PRINCIPIA **27**(1): 137–163 (2023) doi: 10.5007/1808-1711.2023.e80487
Published by NEL — Epistemology and Logic Research Group, Federal University of Santa Catarina (UFSC), Brazil.

VALUE-FREE IDEAL IS AN EPISTEMIC IDEAL: AN OBJECTION TO THE ARGUMENT FROM INDUCTIVE RISK

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Abstract. Arguing from inductive risk, Heather Douglas tried to show that the ideal of value-free science is completely unfounded. The argument has been widely acknowledged to be a strong argument against the ideal. In this paper, beginning with an analysis of the concept of an ideal, we argue that the value-free ideal is an epistemic ideal rather than a practical or ethical ideal. Then, we aim to show that the argument from inductive risk cannot be employed against the value-free ideal as far as it is understood as an epistemic ideal. We try to show that the argument takes practical and ethical limitations of actual scientific enterprise into account to undermine the value-free ideal. But employing non-epistemic considerations makes the argument impotent against an epistemic ideal.

Keywords: value-free ideal • inductive risk • policy-making • epistemic ideal • scientific objectivity

RECEIVED: 04/04/2021 REVISED: 21/04/2022, 01/08/2022 ACCEPTED: 30/09/2022

1. Introduction

The ideal of *value-free science* or the *value-free ideal* — hereafter VFI — can be traced back at least to the works of logical positivists and logical empiricists in the first half and the middle of the twentieth century (Douglas 2009, pp.47–9). According to this ideal, *non-epistemic* or *contextual* values (including social, ethical, political, and religious values) should not have any *internal* role in scientific activity, even though they play a part in the *external* stages of science, e.g. choosing a *subject* to undertake the research into, considering whether to employ a specific *method* due to ethical considerations, or applying the research outcome to satisfy some need (Lacey 1999, pp.16–7; Longino 1983, pp.7–8; Douglas 2007, p.121, 2009, p.50; Doppelt 2007, p.189). These external parts of science are related to the setting of a research project or to the *practical* applications of it. These are at the beginning of a research project — in fact, before it is started when drawing up its proposal — or after it is finalized. In contrast, internal parts of science generally include those activities done in order to test a hypothesis, either to support or to reject it. In general, these include gathering data, analyzing the gathered data, and interpreting the results.

Two different sorts of objections can be raised against VFI. First, it is objected that, due to some philosophical problem or because of the constraints any scientific research is subject to, the ideal is in principle or actually *unattainable* — or it is not *viable*. For example, some philosophers of science have tried to show that there is not a *sharp* dichotomy between epistemic and non-epistemic (or constitutive and contextual, or cognitive and non-cognitive)[1] values, and if so, the values considered to be "epistemic" or "constitutive" in scientific enterprise — roughly, the values that lead scientists to the epistemic (or, according to Levi (1960, p.350), "theoretical") goals of science, especially the goal of reaching nearly true or reliable knowledge (Douglas 2009, p.93 and passim; Levi 1960) — are in fact an entangled combination of both epistemic and non-epistemic values. Therefore, science cannot be value-free. (See Rooney 1992; Longino 1996; Machamer and Douglas 1998.) This line of argument, though important, cannot undermine VFI since it is not the case that every ideal should be attainable, even in principle (see de Melo-Martín and Intemann 2016, p.502).[2]

Second, it is argued that VFI is not actually an ideal for science, that is, it should not be an ideal for science to be value-free. Science *needs* non-epistemic value judgments and value-free science is not a "good" science. This kind of objection, if successful, *can* undermine VFI. It can also imply the first objection: if science does need non-epistemic value judgments, then VFI is not attainable at all.[3]

Following Rudner (1953) and Churchman (1948a),[4] Heather Douglas (2000, 2007, 2009, 2017) raised this latter objection by the *argument from inductive risk*. According to her, in those areas of science that have applications in policy-making — and are the most funded areas of science — an inductive error may lead to *non-epistemic* detrimental consequences. Any such "bad" consequence is due to one type of error, that is, either *false positive* or *false negative*. Moreover, due to some limitations, scientists cannot reduce both types of error at the same time. So, they should *decide* which error to reduce more than the other. But, these decisions that are internal to scientific activity are to be made according to non-epistemic values: scientists should decide to reduce the error that has more harmful non-epistemic consequences. Therefore, as these decisions are necessary for (and internal to) scientific activity, VFI is not beneficial to science.

In this paper, we criticize Douglas's objection to VFI. Beginning with an analysis of the concept of an ideal, we show that her objection does not fulfill all the requirements needed for any objection to VFI, which aims to undermine it thoroughly. In brief, the role Douglas describes for non-epistemic values in science, it is shown, is not constitutively necessary for scientific activity, since it is due to some practical (political or economic) or ethical constraints on scientific practice. And Douglas's argument can only be shown to be a case of conflict between VFI and some other ideal(s). It should be noted that by our objection to Douglas we do not mean that VFI is more important than any other ideal, or that non-epistemic values do not (or cannot) have any legitimate role in science. We only aim to show that when VFI comes into conflict with another ideal, it does not in itself undermine VFI, while we approve that in any such situation, we should after all make a choice between the conflicting ideals according to our most primary goals and values.

2. What Is an Ideal and How to Reject It

To criticize Douglas's argument against VFI, we first reflect on the ways through which the ideal may be rejected. To do so, we contemplate how an ideal may be rejected in general. But, what is an ideal after all?

By "ideal" we mean what Rosati calls a *substantive ideal*.[5] According to Rosati (1998), substantive ideals are those ideals that "present models of excellence against which things in a relevant class can be assessed, such as [ethical] models of the just society or the good person". About the ideals, Rosati (1998) continues:

> Substantive ideals delineate the features that something or someone must possess in order to be excellent in a specific regard. [...] substantive ideals logically imply certain evaluative judgments in conjunction with the facts about whether and to what degree something or someone possesses the relevant features.[6]

The phrase "to be excellent in a specific regard", reminds us about a *goal*: it can be said that any substantive ideal delineates some features needed for reaching a goal.[7] So, a substantive ideal — henceforth an ideal *simpliciter* — may roughly be stated as the following hypothetical imperative:

For agents of the group A to pursue the goal G, anything of the category C should have the feature F.

For example, the ideal of just society can be stated as the following hypothetical imperative:

For the members of a society to be as happy as possible, the political institutions, laws, relations, and practices of the society should be just.

The goal of an ideal is usually implicit in that ideal and any ideal can roughly be stated as the following categorical imperative *norm*:

Anything of the category C should have the feature F.[8]

So, the ideal of just society can simply be stated as the following norm:

The political institutions, laws, relations, and practices of any society should be just.

Consequently, any ideal can be criticized in two different ways. First, one may reject a seemingly ideal as not in fact being an ideal. To do so, one may either reject the goal toward which the ideal is directed, or deny that the feature the ideal delineates can direct one toward that goal. This kind of criticism can be found in Harry Frankfurt's (1987) work against the ideal of "economic equality". According to Frankfurt, if we consider economic equality as a moral ideal, it can be shown that it cannot be morally defended. He tries to show that economic equality does not lead to the "morally important social goal" of "maximization of aggregate utility" (1987, pp.25–6). He also argues that there is not a moral intuition against economic inequality. Rather, what seems "morally objectionable" is the fact that some of the people do not have enough money for their essential needs and are very poor, and "not the fact that the economic resources of those who are worse off are *smaller* in *magnitude* than [others]" (1987, p.32 (original emphasis)).

Second, one may show that, in some situations, an ideal is in conflict with some worthier ideal(s) — when two or more different ideals give contradictory prescriptions. This kind of criticism cannot thoroughly undermine an ideal alone. An example of this kind of criticism is Wolf's (1982) rejection of "the ideal of moral sainthood". According to Wolf (1982, p.421), a moral saint is a person who "must have and cultivate those qualities which are apt to allow him to treat others as justly and kindly as possible". Such an ideal, Wolf argues, "does not constitute a model of personal well-being toward which it would be particularly rational or good or desirable for a human being to strive" (Wolf 1982, p.419), since "such a life is *incompatible with* well-roundedness" (Wolf 1982, p.423 (emphasis added)), and the domination of morality in it "seems to require either the lack or the denial of the existence of an identifiable, personal self" (Wolf 1982, p.424). She says that "some of the qualities the moral saint necessarily lacks are virtues, albeit nonmoral virtues, in the unsaintly characters who have them" (Wolf 1982, p.426). She concludes: "moral ideals do not, and need not, make the best personal ideals" (Wolf 1982, p.435). So, Wolf's idea amounts to the claim that the ideal of moral sainthood, when it is in conflict with other non-moral ideals, is not the ideal that *should* trump the others: "I have meant to insist that the ideal of moral sainthood should not be held as a standard against which any other ideal must be judged or justified" (Wolf 1982, p.435) and "a person may be *perfectly wonderful* without being *perfectly moral*" (Wolf 1982, p.436 (original emphasis)).

Therefore, to undermine an ideal thoroughly, one should either reject its associated goal or show that the feature delineated by the ideal cannot direct one toward the goal. However, for such an argument to be acceptable, the associated goal should be of the same type as the ideal itself. For example, you can undermine a moral ideal only in relation to a moral goal, and it cannot be undermined in relation to a non-moral, e.g. political, goal. Then, to undermine an ideal, it should be shown that the feature the ideal delineates cannot direct people toward the goal associated with the ideal or is an obstacle in the way toward another goal of the same category as the ideal itself. For example, one cannot undermine the ideal of just society by showing merely that the just society would not develop the most powerful economy, which is an economic goal. It should be kept in mind that one may argue against an ideal by appealing to another worthier (but incompatible) ideal, whether or not the two ideals are of the same category.[9] However, as said above, it is not a complete rejection of an ideal.

3. Value-Free Ideal Is an Epistemic Ideal

VFI seems to be a substantive *epistemic* ideal: an epistemic *model of excellence* against which any piece of scientific work can be assessed with regard to the goal of *objectivity*, which is obviously epistemic. About this matter, Reiss and Sprenger (2020) write: "VFI may have an important function for *guiding* scientific research and for minimizing the impact of values on an *objective science*" (emphasis added). Although it does not seem to be a challenging claim to make, two reasons may be given for it.

The first reason is historical in nature: it seems that logical empiricists' initial motive behind their conviction that science should be value-free was mainly the belief that "science is [and should remain] the objective enterprise *par excellence*" (Machamer and Wolters 2004, p.8 (original emphasis)). For example, Reichenbach's distinction between context of discovery and context of justification seems to aim, inter alia, to exclude non-epistemic evaluative considerations from any objective scientific work and place them into the context of discovery. About this matter, Howard writes:

> For Reichenbach, as for Schlick, therefore, there is no room for social influence in theory choice in an objective science of nature. [...] The distinction between context of discovery and context of justification was his new way of attempting to preserve the epistemology of science from intrusions from the side of value and social influence. (Howard 2003, p.54; see also Douglas 2009, p.48)

Douglas, when considering the origins of VFI, states that the philosophers of science who argued for the ideal up to 1980s, "assumed that science was fundamentally and acceptably isolated from society" (Douglas 2009, p.46). According to her, logical empiricists like Hempel and Reichenbach also were focusing on the cognitive aspect of scientific reasoning, which they thought was distinct from any normative enterprise like ethics (Douglas 2009, p. 48).

The second reason for VFI being epistemic concerns the fact that in most discussions on the role of values in science, value-free ideal is actually treated as an epistemic ideal with objectivity as its goal. For example, Rudner says: "The traditional search for objectivity exemplifies science's pursuit of one of its most precious ideals", where he calls the ideal "the ideal of objectivity" (Rudner 1953, p.6; see also p.2). Douglas herself allocates a whole chapter of her 2009 book to argue for the position that the concept of objectivity the proponents of VFI posit is not necessarily inherent to scientific activity (Douglas 2009, Chapter 6).

But why is VFI associated with objectivity as its goal? The main reason behind this seems to be that VFI is aimed "to protect the epistemic integrity of science against the *problem of wishful thinking*" (de Melo-Martín and Intemann 2016, p.502 (emphasis added)). It means that if non-epistemic values enter into scientific reasoning, then they may be employed by scientists (consciously or un-/sub-consciously) as evidence for some hypotheses that they wish to be true. So, *value-ladenness* may corrupt the value-neutrality and then, the objectivity of science (see Anderson 2004; for an assessment and criticism of the concept "wishful thinking", see Steel 2018). Many opponents of VFI, among them is Douglas herself, have tried to show that the *legitimate* role of non-epistemic values in science does not corrupt the objectivity of science (see Douglas 2009, Chapter 6). Some other opponents of the ideal even go so far as to argue that entering values into scientific theorizing may help science be more objective. For example, some feminist philosophers of science defend appealing to feminine values against androcentric values in this way (see, e.g., Anderson 1995).

However, there are alternative non-epistemic views of the basis of VFI.[10] First, Bright (2018) discusses W. E. B. Du Bois' non-epistemic defense of VFI, according to which VFI is ultimately based on the essentiality of retaining public trust in science. Briefly, according to Du Bois, scientists in a democracy should try to win public trust so that the policy-makers elected by the public may draw on scientific results for decision making. And public trust in science may be won only if scientists are "pure-truth-seekers" not intervening their values into the scientific practice.

There are three reasons why Du Bois' non-epistemic defense of VFI does not make the ideal a non-epistemic one. First, for Du Bois, providing information for policy-makers is the *mediate* aim of science, and its *immediate* aim is reaching truth. However, as the proponents of the inductive risk argument emphasize, making science value-free is rather impossible, especially for those branches of science that serve to make public policies. Therefore, the pure-truth-seeker image of scientists seems to be unattainable, and as a result, we may well consider VFI as an ideal for this immediate aim of science — i.e. truth-seeking — and not for its mediate aim. The associated goal of the ideal is still truth-seeking or objectivity, which in turn serve the mediate aim of science. Second, as Bright states, democracy in public policy has an epistemic aim for Du Bois: that when all scientists contribute to make a decision democratically through proposing all the available hypotheses for a set of data — and not entering their value-judgments into their scientific works to reach a single hypothesis — the best decision is more likely to be made. Because in this way, the public will have access to the most information necessary to make an informed decision. And it can be made possible through following VFI and eliminating any value judgment from scientific practice. About this matter, Bright writes: "[U]ltimately, it is on the basis of an epistemic understanding of the nature of democracy, that Du Bois argues that scientists should be pure truth seekers" (Bright 2018, p.2242). Then, the final aim of science will be epistemic. If it is true, one may even say that Du Bois' main argument is ultimately epistemic. About this matter, Bright himself writes:

> [...] in light of the total corpus of Du Bois' work it is likely that the argument he had in mind here was *epistemic rather than so directly moral*. As already argued, the mediate aim of science, according to Du Bois, is to facilitate sound decision making concerning policy. My claim is that Du Bois thought that scientifically acquired information better serves this goal if it is presented *en masse* rather than filtered via the preferences of scientists. (Bright 2018, p.2240 (emphasis added))

And for the third, suppose the final (mediate) aim of science is ethical or practical, i.e. to retain public trust in science. Then, one may say that the public trust in science may still be retained if scientists intervene those values into their scientific practice that *are endorsed by the public*. John examining the vindication of VFI by appealing to the moral obligation to respect the autonomy of others — as an ethical aim — writes:

> [...] respect for autonomy does not demand that speakers *always* recuse themselves from making non-epistemic value-judgments in communicative contexts. Rather, it places limits on which values they should use; specifically, they should use *values shared by their audience*. (John 2019, p.69 (emphasis added))

But, as Bright states, "Du Bois was willing to make [his] argument against even people whose political projects he agreed with" (Bright 2018, p.2236). Bright continues: "It was not just that Du Bois did not like it when those who did not share his political goals let their political motives shape their reports, he also objected when those he agreed with did the same" (Bright 2018, p.2237). From this we may conclude that for Du Bois, it is not legitimate to enter even those values shared by the public into scientific theorizing. Then, Du Bois should show us how appealing to publicly-favored values in science may be destructive to public trust in science. Otherwise, it seems that Du Bois' seemingly non-epistemic defense of VFI does not affect its status as an epistemic ideal.

The second alternative view of the basis of VFI is John's (2018, 2019), which targets the need for avoiding wishful thinking as the basis of VFI. According to him, wishful thinking is different from *wishful speaking*: one may make an assertion wishfully — since e.g. it has some benefit for her — but does not believe what she asserts, and the vice versa. In his view, the need to avoid wishful thinking may only be justified on the grounds that wishful speaking is ethically illegitimate. Now, the question is what the relation between wishful speaking and VFI amounts to? John concedes that wishful speaking may sometimes have positive non-epistemic (and even epistemic) consequences — though it "is often wrongful in large part because of its [negative] consequences" (John 2019, p.67). Hence, the illegitimacy of wishful speaking depends on the speaker's — scientist's — values. John writes:

> [...] in speaking wishfully, a speaker treats hearers' beliefs as mere means to be manipulated for the sake of ends which the speaker values. Even if these ends are noble, such an attitude is to disrespect hearers' status as autonomous agents. [...] This respect-based consideration implies that wishful speaking is always *pro tanto* wrongful. (John 2019, p.67)

Therefore, wishful speaking may also be justified if those values entangled in scientists' assertions are shared with the audience, especially the policy-makers:

> It may be entirely proper to assert "value-based claims" to a policy-maker who endorses the relevant trade-off [values], but impermissible to communicate the very same claim to a policy-maker who does not. (John 2019, p.69)

So, John introduces an alternative ideal that he thinks is superior to the value-free ideal: value-apt ideal (VAI), that is:

> When we are justifying scientific findings to be communicated to some audience, the justification of those findings should not be based on non-epistemic (e.g. political or moral) values which are incompatible with the values of the putative audience. (John 2019, p.69)

John believes that although VFI and VAI are both acceptable, the former is subject to the objection that it is not viable, while the latter is not. If VFI turns out to be viable, it is in the same direction as the VAI and "a safe way of respecting autonomy" (John 2019, p.69). And if not, VAI is the one to follow: "it [at least] places important limits on the (non-epistemic) values to which scientists may appeal: they must be values the putative audience holds" (John 2019, p.69). Then, actually, John believes that it is VAI that confers justification — an ethical one — on VFI. As a result, one may conclude that the ultimate basis of VFI is ethical.

But, it can be argued that John's ethical defense of VFI does not make the ideal a non-epistemic one. In fact, John does not show that VFI is an ethical ideal, and he introduces VAI as an ethical ideal to which VFI is *in agreement,* without having to consider the latter as a non-epistemic ideal. Suppose, for example, there is a religious ethical system that accidentally corresponds to some philosophical ethical system. These are two different ethical systems with different bases, which relate to two different ethical ideals with different goals. However, following one of them meets the obligations imposed by the other. It should also be mentioned that John's defense of VFI is a "limited" endorsement of the ideal. According to him, we may follow VAI and neglect VFI even if it is viable: "the VAI is an appealing ideal, which can, in practice, provide guidance regardless of whether or not the VFI is viable" (John 2019, p.69). So, for John, they are not necessarily of the same sort.

4. How to Reject the Value-Free Ideal

In the previous section, we argued for the claim that VFI is an epistemic ideal with objectivity as its goal. In this section, we investigate how one can reject the ideal. The feature that the ideal delineates is *value-freeness*: any piece of scientific work, to be excellently objective, should be value-free, at least to a high degree. That is, objectivity is one *epistemic goal* toward which every piece of scientific work should be directed, and to pursue the goal, it should possess a high degree of value-freeness.

VFI can be stated as the following categorical imperative norm in which the goal of objectivity is implicit:

(VFI) *Non-epistemic values should not be given any internal role in scientific practice.*

It seems evident that epistemic values have a central role in the internal stages of science: even the proponents of (VFI) concede that epistemic values are the standards according to which scientific practice should be conducted to reach its goals (see, e.g., Lacey 1999, pp.16–7). So, it seems implicit in (VFI) that epistemic values are separate from non-epistemic values. Therefore, one of the main arguments posed against (VFI), as said above, is the claim that epistemic and non-epistemic values are not separate and cannot be disentangled. However, as said above, it is of the second kind of criticism against the value-free ideal, and Douglas's argument against (VFI), which is our focus here, is of the first kind. Douglas, as well as other opponents of (VFI), reject it and endorse its negation. The negation of (VFI) can be formulated as follows:

(¬VFI) *Non-epistemic values may be given an internal role in scientific practice.*

However, what most of the opponents of (VFI) including Douglas herself claim goes beyond (¬VFI): not only do they claim non-epistemic values *may* be given an internal role in scientific practice, but also they claim that non-epistemic values *should* play an internal role in scientific practice. So, the view opposite to (VFI) that can be called the *value-laden doctrine* can be formulated as the following categorical imperative norm:

(VLD) *Non-epistemic values should be given an internal role in scientific practice.*

Now, any objection of the first kind to (VFI) from (VLD) should fulfill these four requirements:

(1) There are *non-epistemic* values that should be shown to have some role in science.
(2) The role the non-epistemic values play in science should be shown to be at the *internal* stages of science.
(3) The role the non-epistemic values play at the internal stages of science should be shown to be *necessary* for scientific practice.
(4) The role the non-epistemic values necessarily play at the internal stages of science should be shown to be *constitutive*.

The requirements (1) and (2) seem to be obvious regarding (VFI). The requirement (3) seems to be evident in (VLD) due to the modal verb "should" within it.

About the requirement (4), as said above, to undermine (VFI), it should be shown that value-freeness — the feature value-free ideal delineates — cannot direct scientists toward the goal associated with the ideal itself — that is, the epistemic goal of objectivity — or is an obstacle in the way toward another goal of the same category. Therefore, to undermine (VFI), non-epistemic values should be shown to be necessary for pursuing some epistemic goal, and not some practical or ethical goal. That is, they should be shown to be *constitutively* necessary for scientific practice.

It seems to be in need of a bit more detail. Apart from the theoretical limitations of scientific research, especially the one resulting from the problem of *underdetermination*, scientific activity is also constrained by practical and ethical considerations (see Resnik 1998, pp.54–5,118–9; Lemons, Shrader-Frechette, Cranor 1997; Douglas 2009, p.9). So, in many cases, for example, getting to more adequate results needs much more resources, which their allocation to a scientific research is not practically or ethically possible or reasonable — in comparison with more vital problems, or considering moral codes. Due to such limitations, research areas and problems should be prioritized for doing research. Furthermore, even in the areas and problems selected for doing research, there are many constraints that prevent scientists from reaching errorless results. These are practical barriers to scientific research that lead to such issues as the lack of enough evidence. However, such constraints are presumed to be absent in an (imaginary) *epistemically ideal situation* in which reaching the primary goals of science is much more straightforward than our actual situation. To undermine VFI, non-epistemic values should be shown to be necessary for attaining the goals of science, and they should not only be practically or ethically necessary for the scientific enterprise (see de Melo-Martín and Intemann 2016, p.510). This is the case even if, as Douglas (2009, p.95) claims, the value of science itself is socially (and not only epistemically) justified — science to serve the social purposes should after all be at least reliable (Douglas 2009, pp.95–6).

In her undermining (VFI), Douglas mentions the first three requirements when, for example, she writes: "*non-epistemic* values are a *required* part of the *internal* aspects of scientific reasoning for cases where inductive risk includes risk of non-epistemic consequences" (Douglas 2000, p.559 (emphasis added)). However, she seems to neglect the last requirement. What we want here to show is that Douglas's argument against (VFI) does not fulfill this requirement. In the next section, we discuss Douglas's argument.[11]

5. Douglas's Argument from Inductive Risk

To undermine VFI, Douglas draws on the *inductive risk* associated with most scientific research but mainly serious in those areas of science that their results serve as a basis for policy-making — decision-making in public policy. For brevity, we call such areas of science *PM-sciences*, and the research carried out in these areas *PM-research*. According to Douglas (2000, pp.577–8), these are among the most funded areas of science.

Douglas (2000, p.561) defines "inductive risk" as "the chance that one will be wrong in accepting (or rejecting) a scientific hypothesis". There is *always* this chance of error in PM-research, especially where the size of the population under test is relatively small. But, due to the fact that any research, roughly speaking, consists of testing a hypothesis, and the hypothesis may be confirmed or disconfirmed based on the research findings, a PM-research encounters the possibility of two different types of errors: *false positive* (*type I* or α *error*) and *false negative* (*type II* or β *error*). False positive error occurs when the hypothesis under test is confirmed but it is actually false, and false negative error occurs when the hypothesis under test is disconfirmed but it is actually true. Any of these errors in a PM-research findings may lead to some non-epistemic detrimental consequence and scientists naturally try to reduce those errors to prevent their bad consequences. However, due to some financial, temporal, or other practical limitations, as well as methodological constraints, the chance of those errors cannot be simultaneously reduced, and scientists have no choice but to maintain a balance, and try to reduce the more harmful error more than the other. But, because those consequences are not epistemic, Douglas argues, scientists should include non-epistemic values in choosing an error to reduce. And finally, these choices are within the internal stages of science. Douglas' argument is summarized in the following passage:

> In cases where the consequences of making a choice and being wrong are clear, the inductive risk of the choice should be considered by the scientists making the choice. In the cases I discuss below, the consequences of the choices include clear non-epistemic consequences, *requiring* non-epistemic values in the decision-making. Thus, where the weighing of inductive risk requires the consideration of non-epistemic consequences, non-epistemic values have a legitimate role to play in the internal stages of science. [...] non-epistemic values are required for good reasoning (Douglas 2000, p.565 (original emphasis)).

Douglas tries to show that her argument fulfills some requirements similar to the ones we articulated in the form of (1)–(3). Her argument fulfills (1) since it shows that scientists should choose one error to reduce regarding the "reasonably foreseeable" detrimental consequence every error leads to. So, they should weigh different consequences to determine which one is worse, and this is an evaluative task done by appealing to some social, ethical, political, or religious values.

Douglas tries to show that her argument also fulfills (2) and (3). She tries to show that the scientists' evaluative task is an integral part of all the three main parts of any PM-research: choosing (the settings of) the method of the research, characterizing data, and interpreting the gathered data. To show this, she focuses on the case of the studies carried out on the toxicity and carcinogenicity of 2,3,7,8-tetrachlorodibenzo-p-dioxin. We briefly consider her reasons.

First, she argues that in the stage of choosing a research method — in fact, in choosing the settings of a research method — scientists should choose a *statistical significance level* (see Rudner 1953). The level of statistical significance denoted by α is the value selected as a cut-off that if the *p-value* is below which, the gathered data are statistically significant and the *null hypothesis* is rejected. The p-value is the likelihood of the gathered data if the null hypothesis is true. The null hypothesis, H_0, is the hypothesis that the gathered data do not show any significant relationship between the two or more measured quantities, or the quantity measured in two or more different groups of individuals does not show any significant difference between them, or the value of one quantity does not make a difference to the value of the other, and any seeming relationship between them is only due to chance. Then:

$$p = Pr(D \mid H_0),$$

where p denotes the p-value, and D denotes the set of all the gathered data. Now, for the research hypothesis or the alternative hypothesis, H_1, to be statistically significant, we should have:

$$p < \alpha.$$

(See Dawson and Trapp 2004, pp.104–10; Dawson 2008, Chapter 10; Sober 2008, pp.48–54.)

Now, according to Douglas, the more the value of significance level is, the less the false positive and the more the false negative will be (Douglas 2000, p.566; see also Lemons, Shrader-Frechette, Cranor 1997). That's right. The probability of false positive and false negative errors is calculated as follows:

$$FP = Pr(p < \alpha \mid H_0)$$

$$FN = Pr(p \geq \alpha \mid H_0)$$

where FP denotes the probability of false positive, and FN denotes the probability of false negative. According to the above-mentioned definition of the p-value, decreasing α results in decreasing FP and increasing FN, and the vice versa. So, according to Douglas, the scientists should select a level of significance with regard to the consequences of false negative and false positive errors. And since determining a significance level is an internal part of a scientific research, non-epistemic values play a role in the internal stage of science and (2) is fulfilled. It also makes (3) satisfied: a scientist (and not another person like a policy maker), according to Douglas, *should* choose a significance level, and it is necessary for her scientific activity.

Second, Douglas argues that in the phase of data/evidence characterization, scientists need (and should appeal to) contextual values. For example, in the case of dioxin studies, Douglas mentions the fact that different scientists in different studies did not agree with each other whether the female rat liver slides — gathered from the animals in three dosed groups and one control group — being malignant, benign, or not tumorous at all, and even in a reevaluation of those slides in 1990, the scientists collaborating on the research "resorted to majority voting to reach an opinion about the slides" (Douglas 2000, p.570). Thus Douglas argues that: "That the pathologists resorted to voting indicates that there is still a significant degree of judgment required in the evaluation of the rat liver slides" (Douglas 2000, pp.570–1). Those scientists had different opinions about diagnosing cancer in the borderline cases, and this difference depended on different contextual values they believed. This role of non-epistemic contextual values in science is apparently internal to scientific activity. It also seems to be necessary since the results of the available tests for different kinds of cancer were not (and still are not) decisive, especially in borderline cases.

Third, Douglas insists that scientists should make non-epistemic value judgments to make a choice of an interpretation approach between the *threshold model* and the *likelihood curve model*. Adopting any of the interpretation approaches results in a (sometimes radically) different result of the research. The situation becomes even worse when the size of the population under test is rather small: if there is a low threshold, it cannot be detected in such studies (Douglas 2000, pp.575–6). Adopting an interpretation approach is a necessary and internal part of science. For one would not be able to continue to interpret the evidence without adopting a specific interpretation approach.

Up to this point, Douglas' argument for (VLD) and against (VFI) seems to be apparently convincing. Nevertheless, the argument is deficient in at least two ways. The first criticism that can be levelled at Douglas' argument is that it cannot get off the ground unless one can vindicate the epistemic/non-epistemic value distinction, which is criticized by Douglas herself (Machamer and Douglas 1998; Douglas 2009, pp.89–91; see also de Melo-Martín and Intemann 2016; Steel 2010). She tries to address this problem giving an account of how to distinguish between the legitimate and illegitimate roles of values in science (see Douglas 2009, Chapter 5). However, we will not go through this line of argument here. Second, it cannot fulfill the requirement (4). Showing this is the subject of the next section.

6. The Necessity of Non-Epistemic Values for Science

As said above, there are many practical and ethical limitations that make science fall short of the value-free ideal —for example, due to lack of enough evidence. Such limitations force scientists to make non-epistemic value judgments in their scientific works so that the value judgments seems to be necessary for doing science. But, as far as this necessity is a practical one, it has nothing to do with the value-free ideal. Douglas seems to implicitly assent this fact when she writes: "If we find new evidence, which reduces the uncertainties, the importance of the relevant [non-epistemic] value(s) diminishes" (2009, p.97), and the main obstacles in the way of obtaining more evidence are practical and ethical ones. So, we seem to be obliged to do value judgments because of many practical and ethical limitations scientists encounter, and if they overcame those limitations, they would be free from the value judgments (de Melo-Martín and Intemann 2016, p.510). About the ethical issues, she also says: "Even if the research goes exactly as planned, and the scientists intend the best, moral concerns may trump epistemic drives" (Douglas 2009, p.72; see also pp.75–7). And it seems that what Douglas shows us through her argument is likewise that on many occasions, our epistemic ideals are trumped by the moral, political, or economic ideals, and this fact is not a threat to the status of the value-free ideal as an epistemic ideal, since it is in fact a second kind of objection against the ideal.[12] In fact, any two ideals — epistemic, ethical, political, cultural, or another type — may be in conflict in a situation, and stakeholders should prioritize the one that is more *important* in that situation. But, as said above, it doesn't undermine the less important ideal *in general* (Brownlee 2010, pp.439–40). For example, it is epistemically ideal to do dangerous experiments on human subjects — rather than laboratory animals — to study a new treatment of a disease, or to study the effect of different doses of a toxic substance on human health. But, it is ethically illegitimate, and ethical issues are usually more important to the society (see Hicks 2018; Douglas 2009, Chapter 4-5). However, this fact does not threat the status of that epistemic ideal as an ideal.

Douglas apparently takes into account the requirement (4) when saying that: "In these cases [i.e. the PM-researches], value-free science is *inadequate* science" (Douglas 2000, p.559 (emphasis added)), and therefore she claims, the ideal is "undesirable" and "bad" (Douglas 2009, pp.13–4, p.87). It means that if we make PM-science value-free, it will become "inadequate" and then, non-epistemic values are necessary for "adequate" PM-science. "Inadequate" science, whatever it may mean, seems to be a kind of science that cannot direct scientists and/or society toward some goal(s) of science. But, as said above, Douglas' argument could be said to fulfill the requirement (4) only if she would show that at least one goal of science that value-freeness is an obstacle in the way toward which is epistemic. Douglas herself pays attention to this point. She says that "non-epistemic values are required for good reasoning" (Douglas 2000, p.565; see also Douglas 2009, p.85 and passim). However, it doesn't seem to be the case in Douglas' argument. To show this, we discuss all of the three parts of science she mentioned that non-epistemic values play a role in.

First, why are non-epistemic values necessary for selecting the level of significance? As said above, according to Douglas, it is because the level of significance is selected regarding how disastrous the consequences of inductive risk errors are. But, it does not seem constitutively necessary for scientists to make value judgments. For if we could, for example, dramatically increase the population size in a PM-research, then choosing a proper value for the level of significance to reduce one type of error would lose its importance. And these are financial, temporal, and other practical limitations that prevent scientists from having a large population for their study (Lemons, Shrader-Frechette and Cranor 1997). And as said above, these practical limitations have nothing to do with the value-free ideal. Douglas herself mentions this point when she says: "In order to reduce both types of error, one must devise a new, more accurate experimental test (such as increasing the population size examined or developing a new technique for collecting data)" (Douglas 2000, p.566). She also says that:

> Reducing the possibility of any error by increasing the power of the study

> would help mitigate the dilemma here, but doing so is *extremely difficult*. For example, one way to reduce the risk of both false positives and false negatives is to increase the animal populations under study. Currently, most studies use 50-100 animals in each dose group. Increasing those numbers would decrease the chance of false positives and false negatives. However, it is *extremely expensive and difficult* to do larger studies. (Douglas 2000, pp.568–9 (emphasis added); see also 2009, p.104; Lemons, Shrader-Frechette and Cranor 1997.)

Being “extremely expensive” and “extremely difficult” are practical (and not epistemic) limitations in doing science. Then, non-epistemic values are necessary to deal with the problems that lie in these practical (and also ethical) constraints to reduce their worst consequence. However, it is practically or morally (and not constitutively) necessary to take account of these limitations. Therefore, if we were in an epistemically ideal situation, we would not probably need non-epistemic values for our scientific practice. Douglas herself points to this fact. At the conclusion of her 2000 paper, she says:

> When there is very low uncertainty, such that a scientist believes there is virtually no chance of being wrong, there is little gained by considering the consequences of being wrong — the chance of error is so small that consequences of being wrong become insignificant. (Douglas 2000, p.577)

Second, why, in the case of dioxin studies, were the non-epistemic values necessary for diagnosing the female rat liver slides as being malignant, benign, or not tumorous at all? As said above, Douglas’s reason for the claim was that “a significant degree of judgment required in the evaluation of the rat liver slides”, since scientists disagree about this matter and had no choice but to vote on it. However, the 1990 study (reported in Goodman and Sauer 1992) was done as a reevaluation of those slides according to the *refined* “criteria for the diagnosis of proliferative hepatocellular lesions in the rat”, which had been introduced *after* the previous evaluations on those slides (Goodman and Sauer 1992, p.246). Each slide was evaluated by seven expert pathologists and their “final opinions were recorded”. If at least four pathologists agreed on a result, it would be counted as a *consensus*. But, it is not clear whether the diagnosis was affected by non-epistemic values of the pathologists. Goodman and Sauer (1992) described in detail the observations of the rat lesions and the refined criteria according to which, they categorized the observed lesions. So, it seems the criteria were shared among the pathologists, and what may cause them to disagree was probably what they observed. Therefore, the voting system was probably designed to avoid the probable human errors in borderline cases. If so, this study seems to have more reliable method than the two previous evaluations of the slides.

On the other hand, some *practical* limitations, most notably the time of treatment, placed some constraints on those researches. For example, if the female rats were undergone the treatment more than two years, the evaluations would probably lead to more non-equivocal outcomes. But, as Douglas emphasized, not only does it need more money, but also it will prolong the process of regulation that, in turn, may cause harms both to society health and its economy. Actually, *early diagnosis* of an individual with nearly any kind of cancer is still a problem. Virtually, for any kind of cancer, a lot of research has been carried out to reach a reliable diagnostic test, and it seems a lot of research is still to be done to find such a test or to improve the effectiveness of the existent tests for any kind of cancer. However, due to the fact that these are practical limitations, they do not have anything to do with VFI.

Another matter, mentioned by Douglas (2000, p.571), is that the diagnosis of cancer in the female rat livers, has, like any other diagnostic test, its own inductive risk as false positive and false negative errors. Then, according to the non-epistemic consequences of those errors, scientists should decide to prevent one error more than the other. We discuss this matter a bit more.

Every diagnostic test has its own false positive and false negative errors calculated as follows:

$$fp = Pr(T^+|D^-)$$
$$fn = Pr(T^-|D^+)$$

where T^+ denotes a positive test result, T^- denotes a negative test result, fp denotes the probability of false positive error, fn denotes the probability of false negative error, D^+ means the individual under test actually contracted the disease, and D^- means that the individual does not actually contracted the disease. Medical researchers naturally try to reduce both of these errors. They evaluate every diagnostic test using two parameters: *sensitivity* or *true positive*, and *specificity* or *true negative*:

$$tp = Pr(T^+|D^+) = 1 - fn$$
$$tn = Pr(T^-|D^-) = 1 - fp$$

where tp and tn denote sensitivity and specificity of the test respectively. Then, medical scientists continuously try to increase sensitivity and specificity of diagnostic tests by reducing the false negative and false positive errors. However, due to *practical limitations*, they sometimes could not reach a diagnostic test with high enough sensitivity and specificity. So, they have no choice but to reduce the error that has less harmful consequences. But, this is a practical necessity that, again, has nothing to do with an epistemic ideal. And what if, one may ask, there is no diagnostic test with a decisive result at all? (see Douglas 2000, p.574) It is disappointing, but not epistemically a problem. After all, we would know that cancer cannot be decisively diagnosed, especially at an early time!

Third, why is the selection of a model for interpreting the gathered data, especially in deciding whether there is a threshold, for example, for the carcinogenic effects of dioxin, necessarily dependent on the non-epistemic values? About toxins, Douglas mentions this basic assumption of toxicology that "there is always a threshold for toxic effects" (2000, p.574). However, about mutagenic chemicals as well as radiation, Douglas accepts the assumption that "there is no safe dose or threshold for carcinogenic effects" (2000, p.574). Dioxin is neither a mutagen nor a toxin. It is a *promoter*, and it is not clear which assumption should be made about its carcinogenic effects. This implies that one should choose between these models of interpretation, and in doing so, one should take into account the risk of following any model, drawing on non-epistemic values. She says: "An acceptable dose is determined not by a threshold but by what risk one is willing to take" (2000, p.574). But, don't the gathered data suggest a specific interpretation approach? For example, if the carcinogenic effect of dioxin *under* a specific dose is not so different from the control group, doesn't it imply that that dose can be regarded as a threshold for the carcinogenic effect of dioxin? Douglas' response is that it doesn't, since the threshold should be the *highest* dose that produces cancer less than 1% of the dosed rats in comparison to the control group. However, it cannot be observed in a study with the population size of 50 female rats, because one percent of this population does not include even one rat. Therefore, Douglas concludes: "If [...] the threshold is a low probability threshold, we won't be able to detect this with such a study. If there is no threshold, we won't be able to detect this with such a study either". (Douglas 2000, pp.575–6).

However, this is a result of the practical limitations that are naturally placed on scientific activity. Again, it has nothing to do with an epistemic ideal. It means that, scientists would be epistemically in a better situation if they could carry out their research, for example, on toxicity of dioxin, with a population size of 10000 or more female rats: if they were able to increase the population size to this amount, they would find out whether there was a threshold, and they could determine the threshold, if there was any. Besides, if they were allowed to carry out the experiment on human subjects — an obviously unethical and illegal act — they would achieve more accurate results. So, VFI is still an ideal from an epistemic point of view, though it is not an ideal from economic, ethical, or social point of view, and may be in conflict with economic, ethical, or social ideals.

At the end of this section, we add another point. Douglas believes that the proponents of VFI have this presupposition that science should be insulated from the society (Douglas 2009, Chapter 3). About this matter, it can be said that science, epistemically thought, *should be* insulated from the society, though it morally (and also practically, politically, and socially) *should not be* insulated from the society.

7. Pragmatic Considerations in Science: An Objection

A possible objection against VFI, as an epistemic ideal, is that pragmatic considerations and necessities are so woven into the fabric of science — and its methodology — that it is impossible to do science without taking them into account and think about epistemic considerations and necessities in isolation.[13] In particular, C. West Churchman (1948a, 1956) seems to have argued in such a direction. In an analogy, for example, he compares the acceptance of a hypothesis with accepting an artifact as a "good" product. In both cases, there is a "decision" we have to make, and to make such a decision, three things need to be done. In the case of an artifact, we should do "(1) a 'market' survey of the demand for products, (2) specification of product lines in terms of the most important of these demands, and (3) consumer education", and in the case of a hypothesis, "(1) a 'market' survey of the demands for information, (2) specification of hypotheses in terms of these demands, and (3) consumer education" (Churchman 1956, p.248; see also Churchman 1948b, Chapter XVI). Such a comparison shows the importance of pragmatic considerations in doing science.

Churchman also sees the decision on whether we have enough information and empirical observations at a specific time to choose a hypothesis — or we still need to gather more information — as a contingent matter upon pragmatic considerations. The aims we have in mind of scientific activity, and of a particular hypothesis, and what relative weight we attribute to them are crucial. As Churchman (1956, p.247) puts it, "the criteria of optimal decisions for the type and number of observations, the conceptual framework, etc., depend (I think) on the ultimate aims of scientific activity. More specifically, they depend on the relative values of these aims". This also shows that by changing the relative weight of the aims, we may reconsider our previous decisions as to whether or not the available empirical observations are enough. And in a more telling case, Churchman introduces the concept of "partial confirmation", stating that:

> It is not observation (sensation) alone which supplies the sound basis for accepting hypotheses. Further, the experimenter is obliged to make a certain selection of the data that are producible by himself or the environment, and this selection is itself an aspect of his method. A physicist testing a general theory may examine only one type of consequence of the theory, and decide to accept the whole if he accepts this consequence. Such "partial confirmation" is a practical necessity of experimentation, and the manner in which it is applied actually constitutes a description of the method. (Churchman 1948a, p.260)

What is common in all these passages is the point that accepting a scientific hypothesis is a kind of "decision", which is inevitably intertwined with pragmatic considerations. We, however, believe that the presence of such pragmatic considerations in accepting a hypothesis or deciding on the adequacy of existing empirical observations etc. does not impair VFI as an epistemic ideal that serves to achieve objectivity. What Churchman says about the pragmatic considerations in hypothesis selection can be divided into two main categories. First, in hypothesis selection, scientists usually face "practical" limitations due to the non-ideal situation we humans are all in, which would be absent in an epistemically ideal situation — the ideal seems to amount to the one Churchman calls "ideal of an errorless measurement" (Churchman 1948b, p.267). For example, Churchman mentions that in the absence of enough information, scientists must decide at a certain point whether the available information is enough to support a hypothesis (or they need to continue gathering more information), and such a case would be disappeared in an epistemically ideal situation in which nearly all the necessary information is available. More specifically, Churchman (1948b, p.255) himself concedes that sample size is related to the "cost" of a science-based decision. And it is shown that "the *p-value* tends to zero when the sample size tends to infinity" (Gómez-de-Mariscal et al. 2021 (original emphasis)). Therefore, in an epistemically ideal situation in which the sample size tends to infinity and the p-value tends to zero, scientists would not need to set a statistical significance level and "decide" whether the observations are enough. Churchman also points out the possibility that a scientist may decide to consider the whole hypothesis as confirmed by partially testing only one type of its consequences. Again, it seems evident that in an epistemically ideal situation in which all types of consequences of the hypothesis are available to scientists, that limitation would also disappear. On this matter, Churchman also writes:

> [...] the ideal of an errorless measurement could only be approached by taking observations in indefinitely increasing number, and that there was a constant demand for the experimenter to decide whether the ideal was being approached satisfactorily or not, i.e., whether the observations are or are not "in control". (Churchman 1948b, p.267)

For this reason, such limitations can be called "non-strictly pragmatic", since in an epistemically ideal situation, they would be disappeared. Then, as far as Churchman's claim is about non-strictly pragmatic limitations, there is no need to modify our idea of VFI here since it is an epistemic ideal that would lead us to objective knowledge in an epistemically ideal situation.

Churchman's second category of pragmatic considerations — that may be called "strictly pragmatic" considerations — however are those that exist even in an epistemically ideal situation. As said above, he proposes that selecting any hypothesis is a kind of "decision" made by reference to the supposed values and aims of doing science (in general) and accepting a specific hypothesis (in particular). It means that these aims and values and their relative weights are effective in decision-making, and different aims and values or their different relative weights might lead scientists to make different decisions. And an important point here is that even in an epistemically ideal situation, these aims and values are present, and that situation does not make them idle. Then, it may be claimed that strictly pragmatic considerations put VFI into a challenge and make it lose its status as an epistemic ideal whose aim is to achieve objectivity and avoid wishful thinking. The reason may go like this: by taking strictly pragmatic considerations seriously, truth-seeking either is out of the list of aims and values of science and hypothesis selection, or it is not the "only" item on the list. As a result, there is no room for objectivity, which is supposed to guarantee the avoidance of wishful thinking on the truth-seeking path. In short, because objectivity is removed from the list of aims and values of doing science and hypothesis selection, VFI, which was supposed to bring us to objective knowledge, will also lose its status as an epistemic ideal.

Now, if we consider the ultimate aim of doing science to be reaching the truth, ultimate objectivity guarantees not to fall into wishful thinking on this path. However, objectivity can be thought of in an instrumental way, and science or hypothesis selection may be done towards other aims and values than truth-seeking. These aims and values might be contingent, local, sensitive to scientists' (or even consumers') preferences, and may change over time. However, it seems scientists need to avoid wishful thinking to be successful in reaching those aims. For according to the aims and values, they determine a set of criteria for hypothesis selection. Then, it seems they want to select a particular hypothesis using those criteria to lead them to the aims in an "objective" way, without being engaged in wishful thinking and interfering with other unrelated aims and values. Suppose, for example, that a feminist scientist's aim *A* in selecting her hypothesis — among some existing ones — about how to understand people's sexual preferences is that the individual's autonomy in manifesting her/his sexual preferences must remain preserved — in a way that her/his sexual preferences are not merely functions of her/his biological structures. Based on aim *A*, the hypothesis selection criterion *C* might be that the selected hypothesis should not be deterministic regarding sexual preferences. Suppose, among the existing hypotheses, some hypothesis *H* has been selected according to criterion *C*. Here, instrumental objectivity means that the scientist wants to ensure that no aim and value other than *A* involved in selecting *H* and that she did not fall into wishful thinking in selecting this hypothesis. In other words, it is still legitimate to ask whether selecting *H* is (instrumentally) "objective" regarding aim *A*, which in this case is to recognize the individual's autonomy in her sexual preferences.

The main argument of this paper can be expanded in response to Churchman's points about strictly pragmatic considerations. Wherever we talk about "objectivity", be it ultimate or instrumental, VFI may be regarded as a kind of epistemic ideal. It does not matter if we consider the aim of doing science to be truth-seeking or to reach a set of other alternative aims and values. As long as we approach an aim and value, it seems necessary to have a guarantee to avoid deviation by wishful thinking. Such assurance may be called "objectivity" (ultimate or instrumental), and VFI is a means to achieve this. As a result, even within Churchman's pragmatic framework, VFI may still play its role as an epistemic ideal to avoid wishful thinking.

8. The Decisions Scientists Should Make and the Ones They Should Not

Up to this point, if successful, we showed that what Douglas takes to be an argument against VFI, i.e. the necessary roles of non-epistemic values in the internal stages of science, cannot undermine VFI since it merely draws on the practical or ethical limitations naturally imposed on scientific activity that lead to the necessity of the non-epistemic values for science, and she does not show that those values are constitutively necessary for science.

Now, we want to answer the question that if non-epistemic values are not constitutively necessary for science, should scientists *qua scientists* do non-epistemic value judgments? Following Rudner (1953), Douglas' response is positive. It seems that Douglas' argument has this implicit premise that every scientist *herself* should decide which error to reduce regarding the bad consequences different types of errors have, if such a decision is internal to scientific activity. Here, we want to argue that although such decisions are internal to scientific activity, they should not be made by scientists qua scientists.

It can be said that it is not the scientist who should do non-epistemic value judgments. Even if a scientist makes value judgments in her scientific work, she assumes a different role from the one she plays when, for example, doing an experiment to test a hypothesis. It is important to distinguish her two different roles (Mitchell 2004).[14] Although Douglas (2009, p.67) is right in thinking that "scientists are generally capable moral agents", it is not correct to think that this is a sufficient condition for a person to make "complex" non-epistemic value judgments in a scientific research project (see de Melo-Martín and Intemann 2016, p.508).[15] Then, it seems to be the task of someone else who specializes in value analysis, value judgment, and weighing negative consequences: an ethical advisor or consultant. In this way, scientists do their epistemic work — e.g. assigning probabilities to scientific hypotheses (Jeffrey 1956) — and do not worry about such *non-epistemic* issues anymore.

Douglas herself responds to this objection. According to her, saying that the responsibility for weighing the negative consequences of different errors lies with someone other than the scientists, e.g. a policy maker, means that that person should make many *scientific* choices in scientists' place, which are internal to scientific activity. This fact, Douglas claims, restricts the scientists' freedom to make scientific decisions, and may lead to scientists being irresponsible and not caring about the consequences of the research findings errors, and also to the members of the research team being annoyed with each other. Besides, Douglas argues, the person who can best evaluate the negative consequences of the errors is the scientist herself. (See Douglas 2009, pp.73–4.)

However, a scientist is to have *educated* and *trained* to device hypotheses and theories to explain and predict some natural phenomena (according to her area of research), and to do experiments to test those hypotheses. So, roughly speaking, scientists are expected to reach some empirically adequate hypotheses, and some *nearly true* explanations and *nearly correct* predictions concerning the phenomena under study. She *qua scientist* actually does not have the relevant specialty to analyze values (see Hudson 2016, pp.188–90). We believe that someone who is expert in non-epistemic value judgment and policy making should be *joined* to the scientific team and her contribution should be part of the research outcome going to be published; i.e. an ethical consultant. Nevertheless, she should work as a team member collaborating with other members of the team, and her collaboration is after all internal to scientific activity (see de Melo-Martín and Intemann 2016, p.513). It means that a scientific team *as a whole* should be responsible to the society, and for that, all members of a scientific team need not be responsible to the society *in the same way*. Accordingly, the requirement of "constant ethical oversight of all scientific practice" mentioned by Douglas (2009, p.74) will be fulfilled, and, at the same time, the autonomy of the scientists who supervise research projects will be secured, and the burden of responsibility for the research consequences will be shared by all members of the team including both the supervisor and the ethical consultant, though not in the same area and not to the same degree. As Douglas herself notes, a scientist qua scientist should take the responsibilities "special to science", the ones Douglas calls *role responsibilities*: "those that assist scientists in achieving the central goals of science" (Douglas 2009, p.72). Nevertheless, as Douglas (2009, p.73) concedes, they do not include such *general responsibilities* as choosing the riskier error to reduce more. So, it is not to exempt scientists from their general responsibilities in their work. Rather, it is a kind of *division of labor* and, as a result, a sharing of responsibility.[16]

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Notes

[1]"Epistemic" value, sometimes it is argued, does not essentially express the same concept as "constitutive" or "cognitive" value (and the same goes for "non-epistemic", "contextual", and "non-cognitive" values) (see, e.g., Douglas 2009, pp.93–4; Douglas 2017). Here, for the sake of simplicity, we regard them as though they are the same.

[2]Rescher (1987, Chapter 5) and Coady (2008, pp.51–6) go so far as to regard ideals as "inherently unrealistic" or absolutely "unrealizable". For a criticism, see Brownlee 2010.

[3]There is also another important argument against VFI. The argument aimed to undermine the *fact-value dichotomy* by focusing on the evaluative concepts that are present in different areas of science (see, e.g., de Melo-Martín and Intemann 2016). When philosophers of science pose such an argument against VFI, they mainly focus on the *product* or the *content* of science as knowledge of nature, rather than the *practice* or *method* of science mainly emphasized here (see Longino 1987).

[4]There were other similar works to which Douglas did not refer. See, e.g., Lemons, Shrader-Frechette and Cranor 1997.

[5]Any ideal, according to Rosati (1998), is either substantive or deliberative. "Deliberative ideals present models of excellent deliberation, leading to correct or warranted [...] conclusions". They

> specify optimal conditions for reflection on ethical questions. They form the basis of broadly counterfactual accounts of moral and nonmoral value, most notably, in 'ideal observer' theories and contractarian theories. (Rosati 1998)

It seems quite evident that what is usually meant by an "ideal" when discussing the value-free ideal is not a deliberative ideal.

[6]There are also other characterizations of ideals. See Van der Burg 1997 for an alternative view. See Rescher 1987, Chapter 5 for a quite similar view.

[7]It should be mentioned that here it is not claimed that an ideal amounts to a goal. Rather, we characterize an ideal as being *associated with* a goal. For a brief discussion about the difference between an ideal and a goal, see Brownlee 2010, p. 439-41. About the relation between ideals and goals, see Coady 2008, pp.51–3.

[8]Again, we do not claim that ideals simply amount to norms. See Van der Burg 1997 for a discussion about the relation between ideals and norms — or ideals and principles in Van der Burg's words.

[9]Two different ideals of the same category may also be in conflict. For example, one may show the ethical ideal of a just society to be in conflict with the ethical ideal of sainthood. Such situations, we think, should be treated as similar to the cases of two conflicting ideals of different categories described above.

[10]We thank an anonymous referee of *Principia* for informing us about the alternative views and their proponents.

[11]It may seem that de Melo-Martín and Intemann (2016) try to make a case for the same position. However, what they argue for is the claim that there is an assumption common to

VFI and the inductive risk argument: that contextual values do not have a role in determining what evidence may be; rather, contextual values can only affect how strong a piece of evidence should be to support a scientific result. On the contrary, they try to show that contextual values can play an evidential role in science. What we try to argue here is that the fact that contextual (non-epistemic) values play an internal role in science is not enough to undermine VFI — since it is an epistemic ideal. Besides, one should also show that contextual values have a constitutive role in science, and the inductive risk argument does not perform the task.

[12]It should be mentioned that it is not true that all the instances of non-epistemic value judgments in scientific practice are due to some practical or ethical limitations. For example, as Wajcman (1991, Chapter 3) illustrates, the research carried out in reproductive technology has been strongly influenced by masculine values. So, social, cultural, or religious values may also place constraint on our scientific practice. However, in an epistemically ideal situation, *any* kind of non-epistemic constraint is expected to be diminished or at least reduced to the lowest possible degree.

[13]We thank an anonymous referee of *Principia* who brought this objection to our attention.

[14]Sandra Mitchell (2004, p.251) describes her idea in this way: "My thesis is that the norms that should govern the actors in science policy are multiple and can be distinguished in terms of 'role obligations'." And in defending the value-free ideal, she concludes: "What counts as sufficient evidence for scientific assertability is generally much higher than what policymakers require for action" (Mitchell 2004, p.253).

[15]Douglas' case of Diethylstilbestrol (DES) illustrates the complexity of many of value judgments (see Douglas 2009, pp.108–12).

[16]Our proposal for the role of scientists in policy making seems to be in harmony with the proposals put forward by Betz (2013) and Steele (2012), and even with those that may be deduced from Jeffrey 1956 and Levi 1960.

Acknowledgments

The research project that led to this article was funded by Iran National Science Foundation (INSF-95838651). We would like to thank two anonymous referees of Principia for their valuable comments on two earlier drafts of this paper.