because they are an essential part of the training of scientists.[2] By making this assumption, it seems to me that bringing into the discussion the adjective *social* and contrasting it with *epistemic* can only cause confusion and misunderstandings. The interesting contrast is not between the epistemic and the social but rather between the epistemic and the ideological, as already clarified. Inquiring into the contrast between the epistemic and the ideological entails raising the question whether scientists' acceptance of a hypothesis (its warrant) depends on their allegiance to values furthering our knowledge or instead to values pertaining to their political or ideological agendas. Whether these values can and ought to be separated is the object of my investigation here.

I will say little on the first type of dependence since, as we shall see, it is highly noncontroversial and universally recognized: political, military, economic, and, less grandiosely, purely personal and idiosyncratic interests or values clearly influence the choice of scientific problems and facts to be investigated. Though in this sense science is obviously value-laden, such a form of value dependence is not equivalent to claiming that the *cognitive content* of science is laden with nonepistemic values (section 1). As to the second type of dependence, I will argue that nonepistemic values typically do not have (a descriptive claim), and ought not to have (a prescriptive one), any role in the choice among empirically equivalent theories. Nonepistemic values can at most motivate the *pursuit* of a theory, but they *should* never be regarded as a *justification* for choosing one among a class of empirically equivalent theories. The “should” of the previous sentence clearly shows that the

claim it expresses is a *normative* one; however, I would also like to contend that the norm in question is *de facto* accepted by most good scientists. //**Au: is this what you mean?**//

In addition, a widespread misunderstanding of the relationship between theory and evidence has greatly exaggerated the threat posed to the epistemic claims of science by the alledged existence, in any historical situation, of mutually incompatible but empirically equivalent theories. //**Au: I'm not sure whether preceding revision is what you mean. Original sentence was unclear, primarily because of its passive construction. Please review and revise as necessary**// Curiously enough, scientists often hold that, rather than having to constantly and arbitrarily choose between different but empirically equivalent theories, as some philosophers would have them do, they do not even have *one* theory compatible with all known data! Given the clash of these views, something is clearly amiss here (section 2).

The third type of dependence would seem to be the ideal home for the influence of nonepistemic values on science; for instance, whether science ought to aim at truth and true explanations or merely at empirical adequacy or whether its knowledge claims exhaust all we can know about the world seem to be questions imbued with nonepistemic values. On closer look, however, we see that the *evidential strength* or plausibility of, say, the various realist or antirealist scientific and/or philosophical *arguments* do not depend on one's allegiance to such values, so the arguments themselves can and ought to be regarded as free from nonepistemic values. The general argument I will rely on in this context is that the causal origin of *any* argument--the reason why anybody would want it to be valid--has nothing to do with its validity (section 3).

As to the last type of dependence, I will discuss Douglas's (2000) recent analysis, according to which the nonepistemic values involved in inductive risks do not merely belong to the problem of applying our (more or less uncertain) knowledge to action but concern as well the epistemic import of the hypothesis at stake (section 4). Finally, I will also show how these four different ways science depends on values, despite their logical independence, intersect more than once.

In a word, my main thesis is that cognitive values are the only values that *ought* to belong to the *justification* of a scientific theory, or, differently put, to the relationship between evidence and

theory. In all cases in which scientists are nevertheless forced by insufficiently reliable knowledge to rely on nonepistemic values to formulate empirical hypotheses, I will show that the resulting claims remain wholly objective (that is, intersubjectively valid).

Since my main claim--that science *is*, in its best examples, free from nonepistemic values and *ought* to remain this way--reflects in part an evaluative attitude toward science, those who disagree with me partially disagree on what the general aims of science should be. Consequently, the dispute about the independence of science's cognitive claims on noncognitive values belongs to the *third* section of this chapter (the third type of dependence in my classification). Being a *normative* thesis about the general aims of science, such an independence thesis cannot be attacked (or defended) *solely* by invoking *empirical* data coming from the history or the sociology of science.[3] Of course, I could be accused of defending too idealized an image of science, and I hope to respond to this criticism in the last section of the chapter.[4]

<H1>1. Values as Selectors of Problems: The Choice of Facts

<FFL>Granting the obvious point that values in general (what we care about) have, among other things, a *selective function*--that is, they help us to choose among different alternatives or possible states of affairs--in the *first* of the four senses mentioned above, individual or social values of all sorts help us to select what we should study, what is worth our scientific investigations, or what, in a broad sense of the words, *looks interesting and important for us*. We are not merely seeking truth *simpliciter*, or even the *whole* truth; what we are after is *significant, interesting truths*: in most cases, we do not care about finding the number of grains of sand on a given time of the day on the shores of the planet Earth, even though there certainly is fact about it.

<TX>In this first type of science's value dependence, a particular value, that is, a particular *interest* in a certain area of science rather than in another, can "drive" a particular scientist (or groups of scientists) to select that area as his or her own field of expertise. For instance, a young researcher may think it much more promising to devote herself to solid-state physics rather than to particle physics, because there may be much more money in the former field than in the latter (economic, nonepistemic interest), and because teachers operating in solid-state physics may be much more

prestigious and fun to work with than those operating in particle physics. In my schematic distinction, all these would count as nonepistemic interests. The young researcher's career may therefore be better fostered in one field than in the other, and her choice might reflect this.

However, within this kind of dependence of science on nonepistemic values psychological preferences like ambition, desire for social recognition, and expectation of gain, typically enter science from "outside" or in an *external* way, namely *by simply stimulating or encouraging the pursuit and development of certain areas of inquiry rather than others.* //**Au: something is awry in preceding; sentence has no grammatical subject -- that is, *what* typically enter science? Perhaps I'm misreading? Please advise//**

Interestingly, the role of nonepistemic values as selectors of interest-relative facts has been first realized by the methodologists of the social sciences. It is this type of value-ladenness of science that Max Weber referred to when he stressed the role cultural values play in making us adopt a particular "perspective" from which to inquire into a certain area of research (say, the Reformation). He called this phenomenon *Wertbeziehung* (reference to values): we are free to adopt an *economic, moral,* or *religious* point of view or any other relevant perspective in order to illuminate the historical phenomenon of the Reformation, and each of these value-laden viewpoints functions as a selector of *some* facts as causally relevant factors.

What is essential is that this form of science's dependence on nonepistemic values--which is undeniable and omnipresent and does not distinguish the human sciences from the natural sciences, despite the fact that the role of nonepistemic values might seem more prominent in the former than in the latter sciences--is clearly *not* sufficient, by itself, to deprive the social or the natural sciences of their value-free character from a *cognitive* point of view. For instance, once we try to explain the causal relevance of economic factors in the historical processes leading tothe Reformation, by using *economics* as a selective principle, the causal link we thereby establish is (or is not) valid, according to the evidence it has, independently of our particular economic (or religious) convictions (for Weber's famous *Wertfreiheit* as distinguished from the *Wertbeziehung*, see Weber 1904).[5] //**Au: OK to move this parenthetical to the footnote?// ok, put in a footnote**

Likewise, military interests in projectiles ballistics may have encouraged the birth and growth of modern dynamics //**Au: dynamics? missile dynamics? Pls clarify**// at the time of Galileo, but the epistemic warrant of the theory enabling us to calculate the trajectory of projectiles (Newtonian mechanics) does not depend on the different military and political interests. These days, economic, political, and military lobbies do certainly stimulate scientific research by investing large amounts of money in some fields of inquiry while neglecting others. However, the achieved "causal knowledge" (say, the predicted effects of certain chemicals on human beings) does *not* vary according to our nonepistemic preferences and *is* as such *free from nonepistemic values*. We can use our causal knowledge to achieve certain (often morally objectionable) aims; now, if the validity of such knowledge depended on our different nonepistemic values, people with different such values might end up achieving different results by using the same means, and this is obviously not the case.

Consequently, without further arguments at least, we cannot assume that the *cognitive* claims of an empirical, scientific discipline are not objective (that is, intersubjectively valid) simply because, in order to select what is interesting for us, we must restrict our attention to a particular class of phenomena by using potentially nonobjective nonepistemic values.

Insisting on the massive role played by nonepistemic values in the context of discovery enables us to let the possibly *subjective* dimension of nonepistemic values enter the empirical sciences: such a "subjective" dimension of science is, at least in this first context, something to be *encouraged*. It is here that *a plurality of values may favor the growth of science*: in this context, "letting thousands of different flowers bloom" is the right attitude to take. In fact, in both the natural and the historical sciences it is only by looking at the same entity or event from a variety of different perspectives--which presuppose, in turn, many different interests or values--that we can more thoroughly understand the entity or event in question. Just as, following Weber, we should try to understand a historical event from as many different relevant viewpoints as possible--military, economical, political, anthropological, and sociological--a human being can be studied from the biophysical, biochemical, physiological, neurophysiological, psychological, sociological, historical,

and gender "perspectives," all of which contribute to a more complete understanding of our multifaceted nature.

In a word, values used as selectors of problems can play their role without necessarily jeopardizing the objectivity of the cognitive claims of science, *provided*, of course, that science *can* and ought to be regarded as an intersubjectively valid enterprise--that is, provided that we can claim that whenever nonepistemic values are the essential factor determining the acceptance of a hypothesis, we are facing an instance of *poor* science or of an *unreliable* piece of knowledge.[6] But since intersubjectivity is one of the epistemic and social values that science is said to promote, I now turn to the second mode in which science and values relate to each other.

<H1>2. Values as Selectors of Empirically Equivalent Theories

<FFL>The second, distinct sense in which (both nonepistemic and epistemic) values could be relevant for science involves the process of comparing and evaluating two rival theories. Epistemic, knowledge-serving values like accuracy, consistency, scope, simplicity, and fruitfulness (see Kuhn 1977, 321<N>22) have often been regarded as crucial in evaluating two rival theories during a scientific revolution or, more generally, during theory change.

<TX>There are two different formulations of the thesis of the empirical equivalence of theories by data. In some overly skeptical renderings of the epistemic power of scientific theories, one could argue that *for any theory T* that has ever been proposed in the history of science, there are (known or unknown) alternative theories *T'* that are *empirically equivalent* to *T*.[7] While *T* and *T'* are equivalent because they *entail* the same body of evidence, they are incompatible with each other because, for instance, their unobservable substructures explain the observable phenomena by postulating different and incompatible theoretical entities. A less radical but historically more plausible rendering of this thesis claims that *at some moments* in the history of science there are *some* interesting cases of empirically equivalent theories (for this distinction, see Psillos 1999, 167).

Leaving aside the provocative but too often neglected hypothesis that by depending "on an impoverished picture of the ways in which evidence can bear on theories" even the weak form of empirical equivalence of theories might be a figment of armchair philosophers' imaginations, [8] let us

assume for the sake of argument that the empirical underdetermination of theories by data (in the less radical sense) is a genuine problem of the methodology of science. On this hypothesis, how should we choose among empirically equivalent theories?

The main problem to be raised in this context is whether purely epistemic values, like those mentioned earlier, are really *sufficient* to resolve cases of underdetermination of theories by the empirical data, so that other, nonepistemic values can always be left *outside* the domain of science (externality versus internality pictures of science's value dependence). As a matter of fact, one of the strongest arguments in favor of science's nonneutrality toward ideological or political values depends not just on the highly controversial thesis of the "empirical equivalence of alternative theories" but also on the observation, already made by Thomas Kuhn, that his five epistemic values prove "*ambiguous* in application, both individually and collectively," in such a way that they do not determine "a *shared* algorithm of choice" (Kuhn 1977, 331).

To be charitable toward the advocates of a role for nonepistemic values in theory choice, let us suppose that there have been real historical cases in which scientists had to decide between two rival theories, *T* and *T'*, that at the time were regarded by the experts as being *empirically* equivalent. **//Au: is this what you mean? Or do you mean "...that experts now regard as being empirically equivalent at that stage in the development of our knowledge"?no, what you wrote is what I meant//** We could suppose that we could face such a situation even in contemporary science. It is in circumstances like these that epistemic values enter the scene: the *consistency of the two rival theories with other bodies of accepted, well-established knowledge* will obviously become a deciding factor, supposing the unfortunate hypothesis that the two theories are equally well-confirmed by the data, that is, *equally accurate from an experimental or observational viewpoint.*[9]

Now, so the story goes, what should we do in cases where the two rival theories are also both consistent with everything else we know and have the same *scope* or *unifying, explanatory power*? Since *fruitfulness*, or the capacity to generate novel predictions, is very often only a post factum virtue (we cannot know in advance which of the two theories will be "fitter" as shown by its having more "offspring"), we are left with *simplicity*, which, besides its vagueness and language-dependent

features, is not a sure-fire guide to truth:[10] ellipses, while less *simple* than circles, still describe the route planets travel!

If we continue supposing the *historically rather implausible* hypothesis that all the "epistemic virtues" (explanatory power, simplicity, fruitfulness, and so on) are satisfied to the same degree by any two empirically equivalent theories, and that the body of evidence remains stable for some time, it would seem plausible to assume that *other, nonepistemic principles of choice should intervene*. If one theory had more acceptable ethical or nonepistemic consequences than the other, should it not be preferred just for this reason? My answer is, of course, that it should! If we could concede all the hypotheses we have introduced so far, in this case an important role for nonepistemic values should certainly be acknowledged.

Note, however, that the way I formulated this example suggests, first of all, that we intuitively distinguish epistemic from nonepistemic values. That we are legitimated to switch from epistemic to nonepistemic values only after our resort to epistemic values fails shows at least two things: in science epistemic values are regarded as hierarchically more important than nonepistemic values, given that we first try to resolve the case of indetermination with the help of epistemic values. Second, we tend to regard the two sorts of values as being *independent of each other*.

After the "wager argument" put forward by Pascal, we know that whenever the *evidential* reasons for two alternative courses of action are approximately equal in weight, we may call into play our *prudential* reasons or, simply put, our nonepistemically based preferences or values. *But I contend that the whole argument by Pascal is predicated on the possibility of separating the evidential (epistemic) from nonevidential or prudential reasons*. Otherwise, Pascal's suggestion of *acting as if we believed in God* in order to end up believing in God would not make sense. It is because we neither believe nor disbelieve at the evidential level that it is necessary to force the body (*il faut plier la machine*) to certain actions, like going to mass or taking the communion. In a word, our prudential reasons for acting (the desirability of an option) should take precedence over our evidential reasons (its probability) only when we have no clear (subjective or objective) evidence for the probability of the conflicting hypotheses. If, in the process of evaluating the expected utility, we

calculated the probability only by considering the desirability of a possible state of affairs, our decision would be irrational: believing *in all cases* that our desire for *X* is going to influence the probability that *X* will occur is a recipe for disaster![11]

The following example should help us to understand what exactly the issue at stake is in real, as opposed to fictional, science. Suppose we have two physical theories that are empirically and theoretically equivalent (that is, they satisfy each of Kuhn's epistemic values to the same degree) but such that they tell us different stories about, say, the deterministic or indeterministic nature of physical systems: Shouldn't we opt for the theory that pleases us most in terms of the consequences for our freedom (supposing, for the sake of the argument, that either determinism of indeterminism is relevant to make room for a free will)? That this example might not be too far-fetched is clear from the "rivalry" between Bohmian mechanics (which treats quantum probabilities as epistemic) and standardly interpreted, *nonrelativistic* quantum mechanics, according to which the quantum world is irreducibly indeterministic. Shouldn't we endorse the former rather than the latter interpretation of the formalism because of, say, our Spinozian preference for a compatibilist solution to the problem of the relationship between determinism and free will?

Before considering these questions, //**Au: *these* questions? 2 questions asked in preceding?**//notice how real-life cases differ from the possible worlds we have been conceiving of so far. First of all, the equivalence of all the *epistemic virtues* of Bohmian and standard quantum mechanics is not to be conceded so easily--for instance, the *explanatory power* of Bohmian mechanics is unquestionably greater than that of the Copenhagen interpretation ofquantum mechanics. //**Au: is this what you mean? If not, pls clarify**//What matters, of course, is that physicists and philosophers of physics do *not* typically regard such a "theoretical" virtue as explanatory power as sufficient to justify the choice of Bohm's interpretation over Bohr's, for the simple reason that the *empirical* equivalence of the two theories in question is not genuine! Bohmian mechanics *as yet* does not cover *relativistic* quantum mechanics in a satisfactory way (it is not Lorentz covariant), and outside the nonrelativistic regime it cannot be regarded as empirically equivalent to standard, indeterministically interpreted quantum mechanics.

However, for the sake of argument, suppose that *at the present stage of our inquiry* these two physical theories are completely equivalent from *both* an empirical and a theoretical viewpoint. What should we do with our nonepistemic values? Two answers are possible, depending on whether such an empirical equivalence in question can be regarded as *temporary* or *definitive*.

First answer: if the empirical equivalence of the two theories ends up being a *temporary* predicament, that is, a situation that can be overcome by further developing our research, *then* one's commitment to compatibilism or incompatibilism between free will and determinism could provide an excellent reason to *pursue* one theory rather than the other. However, such a reason would clearly be *extraneous* or *external* to the evidence for a physical theory rather than the other, in the sense that it could not justify our belief in the determinism or indeterminism of quantum mechanics as such--unless and until, for instance, Bohmian mechanics were pursued and developed to a point where it would become experimentally more accurate than its rival in all known domains of application. Lacking any empirical progress of this kind, however, I do not see why one should *choose* at all costs between the two equivalent theories by invoking nonepistemic, ethical or metaphysical values: from an epistemic point of view, one should conclude with Galileo's two little words, *non sappiamo*! (we do not know), words that, he remarked, we have so many resistances to pronouncing. As it will become clearer in the next section, I consider this skeptical attitude to be a fundamental *norm* aimed at saving the integrity of science's claim to knowledge. I am not claiming, of course, that there have never been cases in which nonepistemic values have intruded into decisions among temporarily empirically equivalent theories; I am just claiming that the role of these nonepistemic values in science is to push us to know more.

The same remark applies, I dare say, to any other commitment to nonepistemic values. Whenever our commitment to purely epistemic values puts us in a condition of having to face two empirically equivalent theories, we can only hope that temporarily "adopting" or "pursuing" one of the rival theories *even on nonepistemic grounds* can eventually lead us to a situation in which their *empirical* equivalence will be overcome. Accepting a theory on the grounds of nonepistemic values need not imply belief in the theory, which should always involve epistemic values, and in particular

experimental accuracy. However, what should we do with a situation in which we suppose that the empirical equivalence is *not* temporary?

This possibility takes us to the second answer to our questions: **//Au: questions?//** If the hypothesized empirical equivalence of the two theories were really a matter of *principle*, that is, *if* we could show that we could never know, even in principle, which of the two theories is the correct one, then the situation with respect to our nonepistemic values *could* change. Note, however, that even in this case we would not be *forced* to introduce nonepistemic values to choose between to the two theories, since choosing between them on grounds other than epistemic would be *permitted but not mandatory*. It is only from the point of view of our *nonepistemic* interests that it might be important to choose between the two equivalent theories--this would depend on the theories in question, of course--but it is also essential to recall that *by hypothesis* such a choice would have no effect whatsoever on future empirical research concerning *those* theories, even though it might affect other theories.

From the viewpoint of our *epistemic interests*, in any case, I think it would be plausible to conclude that the difference between two theories that are empirically equivalent in principle on all epistemic virtues is purely *verbal*: the *two* theories are really to be regarded as *one and the same* theory, cast in different but semantically "equivalent descriptions" (Reichenbach 1958, 35; Carnap 1966, 150). Accepting this conclusion does not depend on a neoverificationist theory of meaning: remember that the equivalence of the two theories involves not just experimental virtues like observational accuracy but also more "theoretical" virtues like explanatory power, scope, simplicity, and consistency. If by hypothesis nothing could ever be discovered that would differentiate the theories theoretically and evidentially, one may conclude that a difference that does not make a theoretical and an empirical difference is--at least epistemically speaking--no difference at all. If the cognitive content of the two theories is the same, it is hard even to make sense of the fact that one could choose between them on the basis of nonepistemic values, since, to the extent that we do not have *two* alternative theories, *there is really nothing to choose from.*

Given that it is only by considering issues pertaining to the ultimate aims of science and to the place it should have in our life that nonepistemic values seem to come into play, in order to inquire

into whether they should have any such role or not, we should move forward to consider the third type of dependence of science on values.

Before doing so, however, it is appropriate to reemphasize the limited role of nonepistemic values in theory choice and the very remote possibility that two theories can count as being equivalent on *all* the epistemic virtues mentioned earlier. Even more remote is the possibility that the scale weighing the two empirically "equivalent" theories will remain in equilibrium for very long. *There are few if any historical instances of pairs of empirically equivalent theories remaining equivalent for a long time*: the heliocentric theory superseded geocentric astronomy as soon as Newtonian mechanics became established, and Foucault's experiment in 1853 decided the controversy between the wave and the particle theory of light not too long after it was sparked at the beginning of the nineteenth century.[12]

<H1>3. Regulative Values of Science and Nonepistemic Values

<FFL>In considering what I referred to as the regulative, or global, aims of science, namely (1) whether science ought to aim for *truth* or merely for *empirical adequacy* (van Fraassen 1980) and (2) whether it ought to postulate the *existence of unobservable entities to explain* the directly observable phenomena or to be content to describe, predict, and control the observable phenomena by remaining silent about the nonobservable realm, it seems natural to ask whether nonepistemic values play a major role in determining which of these aims scientists typically choose or ought to choose.

<TX>Unquestionably, the kind of knowledge science provides is an important value in our life; consequently, trying to understand the function this knowledge plays and ought to play in the overall scheme of things clearly requires considering it in connection with the rest of our values, particularly our *nonepistemic values*. Of utmost importance is *both* how science affects our nonepistemic values *and* whether such values do, and ought to, intervene in scientific methodology.

Schematically, and by recalling Laudan's reticulate model (1984), we can suppose either that (1) nonepistemic values (in the sense previously defined) have an impact on *facts and/or their interpretations* or, less radically, that (2) they affect only the level of science's general values, making

us militate *in favor of or against* //**Au: do you mean *in favor of or against*? Yes”**// scientific realism. earlier.[13] These two possibilities will be discussed in turn.

Consider a classic example that illustrates how one's commitment to certain nonepistemic values may affect one's methodological choices and eventually have repercussions on the interpretations of facts. Historically, it is not implausible to suppose that opposition to Galileo's realism about the hypothesis of terrestrial motion was motivated by the Church's apologetic attempts to defend both its own authority as the only institution entitled to a correct interpretation of the Bible and an anthropocentric worldview that was in better accord with creationism and therefore with the possibility of keeping some sort of moral and political authority over other human beings. Consequently, the political and ideological character of these goals would make Cardinal Roberto Bellarmino's *instrumentalism* about the Copernican hypothesis a direct consequence of a prior adoption of some nonepistemic values. Bellarmino's belief in Ptolemaic astronomy as the true hypothesis "corresponding" to the facts was compatible with his concession that using the Copernican hypothesis as a mere instrumental device could be useful to simplify the astronomical calculations. For him, there was no evidence at all in favor of the motion of the earth.

Clearly, in this case the interpretation of a scientific hypothesis (the Copernican one) as a mere algorithmic device affects what should count as "fact," and it is constrained by one's allegiance to some nonepistemic values. Furthermore, if we take for granted that the Church's general political goals included those listed in the previous paragraph, such an instrumentalism concerning the heliocentric hypothesis was not irrational with respect to those goals. In general, and for obvious reasons, treating *all* scientific theories as mere predicting devices and denying them any truth or explanatory capacity are powerful ways to avoid possible conflicts with the teachings of various churches, from the divine origin of all life on earth to the immortality of the human soul.

Note, however, that the strength or degree of plausibility of philosophico-scientific arguments in favor of realistic *versus* instrumentalistic interpretations of science *ought* not be decided on the basis of one's allegiance to atheism rather than to a revealed religion. We believe that good philosophical arguments in favor of or against scientific realism should be acceptable to people of all

religious beliefs, atheists included: for instance, the so-called pessimistic meta-induction proposed by Laudan is not an exception to this rule. In short, the reasons we may have for *wishing* a philosophical thesis to be true are not among the reasons we have for *believing* it.

I take it as a fact that since the time of the epistolary between Leibniz and Clarke,[14] the weight of politico-theological arguments (correspondingly, of *some* nonepistemic values) within physics has progressively dwindled. Nowadays, we do not believe that the validity of scientific-philosophical arguments in favor of the absolute or relative character of motion ought to depend, in principle, on reasons external to experimental and/or mathematical physics proper (and therefore on whether it would be better for us to live **//Au: live?yes//** in a universe in which God intervenes often or never intervenes, as Clarke and Leibniz respectively believed). This change in attitude toward the role of theological hypotheses within physics from the eighteenth century to the present is due not only to the unverifiable character of such hypotheses but also to the shared, *normative* intuition that theological values may hinder the intersubjectivity of science, given that they are *not* universally shared.

The same argument applies, I surmise, to the rest of our ideological and political values: letting idiosyncratic nonepistemic values play a role in science would run the concrete risk of jeopardizing one of the few cultural conquests of humanity (possibly the only one) that is capable of promoting agreement and consensus, namely science. And of course, this is one of the nonepistemic reasons why one ought to believe in science, but does not and should not to play a role in evaluating single scientific hypothesis. **//Au: footnote text is missing. Pls adviseeliminate footnote number//** Textbook mathematics, physics, chemistry, biology, psychology, and economics are examples of knowledge *de facto* shared across different cultures and ethical and religious values. If science's claims to knowledge depended on such nonepistemic values, people of different nations would non agree on textbook science as they in fact do..

Of course, this is not to deny the existence and importance of debates within science, but they are crucially directed toward areas of less reliable knowledge, and they should not be resolved in

terms of one's allegiance to nonepistemic values. The agreement in science is explained by scientists' *shared* commitment to epistemic values like consistency, experimental accuracy,scopeetc..

Whenever our pursuing epistemic values seem to have an impact on our nonepistemic values, are we allowed to maintain that facts totally depend on nonepistemic value-relative interpretations?

It may be true that the very strong evidence in favor of Darwinian evolutionism has the effect of weakening one's faith in Creationism, but this does not mean that the evidential relationships linking facts and hypotheses in evolutionary biology depend on some biologists' atheistic or materialistic beliefs, as some Italian intellectuals have recently maintained. Note that if these intellectuals were correct, we should introduce Creationism in high schools in the name of a *pluralistic* stance on *ethical* values, and this obviously does not seem right: Creationist and evolutionistic explanations of the origin of our species can and should be compared purely on the level of evidence-theory relationship

In a word, if we defend the value-ladenness of scientific facts in this more internal, constitutive sense (not in the external sense according to which facts are only selected by our values, as in section 1), I see no argument to counter a neo-Nazi's claiming that the "alleged" facts concerning the Holocaust are constituted by the nonepistemic values of the Jewish oligarchy. If we "ideologize" the natural and historical sciences through and through, the price is letting our will to power subjugate every domain of our experience.The view that *all* natural and historical facts are constituted in a strong sense by our nonepistemic values may engender in those who hold it a feeling of self-importance and power , but, besides being based on a wrong philosophical argument, this view may significantly reduce the possibility of a peaceful coexistence of our species. And this is another nonepistemic value which should make us opt for the kind of knowledge provided by science, even though such a value never intervenes in adjudicating internal scientific disputes. The wrong philosophical argument depends on the simple mistake of not recognizing that any possible inquiry into the natural world *presupposes our assuming that there is a way things are*, that there are *facts* that are independent of our wishes and cognitive or noncognitive preferences. Without this

assumption of independence of facts from our nonepistemic values, trying to find out how things are would be a meaningless enterprise.[16]

Note, furthermore, that a relevant amount of scientific knowledge seems to be completely devoid of any social consequence whatsoever. Prima facie at least, the fact that planets have elliptical orbits around the sun or that hydrogen is more abundant than helium in the universe has no relationship to our being committed to racism, sexism, feminism, or any other political or ideological value. Even if Hume's rule "no ought from is" were false (a big if), it is not at all clear how one could use facts like these to promote one's ideological case. Not all science is so disconnected from our ethical and political interests, of course, and one ought to be careful in denouncing ideological uses of science. But all such uses are either (i) a violation of Hume's rule or (ii) an illegitimate attempt at using a nonepistemic value as the only reason to believe in an empirical hypothesis, or (iii) make reference to cases in which our knowledge is very unreliable.[17]

It might be surmised that the defender of a role for nonepistemic values in the third sense need not go so far as to deny the independence of facts from nonepistemic values and be content with the remark that the aims of science in general are and should be intrinsically constituted by our nonepistemic values. Given the undesirable consequences of denying the existence of value-neutral facts in the relevant (internal) sense, arguing that our stance about the issue of scientific realism is infected by our nonepistemic values would then appear a much more plausible road to take.

To understand this more moderate claim, consider a beautiful example taken from Albert Einstein's autobiography (1949), in which we are told that the natural sciences, interpreted as an effort to decipher a mind-independent, external world, can be ascribed the overarching purpose of "freeing us from the chains of the merely-personal":

<EXT>It is quite clear to me that the religious paradise of youth, which was thus lost, was a first attempt to free myself from the chains of the "merely-personal," from an existence which is dominated by wishes, hopes, and primitive feelings. Out yonder there was this huge world, which exists independently of us human beings and which stands before us like a great, eternal riddle, at least partially accessible to our inspection and thinking. The contemplation of this world beckoned

like a liberation, and I soon noticed that many a man whom I had learned to esteem and to admire had found inner freedom and security in devoted occupation with it. . . . The road to this paradise was not as comfortable and alluring as the road to the religious paradise; but it has proved itself as trustworthy, and I have never regretted having chosen it. (Einstein 1949, 4)

<TX>It could be maintained that a belief in a mind-independent "external" reality was an essential presupposition and motivation of Einstein's research, and that, plausibly, a belief in unobservable, mind-independent entities was part and parcel of his religious appreciation for the mysteries of nature. Believing that what exists coincides with the bound of our perceptions--like the typical idealistic philosopher would have it--would not have helped Einstein to free himself of the chains of the purely personal or to relate his limited spatio-temporal existence with the observable universe as a whole. And yet the EPR argument[18] **//Au: Will all your readers know what this refers to? add a brief explanation in a footnote?//** in favor of the incompleteness of quantum physics and therefore of a more realist understanding of the theory, though possibly *motivated by Einstein's idiosyncratic religious faith*, obviously did not make any reference to his "religious" faith in realism. Einstein's argument against the view that quantum mechanics as Bohr interpreted it had to be regarded as complete did not mention his allegiance to scientific realism or his belief in the independence of the quantum world from our measurements but tried to uncover technical difficulties that a physicist of different philosophical convictions like Bohr could acknowledge and worry about.

Likewise, in claiming that the molecules postulated in statistical mechanical models are real, Ludwig Boltzmann was surely motivated by a *general* realistic attitude toward science that he defended in various ways in his scientific and philosophical production. However, until a *common-cause argument* could be produced in favor of the atomic theory of matter, one that made reference to thirteen different empirical methods to calculate Avogadro's number, all converging to the same value, Boltzmann's belief in the reality of molecules was not able to conquer the vast majority of his colleagues.[18]

The moral of these examples is twofold. First, general philosophical convictions about the goals of science are usually *not* sufficient to conquer scientists' allegiance to a *particular*

interpretation of a *particular* scientific theory, since typically only sufficient experimental or mathematical work can decide the issue. And even where the issues are still unsettled, //**Au: already settled? still unsettled?**// as is often the case in philosophy, the commitment to certain nonepistemic values can at most *motivate* a philosopher to build an argument, but such values by themselves are not sufficient to regard the argument as good. Consider, for example, van Fraassen's criticism to the inference to the best explanation, sometimes used by scientific realists to claim that the truth of science is the only explanation that does not turn its predictive success into a miracle(van Fraassen 1980). //**Au: meaning of preceding clause unclear -- too many prepositional phrases. Perhaps you mean something like "...like van Fraassen's criticism of the inference to the best explanation."? Please advise**// The validity, plausibility, or strength of van Fraassen's criticism do not depend on his religious motivations to defend constructive empiricism, and also a scientific realist can recognize that inferences to the best explanation are not sound arguments, if indeed they are not.

The second moral is that perhaps we should stop worrying about "the general aims of science," because we can give a clear answer to the question of correctness of the ontological assumptions of science only by looking at specific, particular scientific theories. Depending on the arguments at hand and the degree of maturity of a certain theory, one can be a scientific realist about a particular set of hypotheses while accepting an instumentalist stance toward a different set.

Finally, notice that whether truth is to be regarded as the aim of science is *de iure* question, //**Au: de jure?yes**//given that most scientists *de facto* regard the discovery of truths about a mind-independent, unobservable world as the main goal of their efforts. Philosophers of course debate whether scientists are justified in doing so, that is, whether the epistemic and methodological resources (*the means*) yielded by current scientific doctrines are adequate to reach truth (*the end*). Since all means-to-ends relationships in principle are descriptive and subject to empirical inquiry (the means can be regarded as "the causes" of the attainment of our aims which are their effects), the question of truth in science can be analyzed without resolving the normative problem, since it is

amenable to rational and objective discussions as is the correctness of any other hypothetical imperative of the kind, "if you want *x*, do *y*."

<H1>4. Applied Science and Nonepistemic Values

<FFL>Besides their role in choosing facts and problems, as discussed earlier, *nonepistemic* values also play a fundamental role in the application of our scientific knowledge to policy making. The structure of the argument is simple and well-known: whenever the *acquisition* of new knowledge or the technological *application* of already possessed scientific knowledge have nonepistemic consequences, say, on the environment, our health, or economic production, nonepistemic values are involved. Often, such values take precedence over the epistemic ones, sometimes for good reasons, sometime for bad. For example, if scientists wanted to find out (an epistemic aim) whether a certain chemical that would have wide industrial use increases the risk of human beings getting cancer, they should not be allowed to experiment directly on people. In general, whenever the application of already possessed knowledge, the means to achieve new knowledge, or the production of new technological devices have damaging nonepistemic (ethical or environmental) effects, we are obliged to refrain from applying our knowledge or from satisfying our epistemic curiosities.

<TX>In addition to these noncontroversial considerations, it has recently been argued that there are cases in which nonepistemic values "have a legitimate role to play in the *internal* stages of sciences" and "are required for good [scientific] reasoning" (Douglas 2000, 565, emphasis added). In what follows, I will briefly evaluate Douglas' claim that the so-called inductive risks carried by many statistical hypotheses pose a threat to the standard view of scientific reasoning as value-free in the "internal sense" explained earlier.

Inductive risk, as Hempel defined it, is the chance that one's hypothesis is false, despite its having been accepted, or the chance that it is true, despite its having been rejected (1965, 92). In a word, inductive risk is the (sometimes nonnegligible) chance that our scientifically acquired beliefs may be wrong. Douglas considers the interesting methodological problem of having to set a standard for the *statistical significance* of certain toxicological tests. Imposing *stricter* standards //**Au: or**

***raising the standards*? If it is not too ugly, leave as it is //** of statistical significance (in comparison with control groups) will reduce the number of false positives and increase the number of false negatives (making the chemical appear to be *less* dangerous than it actually is), whereas imposing *laxer* standards **//Au: or *lowering the standards*?same as above//** will increase the number of false positives as well as increase our chance of considering ill an animal who is healthy: in this case, the chemical will appear to be more dangerous than it actually is. The question that arises is, *if we cannot lower the number of both false positives and false negatives, what should we do*?

In cases like these--even before applying our knowledge coming from the result of the tests, and consequently deciding to prohibit or allow the production of a certain toxic substance--it would seem, so Douglas argues, that the "scientific," empirical claim itself depends on a previous choice of *overregulating* (by setting lower standards of significance) or *underregulating* (by setting higher standards of significance). Overregulating may better protect our health but may have a greater cost for the economy; underregulating may be more dangerous for us but perhaps costs less for the industries involved. In any case, both decisions seem to be strictly dependent on *nonepistemic* values.

It could be objected that, quite independent of one's preference for certain nonepistemic values over others--whether to be more protective of health or a manufacturer's interests--tests forcing us to arbitrarily balance false positives and false negatives exemplify *bad scientific tests* or *bad science*. Here "bad science" and "bad tests" simply mean science and tests that are not conducive to *highly reliable* results, unlike other ordinary tests that are used to diagnose the presence of certain diseases, such as those for prostate cancer. All we should do to improve the situation from an epistemic point of view is to make our sample larger **//Au: make our sample larger? to increase the size of our sample? //** or to change the test in some way, so as to lower the inductive risk.

This desirable strategy, however, may not always be possible. Consequently, in order to find the toxicity level of the chemical, some significance level must be set, and such levels must be determined, as Douglas' case is set up, by the two nonepistemic values of protecting our health or favoring the production of useful but toxic chemicals. And while such tests are not as reliable as other tests in other areas, we still need a policy in the short run, since "in the long run we'll all be dead," as

Keynes put it, so we cannot wait around to see if other, more reliable tests will be found some day. As Douglas puts it, "some balance must be struck" between the need for reducing the number of false positives and the need for reducing false negatives--needs that for each particular kind of test cannot be satisfied simultaneously.

Douglas concludes that in cases like these, nonepistemic values *determine to some extent* the empirical content of our hypotheses. While we are obviously aware that this is the case, we somehow "use our ignorance" to act on the basis of one nonepistemic value rather than another. And despite many of us feeling that setting laxer standards of significance is preferable in order to protect our health, we all know that economic interests at times may be so powerful as to succeed in convincing the "experts" to choose otherwise.

Independent of what occurs in real life in cases like this, it seems undeniable that Douglas's and analogous arguments in favor of the intervention of nonepistemic values in the so-called internal (that is, evidential knowledge-gathering) processes are predicated on the existence of *still uncertain areas of scientific inquiry*--that is, areas in which we still do not know the real effects of certain kinds of causes. *It is exactly this lack of reliable scientific knowledge that explains the interventions of nonepistemic values in the setting of parameters for the significance of the test*. Douglas seems to accept that *ignorance* would be a better word for what concerns the toxic power of dioxin, the case she discusses: "Where the balance should lie for dioxin studies is currently unclear" (2000, 569).

Those who are in charge of deciding what to do should keep this conclusion in mind, since claiming that we have reliable statistical knowledge and are therefore entitled to deregulate or overregulate according to the case is one thing; it is quite another to openly admit our ignorance. Fortunately, not all cases of applied science are like this--think of all the situations in which a *positive* result in a test uncontestably indicates the presence of the disease (consider diseases such as AIDS, epilepsy and some cancers)**//Au: is this what you mean?//** --but it must be admitted that a certain amount of inductive risk accompanies any decision in policy making.

Note, however, that the methodological problems of setting the parameters for the significance of the test do not jeopardize at all the *intersubjective validity* of the empirical claim made

by the statistical hypothesis. In fact, regardless of the standard (strict or lax) one chooses according to one's preferred nonepistemic values, both parties--the overregulators and the underregulators--agree that in both cases our statistical hypotheses carry a measurable margin of error. Namely, they agree that *by choosing a stricter standard dioxin will appear to be less dangerous than it is, and by choosing a laxer standard dioxin will appear to be more dangerous.*//**Au: OK?yes**//

If this is the content of the cognitive claim before we decide which test to administer, we have to try to clarify the sense in which the content of the hypothesis is biased by the choice of the two nonepistemic values. As explained in section 1, we often need and use nonepistemic values to select facts or empirical hypotheses, or to stress one causal factor rather than another in a very complex causal field. Is there any significant difference between the examples discussed in section 1--in which two different nonepistemic values selected different facts or different relevant causal aspects of the same fact--and the case discussed by Douglas?

In Douglas'? case, the choice of two different nonepistemic values make us evaluate in a different way the probability of an empirical hypothesis (namely, that dioxin causes cancer), so that the empirical content of the hypothesis, the assigned probability, seems to depend on the nonepistemic value. This claim is compatible with the fact that the empirical hypotheses made in the two cases as a result of the two alternative choices are objectively valid, because *both* the underregulator and the overrugulator *agree* that by choosing the standard of significance in different ways the dioxine will appear either less or more dangerous than it actually is. Compare this situation with a disagreement about relevant explanatory factors, concerning the importance or relevance of a particular causal factor, depending on whether,in order to explain a car accident, for example, one privileges the speed of the car, the ice on the street, the bad brakes of the brand new car, or the slow reflexes of the driver. The dependence of causal explanations on pragmatic factors presents similarity with the case Douglas discussed, since the car's insurer, manufacturer, and driver may want to stress some factors over others. Also in this case, if no more information is available to decide whether one causal factor weighs more than another (provided all are present), it is not irrational to introduce one’s nonepistemic preferences, but this proves nothing about the subjectivity of scientific knowledge.

The problem of having to act or decide despite inductively insufficient evidence was of course known long before the case discussed by Douglas. In all cases where we must evaluate the probability of a course of action and have no objective frequencies (chance) to rely on we must trust *subjective probabilities*, in which desires and beliefs, prudential reasons and evidential reasons, may be inextricably meshed. For example, to have a good chance of successfully passing an examination, it may be rational to lead ourselves to believe that we will because, as William James taught in similar cases, *our chance of passing the exam, depends on our believing that we can act in such a way as to pass it.* Since the test of whether such a belief was justified can only be given by our taking the exam, in all cases in which we lack sufficient evidence and the desired outcome depends in part on us, it is rational to open the door to our nonepistemic values and interests.

In short, when we have "to strike the balance" for the statistical significance in the dioxin studies, we are just looking at another manifestation of Pascal's wager: "Act as if God existed, if this uncertain state of affair is highly preferable to you, *provided that* the evidence for its existence equals that for its non-existence and the corresponding expected utility is greater." However, whenever *prudential* reasons can be sharply separated from *evidential* reasons--namely, when our empirical knowledge is highly reliable and we cannot just believe what we like--I cannot see how inductive risks in Douglas's sense pose any threat to Longino's claim (1990, 85<N>86) that values intervene in the *application* of scientific knowledge and not in its *constitution*. Douglas seems to agree on this point, since she recognizes that "Hempel was right in asserting that whether or not a piece of evidence is confirmatory of a hypothesis . . . is a relationship in which value judgments have no role" (2000, 564).

To summarize, Douglas's dioxin case appears to favor the view that nonepistemic values intervene in the constitution of the evidential claims of science only because *we still do not know how things really are*. But since Douglas is certainly not prepared to argue that we are always as ignorant as we are in the dioxin case, the value of her analysis in cases where the application of our scientific knowledge has social impact is limited to cases in which our hypotheses are as unreliable as those involving the dioxin case. Furthermore, if we consider that in *many* areas of astrophysics and

theoretical physics and other bits of natural science no social impact is foreseeable and no nonepistemic values seem to intervene in the formation of the hypotheses, the impact of nonepistemic value in evidence-theory relationships should not be exaggerated (see Douglas 2000, 577, for her admission of this point).

Certainly, if in the dioxin case our knowledge were more reliable, the balance between false positive and false negative would not be struck according to one's evaluation of the relative importance of ethical values and economic constraints. Since deciding on the basis of the available tests may be leading us into an irresoluble conflict of nonepistemic values, the moral of the story is that epistemic values should take precedence whenever possible:not by chance, also Douglas admits that we should try to *know more* by changing the experimental technique or by increasing the number of tested cases (Douglas 2000, 566)..

<H1>Conclusion

<FFL>The role of nonepistemic values in science, as it relates to the first three cases in which science depends on values, seems to be limited to the context of *pursuing* a theory, while in the fourth case it is linked either to the various necessities of applying our knowledge or to formulating belief partially supported by nonepistemic values in order to reach a decision. In all four cases, however, no serious objection to the neutrality of the cognitive content of scientific hypotheses seems forthcoming, and this is the result that I find most important. The fact that nonepistemic values may influence the methodology through which the empirical results can be gathered in cases in which we still do not know much about the empirical links between causes and effects is just an indication that we have unreliable knowledge, and, in this sense, no "good science" at all.

<TX>Much more interesting would be the claim that social, nonepistemic values *ought to* condition our acceptance of a theory from an evidential point of view, namely, that we should believe in a certain hypothesis because it supports one ideological position rather than another; however, no serious attack in this direction seems in the offing from the type of cases Douglas studied. That nonepistemic values sometimes do condition the acceptance of a theory is, of course, true, but this is a

bad thing: science *ought* to steer clear of nonepistemic evaluations of the evidential support of hypotheses or theories. Whenever attacking the intersubjective validity of the theory-evidence relationship threatens to become a favorite sport, we should remember that if the *existence* and the *explanation* of facts depended purely on one's involvement with certain religious, ideological, or political values, only the economically, religiously, and politically powerful would have a chance “to be right”.

<NH>Notes

<NTX>I thank Fabio Bacchini, Heather Douglas, Peter Machamer and Gereon Wolters for their numerous critical comments to an early draft of this chapter.

1. For instance, according the type of relationship linking hypothesis and evidence, the former could qualify as plausible, probable, or certain, typical evaluations we attach to hypotheses. Consequently, "certainty," "high probability," and "plausibility" count as epistemic values.

2. Of course, this claim is consistent with the fact that the hierarchy among these values is different for different scientists.

3. This is because of the Humean separation of *is* from *ought.*

4. We will see that it is the social control ensured by the public character of science and the *actual* commitment of scientists to values such as experimental/observational accuracy of hypotheses and their logical consistency that de facto ensures science's intersubjective validity.

5. For a more recent evaluation of this problem that defends the same conclusion, see Longino (1990, 85<N>86).

6. Whether science can reach objectivity, in the sense given by the discovery of a mind-independent world, is a different problem but strictly related to the possibility of explaining the kind of intersubjective agreement that we reach in the sciences. I investigate this other sense of "objective" in part in section 3.

7. Philosophers that, like Quine, have stressed the paucity of inputs (empirical data) vis-à-vis the richness of the hypotheses that are produced to explain them (outputs) may belong to this category.

But in this context I am interested not in capturing anyone's position but merely in exploring a possibility.

8. See Norton (1993, 1), whose analysis is a brilliant defense of this thesis, illustrated with the case study of the origin of quantum discontinuity.

9. To be charitable toward the defender of a role for nonepistemic values in theory choice, I am supposing, against Laudan and Leplin (1991), that empirically equivalent theories are not differentially confirmed. For a criticism of Laudan and Leplin, see Okasha (1997).

10. Algorithmic complexity theory may significantly improve our understanding of the requisite for "simplicity" in a theory, provided that the theory is axiomatizable. **//Au: any other way to say this, besides *axiomatizable*?**// In this case simplicity can be measured in terms of the length of the shortest program that is necessary to generate the theorems (laws) of the two theories.

11. An important exception to this rule will be discussed in the fourth section and has to do with cases in which the occurrence of the future state of affairs depends on our belief that the state of affair itself will occur.

12.Stathis Psillos (1999, 168) also stresses these points.

13. In Laudan's view the methodology of science can be construed as a set of hypothetical imperatives of the type, "if you want aim *x*, follow methodological rule *y*," where *x* represent our cognitive and (possibly) noncognitive aims. Among such cognitive aims, here I focus on those belonging to the issue scientific realism versus antirealism.

14. Their correspondence begins in 1715. The epistolary is collected in Alexander (1956). **//Au: not in reference list. Please provide//**

. **//eliminateAu: Is this simply a numbering error? Please check numbers in text and advise//**

15. For this transcendental argument, see Searle (1998).

16. The third disjoint of this alternative will be discussed in the next section (section 4).

17 This argument used quantum correlations to argue that quantum mechanics is either nonlocal or incomplete. Using two particles measured in different locations in space but originated from the same source, according to Einstein Podolsky and Rosen one could predict with certainty the measurement result on the distant particle after having measured the

particle nearby. If measuring the particle in one wing does not disturb the distant particle (*locality*), the fact that one can predict with certainty the far-away result entails that the particle had a definite value before the measure, which means that the theory as standardly construed is incomplete.

18. The classic text in this context is Perrin (1913). The common-cause structure of the argument has been clarified by Salmon (1990).

<RH>References

<RTX> Alexander, H.G. (ed.) (1956), *The Leibniz Clarke Correspondence*, Manchester, Manchester University Press.

Carnap, R. 1966. *Philosophical foundations of physics*. New York: Basic Books.

Douglas, H. 2000. Inductive risks and values in science. *Philosophy of Science* 67 (4):559<N>79.

Einstein A. 1949. *Albert Einstein, philosopher-scientist*, ed. Paul Arthur Schilpp. New York: Harper & Row.

Hempel, C. 1965. Science and human values, in *Aspects of scientific explanation and other essays in the philosophy of science*. //**Au: this is a chapter in an edited volume? No a collection of articles by Hempelname of editor? page numbers?**//New York: Free Press, pp.81-96.

Kuhn, T. 1977. *The essential tension*. Chicago, IL: University of Chicago Press.

Laudan, L. 1984. *Science and values*. Berkeley: University of California Press

Laudan, L. 1996. *Beyond positivism and relativism*. Boulder, CO: Westview Press.

Laudan, L., and Leplin J. 1991. Empirical equivalence and underdetermination. *Journal of Philosophy* 88:449<N>72.

Longino, H. 1990. *Science as social knowledge: Values and objectivity in scientific inquiry*. Princeton, NJ: Princeton University Press.

Norton, J. 1993. The determination of theory by evidence: The case for quantum discontinuity. *Synthese* 97:1<N>31.

Okasha, S. 1997. Laudan and Leplin on empirical equivalence. *British Journal for the Philosophy of Science* 48 (2):251<N>56.

Perrin, J. 1913. *Les atomes*. Paris: Alcan.

Psillos, S. 1999. *Scientific realism: How science tracks truth*. London: Routledge.

Reichenbach, H. 1958. *The philosophy of space and time*. New York: Dover.

Salmon, W. 1990. *Forty years of scientific explanations*. Minneapolis: University of Minnesota Press.

Searle J. 1998. *Mind, language, and society*. Basic Books: New York

van Fraassen, B. 1980. *The scientific image*. Oxford: Oxford University Press.

Weber, M. 1904. Die Objektivität Sozialwissenschaftlicher und Sozialpolitischer Erkenntnis. *Archiv für Sozialpolitik und Sozialwissenschaft* 19:22<N>87.