Why We Should Not Reject the Value-Free Ideal of Science

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1. Introduction
In recent years, the value-freeness of science has come under extensive critique. Early objectors to the notion of value-free science can be found in Rudner (1953) and Churchman (1956), later objections occur in Leach (1968) and Gaa (1977), and more recent critics are Kitcher (2001), Douglas (2009), and Elliott (2011). The goal of this paper is to examine and critique two arguments opposed to the notion of a value-free science. The first argument, the uncertainty argument, cites the endemic uncertainty of science and concludes that values are needed to give direction to scientific investigation. The second, or moral argument, cites the fact that scientists have moral obligations just like everyone else, and concludes that the decision to accept a scientific conclusion incorporates values by taking these moral obligations into consideration. My goal is to undermine these arguments, thus removing significant opposition to accepting the value-freeness of science.

2. The Uncertainty Argument
The uncertainty argument for the (moral) value-ladenness of science draws from Richard Rudner’s famous argument in his (1953), which Issac Levi (1960) succinctly formulates as follows:

(1) The scientist qua scientist accepts or rejects hypotheses.
(2) No amount of evidence ever completely confirms or disconfirms any (empirical) hypothesis but only renders it more or less probable.
(3) As a consequence of (1) and (2), the scientist must decide how high the probability of a hypothesis relative to the evidence must be before he is warranted in accepting it.
The decision required in (3) is a function of how important it will be if a mistake is made in accepting or rejecting a hypothesis. (1960, p. 347).

Thus, scientists in their capacity as scientists must make value judgments. In Rudner’s original argument, the phrase “in the typically ethical sense” is inserted after “important” in (1953, p. 4). Levi drops this phrase in his reconstruction of Rudner’s argument and so his version of Rudner’s argument is more general, citing the need to invoke values of some kind, not necessarily moral (i.e., social, cognitive, and so on), to address the uncertainty inherent in confirmation. As I discuss later, the more specific, moral argument for the value-ladenness of science proceeds independently of the issue of whether confirmation is uncertain.

There are various ways in which one can respond to the uncertainty argument. To begin with, one can question premise (1), as Richard Jeffrey (1956) does. For Jeffrey, instead of saying that scientists accept or reject hypotheses, “the activity proper to the scientist is the assignment of probabilities [to these hypotheses] (with respect to currently available evidence)” (1956, p. 237). This position James Gaa calls “minimalism” (1977, p. 519): scientists simply determine the degree to which scientific hypotheses are confirmed on the basis of the evidence, and do not go further in either accepting or rejecting these hypotheses. They simply pass along these degrees of confirmation to decision-makers who, using these probabilities, arrive at decisions regarding appropriate modes of action. This approach has been criticized for not recognizing that scientists in fact regularly accept or reject hypotheses (a point made by Gaa 1977, p. 519, and by many others). Additionally, Rudner notes that the probability assignments which stand for the degrees to which hypotheses are confirmed are themselves the subjects of (meta-) hypotheses, a worry that Jeffrey asserts cannot easily be answered (1956, p. 246; see also Heather Douglas 2009, p. 54). In response to these criticisms, it is to be admitted that scientists are committed to many hypotheses in their work, and that often these commitments are based on valuational considerations, such as advocating a hypothesis because the scientific community supports it, or because one wants to get along with others or because it is a hypothesis that attracts a great deal of research funding. However, nothing in the thesis of scientific value-freedom conflicts with these obvious truths. Value-freedom, rather, is a normative ideal that scientists strive to attain: whereas a hypothesis may contingently be held for valuational reasons, it is epistemically preferable if it is held for evidential reasons, and we expect scientists in their work to strive to achieve this ideal. Such an expectation would hold true even with meta-hypotheses about the degrees of confirmation possessed by hypotheses, should it be the case that such meta-hypotheses
are actually needed (e.g., Betz 2013, pp. 214–15, argues that such meta-hypotheses may not be needed so long as the first-order hypotheses under consideration are sufficiently “hedged”).

Of course, by premise (2), there are limits to what one may expect from evidence in terms of establishing a hypothesis beyond doubt, and it is because of this epistemic gap that, on the views of Rudner, Gaa, Douglas, and many others, valutational considerations have an impact on the acceptance of hypotheses. Let us then look in more detail at how one might infer the value-ladenness of scientific conclusions from the uncertainty of evidential support for hypotheses.

3. From Uncertainty to Value-Ladenness

In Rudner’s argument for the value-ladenness of science, it is claimed by Rudner that to bridge the confirmatory gap raised by the uncertainty of science (expressed in premise 2) one must take into consideration “how important it will be if a mistake is made” if one or other hypothesis is accepted (premise 4), with the implication that measuring such importance depends on scientists’ values. To illustrate this dependence, Phyllis Rooney (1992) cites Helen Longino’s (1990) discussion of the comparative empirical support for two competing explanations of physical and cognitive human behavior, the linear-hormonal model and the selectionist model. Given the empirical equivalence of these explanations, Rooney following Longino suggests that the decision between them is made on the basis of “the political and social interests served by … reinforcing [the] gender dimension that the linear-hormonal model supports” (Rooney 1992, p. 18). In short, it is because some scientist values one or other explanatory model that the empirical indeterminacy is resolved. To take a different, familiar case, Thomas Kuhn cites the choice between the geocentric and heliocentric planetary systems at the time of Copernicus which was based on a preference for either external consistency with other established theories (which favored geocentrism) or simplicity (which favored heliocentrism, with its abandonment of epicycles; see Kuhn 1977, pp. 323–4). That is, it is how much one values either external consistency or alternatively simplicity that guides astronomers in their support for either geocentrism or heliocentrism. In general, the perspective of the proponents of value-laden science—here, Rudner, Longino, Rooney and Kuhn—is to assert that it is the valuing of one background theory over another that ultimately resolves the empirical indeterminacy found in the cases they are working with, and by extension in the multitude of other cases that involve a similar indeterminacy.

Yet, contra Rudner et al., it is misleading to say that it is the valuing of background theories that is making the difference here. What is making a difference is what theories or assumptions the participant scientists take to
be true. Suppose, in the case Rudner examines in his (1953) paper, the scientist in arriving at a decision about the safety of a drug becomes aware of two facts: the drug is meant for children and governmental safety regulation requires that the incidence of an adverse side-effect of a drug when administered to children must be below a certain pre-set level. Moreover, let’s suppose that in considering the data and the two stated facts, the scientist accepts the hypothesis that the drug is in fact safe for children. Does it matter here whether the scientist thinks a mistake is important, whether she values children or any relevant governmental policy? She could examine the situation in a completely impersonal manner, one that attends simply to the facts of the situation, and arrive at her decision on that basis alone. In short, she might reason, safety is defined by the government regulation; the drug is safe on this criterion, so the hypothesis that it is safe is to be accepted. (Of course one might suggest that she should value the lives of children and relevant governmental policy, but that is a different argument, the moral argument to be examined below). The irrelevance of values is even clearer in the two other cases we are using. Suppose a scientist in explaining physical and cognitive human behavior simply assumes gender dimorphism to be a fact. There need not be any valuational attitude attached to this assumption. She just believes it. Then in weighing the evidence between the linear-hormonal model and the selectionist model she finds the former model to be more acceptable on the basis of this assumption. We find a similar situation with the choice between geocentrism and heliocentrism. A geocentrist may not have any care whatsoever about external consistency as a theoretical requirement, and care not at all about the ancillary theories with which any planetary model must be compatible. Still, he may maintain that the adequacy of a theory is impacted by its consistency with other true theories. By comparison, it would be quite unusual and completely invalid for a scientist (or anyone, for that matter) to argue that one should accept geocentrism on the basis of the available evidence because one values external consistency or values certain other theories. Similarly, it would fallacious to argue that one should accept heliocentrism because one values simplicity or that one should accept the linear-hormonal model because it is important that gender is dimorphic, or that a drug is dangerous because one cares about children. Simply put, arguments for factual conclusions on the basis of valuational assumptions are non sequiturs.

Thus, the first sort of argument we are looking at which infers the value-ladenness of science based on the uncertainty of scientific evidence contains a flawed premise: it is not necessarily the case that the decision about how high the probability of a hypothesis must be relative to the evidence if one is to accept a hypothesis is a function of how important
it will be if a mistake is made in this acceptance. Rather, a scientifically cogent approach is to motivate such a decision on the basis of which ancillary hypotheses one takes to be true. Some of these hypotheses will be statements about the nature of the current policy environment, if the decision needs to be guided by social policy (such as in Rudner’s drug testing example). But where the moral, social or political implications of the decision are irrelevant, or are simply ignored, the ancillary assumptions will be of a logical, empirical or theoretical nature that compels a conclusion because of their assumed truth, independently of and occasionally despite the importance or value an agent attaches to them. Nevertheless, it may be the case that an agent values these assumptions and takes them to be true because she values them. She may further resolve the evidential uncertainty on the basis of these valuational commitments. But no one doubts that this would be an example of poor reasoning—background assumptions don’t become true because they are valued.

But perhaps the point being made by the proponents of value-ladenness is different: it is that an agent introduces these factual assumptions into her reasoning to begin with because she values them and considers them important. This would be an interesting causal phenomenon, one to keep an eye on. It is not a phenomenon, however, that has any bearing on the evidential standing of a hypothesis. This standing is solely determined by the content of the available evidence and the ancillary hypotheses needed to connect this evidence to the hypothesis under test. The causal forces that lead one to accept either the evidence or the ancillary hypotheses are a separate matter from the epistemic status of such evidence or hypotheses. If there is a link here, it needs to be shown that these causal forces do in fact have epistemic significance. But that is precisely the point up for debate. It is precisely the claim of those who support the Rudnerian uncertainty argument against value-freedom that values, in causally influencing decisions about evidential support, thereby possess the epistemic authority to do so—hence this is not a claim we can assume in advance of justifying the uncertainty argument. Still, it might be suggested that values in causally influencing evidential judgments have an epistemic to play since, in testing a hypothesis, one has a choice about what evidence to use or what ancillary hypothesis to deploy, and thus values are needed to narrow the investigation—simply, we can’t use all the evidence or all the ancillary hypotheses we can think of, so distinctions need to be made on the basis of values (e.g., some evidence is valued more than other evidence, so this evidence becomes more central in the epistemic evaluation of a hypothesis). But it remains the case that using values to justify one’s choice of evidence or one’s choice of ancillary hypotheses is illegitimate, in just the same sense that valuing an outcome doesn’t make the outcome come
about. That is, evidence doesn’t become reliable and hypotheses don’t become informative simply because they are valued. The reliability of evidence and the informativeness of hypotheses are matters of fact that we ascertain epistemically by ensuring that our evidence is generated using sensitive and accurate technology and that our auxiliary hypotheses are empirically well-supported. Of course where the empirical question is more conjectural, such as when evidence is improperly generated or where ancillary hypotheses lack empirical support, it may well follow that values will take a formative role in evidential reasoning—but that is an unfortunate situation we should strive to remedy, not a normative guideline for a theory of evidential reasoning.

The above response will succeed, though, only if there is a sustainable distinction between epistemic and non-epistemic factors, and it has been argued by Rooney (1992) and others that this distinction breaks down. There are three reasons why one might think this to be the case.

To begin with, Rooney (1992) makes the point, as did McMullin (1983) and Kuhn (1977) before her, that the application of epistemic values (understood by her in the general sense of including cognitive values) is imprecise and judgmental (Rooney 1992, p. 15). For example, consider the case described above involving the choice between geocentric and heliocentric planetary systems, where the criterion of external consistency favors the former, and simplicity favors the latter. How one applies the criterion of external consistency (and \textit{mutatis mutandis} for all other epistemic factors) is quite variable, depending on what other theories one asserts it is necessary to be consistent with. So depending on how this criterion is used, one might be lead to different conclusions. For Kuhn, and following him McMullin and Rooney, this means we should not expect the application of epistemic factors to be algorithmic, and in searching for a different term to describe their use, Kuhn suggests the terms “maxims, norms or values” (1977, 330). The benefit of describing these factors as values (for example)—aside from the fact that the term value better describes the variable way in which these factors are applied, generally speaking—is for Kuhn twofold (see 1977, pp. 331–2). First, by calling them values one can account for how epistemic factors can be used in the early stages of research, for just as such research is to a certain degree characterized by imprecision and speculation, so must the application of epistemic factors itself be imprecise and speculative. Secondly, whereas the use of an algorithm removes the possibility of disagreement and debate between scientists (as there would be only one conclusion to draw from the data), viewing epistemic factors as values allows and even authorizes the sort of creative dissension that Kuhn thinks is critical if new theories are to develop.

Now it is worthwhile emphasizing the uniqueness of Kuhn’s description of epistemic factors (i.e., “criteria for evaluating the adequacy of a theory,”
1977, p. 322) as values in light of their being applied imprecisely and judgmentally, as opposed to calling them criteria or rules which would have more determinate applications (1977, p. 330). A more typical understanding of value is offered by Kevin Elliott who remarks that a value (as used in science) refers “to any quality that scientists regard as desirable, meritorious, or worthy in a theory or hypothesis” (2011, p. 57). But the accuracy of Elliott’s definition is likely due to its tautologous nature. My own view is that Douglas’ approach to what counts as a value (as distinguishable from a criterion) is more accurate. For her, epistemic virtues are the criteria a theory must meet in order to be considered a science to begin with—without them the goal of science as reliable knowledge is unachievable—whereas values “are aspects of science for which to strive, but which need not be fully present in all cases” (2009, p. 94). Further, for Douglas, there are two epistemic criteria, internal consistency and predictive competence (i.e., empirical adequacy or conforming to the world), and other quasi-epistemic features such as external consistency, simplicity, explanatory power and fruitfulness (some of which appear on Kuhn’s list of values) are more adequately described as cognitive values (so, for Douglas, there are strictly speaking no epistemic values, only epistemic criteria and non-epistemic values). Even with Douglas’ explication though, it may still be true (as Kuhn suspects) that the application of the criteria (or for Kuhn, the values) of internal consistency and predictive competence is loose, that is, non-algorithmic, imprecise and judgmental. If so, what does this tell us about the tenability of the epistemic/non-epistemic value distinction?

Rooney argues that because of the looseness of how epistemic values are applied, it follows that the epistemic/non-epistemic value distinction breaks down. As she suggests, because of this looseness, values can enter into scientific reasoning in two ways, first, in what criteria scientists use to evaluate a theory, and secondly in how they “weigh or value” these criteria when applied (1992, p. 15). From here she contends that, because of this looseness, non-epistemic values are able to intrude on the use of epistemic values in theory evaluation “in a way that surely fundamentally challenges the [epistemic/non-epistemic] distinction itself” due to the “cognitive gap … [left over] after accepted epistemic criteria and rules of inference are applied to the evidentiary base” (1992, p. 15). It follows that Rooney’s argument from the looseness of epistemic criteria to the conclusion that the epistemic/non-epistemic distinction collapses is itself a version of the confirmatory gap argument we examined and rejected above. Indeed, it is an argument Kuhn himself would be at pains to reject as well. When Kuhn lists his criteria of theory choice, it is a restrictive list containing five elements—accuracy, consistency, broad scope, simplicity and fruitfulness. It is because scientists use these criteria (or values) in their research that
makes this research rational and objective. By comparison, there are other values that scientists could use that would not lead to a rational, objective result. These would be what Kuhn describes as “matters of taste” (1977, p. 336), cases where “bias and personal likes or dislikes function instead of, or in the face of, actual facts” (1977, p. 337). That is, Kuhn is very clear about what values he considers scientific (epistemic) and those he considers non-scientific (non-epistemic), so arguing on the basis of the looseness of epistemic criteria to the collapse of the epistemic/non-epistemic distinction would be anomalous for Kuhn.

The second reason why one might argue that a distinction between epistemic and non-epistemic factors cannot be sustained focuses on how, in scientific theory testing, assumptions are made that involve a mix of epistemic and non-epistemic, practical factors. In this regard, Rooney cites the case of Ernan McMullin’s “non-standard epistemic values” (see McMullin 1983, pp. 19–20, discussed in Rooney 1992, p. 16), metaphysical assumptions that are ostensibly factual and epistemically significant but that are also so abstract as to be practically untestable. McMullin’s examples include Newton’s use of a theological world-view in his physical thinking and the fundamental debate between Einstein and Bohr on the coherence and order of the universe, cases where non-scientific factors played a role in Newton’s, Einstein’s, and Bohr’s intellectual orientations (see Rooney 1992, p. 16, quoting McMullin 1983 and 1984). Rooney also looks at the case discussed by Helen Longino where, once more, the assumption of gender dimorphism plays a constitutive (epistemic) role in the dispute between the linear-hormonal and selectionist models, even though the assumption has obvious “contextual social and cultural underpinnings in a society that supports gender order and hierarchy” (1992, p. 18). These are cases Rooney interprets as involving such an intermingling between epistemic and non-epistemic factors as to render these factors indistinguishable.

Nevertheless, as Rooney notes, McMullin held out the promise that the epistemic and non-epistemic elements in such fundamental assumptions could be teased apart through continued theory testing, that “the progressive march toward conformity between theory and world will ‘sift out’ any non-epistemic factors that may have been instrumental in original theory-formation” (Rooney 1992, p. 18). Now it is to be granted that, despite the critical, self-reflective stance of scientists, assumptions expressing cultural or personal biases could make their way into theoretical and empirical inquiry unannounced, with the result that this inquiry is furtively informed by such biases. However, there is good reason to hope that this will not occur. After all, physicists can now easily disentangle Newton’s theological allegiances from his more rigorous physical reasoning, and the metaphysical debate between Einstein and Bohr is studied these days by historians
and philosophers of science who do not suffer the burdens of Einstein’s and Bohr’s philosophical predilections. Longino, too, is notable for her contention that the gender biases of scientists can be revealed by subjecting these biases to the scrutiny of open, public and inclusive debate. Finally, McMullin’s suggestion that empirical inquiry can sift out non-epistemic factors carries a significant amount of promise, once we recognize the ability of scientists to invent strategic forms of reasoning and innovative technologies that can effectively decide between previously underdetermined theoretical options.

So why does Rooney express skepticism about the possibility of distinguishing the epistemic from the non-epistemic? She comments:

given McMullin’s … admission that to the extent that scientific observation is theory-dependent it is also indirectly value-impregnated [(McMullin 1983, p. 14], it is not clear that one can readily appeal to some neutral value-free court of future observation and experiment that will in time provide the “right” distillation. (1992, pp. 16–17)

But it is an error to equate the theory-ladenness of observation with its value-ladenness, much as I noted above in discussing the flaw underlying the uncertainty argument (leaving aside the question whether it is a flaw that burdens McMullin). That is, when a scientist is committed to a theory which informs her observations, it need not be the case that she values this theory or that this theory is about values. She may simply be making an assumption that is true, and that correspondingly provides her with true observations. Thus, there is no particular reason to worry that the distinction between epistemic and non-epistemic factors necessarily breaks down where we are considering the fundamental assumptions that inform scientific theory testing. A scientist could find an assumption valuable to make, though it is false, just as she could regard an assumption as true, but as leading to an outcome without value.

A third reason for the claim that the epistemic/non-epistemic value distinction breaks down is suggested by Elliott (2011). One might, in an effort to keep apart the epistemic and non-epistemic factors relevant to accepting a hypothesis, recommend that scientists simply provide decision-makers with the evidence they possess on behalf of a hypothesis (perhaps indicating how probable the hypothesis is, given this evidence), and then leave it to the decision-makers themselves to accommodate non-epistemic values when reasoning about which action to perform in a practical context. With this delegation of duties, the non-epistemic and practical can be kept separate from the epistemic. However, this strategy leads to a dilemma. Following the views of Carl Cranor (1990), Elliott notes that scientific evidence is often
complicated and abstruse to the extent that understanding it is beyond the capability of risk managers (see Elliott 2011, p. 67, citing Cranor 1990, p. 139). But if that is so, risk managers aren’t really in a position to make informed and worthwhile decisions. So alternatively, in making their scientific results accessible to decision-makers and the public, scientists need to simplify their results, which Elliott (following Cranor) believes will of necessity lead them to allow “social values to influence their standards of evidence” (2011, p. 68). So either decision-makers lack the scientific expertise to make informed decisions, or they gain this expertise by allowing scientists to make value decisions on their behalf.

However, it is not clear that risk managers, or decision-makers generally, are so completely at a loss when it comes to having the background to understand scientific information. Of course, where such a knowledge gap exists, the resultant decision could become value-informed if scientists sense that values are needed to ensure that a reasonable decision is made. So let us suppose this is the case. The question then becomes whether a scientist can distinguish in her mind the epistemic from the non-epistemic, and there is no indication from Cranor, via Elliott, that this is impossible. Their worry was that decision-makers, due to their ignorance, would need to deal with value-laden evidence. But where the scientist herself becomes the de facto decision-maker, there is no longer any problem with ignorance. Indeed, I think this is an assessment that Elliott ultimately concedes. He recognizes that a scientist could, if intent enough, bracket the pragmatic aspects of testing a hypothesis with evidence, separating them from the pure epistemic aspects. Still, he maintains, this fact does not disrupt his main goal, “to show that, in the context of policy-relevant research, non-epistemic values have a legitimate role to play in the evaluation of evidence for a hypothesis” (Elliot 2011, p. 69). And from where does he infer that non-epistemic values have this role? A key part of that story is, for him, the “uncertainty principle” (2011, p. 70), that “scientific conclusions must be genuinely underdetermined by epistemic factors” (2011, p. 63). In other words, his argument for value-ladenness ultimately reverts to the uncertainty argument, discussed above, which we have already found wanting.

So, to summarize, the Rudnerian uncertainty argument is insufficient to demonstrate the value-ladenness of science. First of all, for a background assumption to help fill the confirmatory gap in compelling a conclusion, it need not be the case (and perhaps should not be the case) that these assumptions are valued. Secondly, arguments for value-ladenness that cite the breakdown of the epistemic/non-epistemic distinction themselves ultimately assume the valued nature of background assumptions.

But perhaps then it is not the uncertainty underlying empirical evidence that forms the wedge through which values enter, but simply the
recognition that scientists have moral responsibilities, just as we all do. This leads to the second argument disputing the (moral) value-freeness of science, the moral argument. We start with Heather Douglas’ version of this argument.

4. Scientists as Moral Agents: The Moral Argument (Douglas)

Douglas (2009) adopts the very reasonable view that scientists possess the same set of general moral responsibilities we all have, and that for scientists this responsibility is heightened by the special authority scientists are endowed with by society. This authority occasionally entitles them to perform research in areas where, if they are not careful, enormous harm could ensue to others. Rightly, Douglas notes that it would be morally reprehensible for scientists to adopt whatever methods they wish in engaging in such research, if this means putting unwilling people at great risk. Her proscription includes even those methods whose epistemic merit is clear. For example, suppose one wants to know whether a new drug has negative effects on humans. One epistemically valuable way to find this out is to give this drug to people without their consent, and in this way effectively avoid the placebo effect. However, despite the epistemic merit of such a strategy, Douglas maintains that one should not for moral reasons investigate the qualities of a new drug in this way—and this is certainly true. A different sort of example is this: suppose one wants to find out whether a doomsday device can be built, a device that when detonated will obliterate the earth. One could even suppose that one’s interest in this question is purely theoretical. One has no desire in the least to create such a device—one simply wants to see if it can be built. Once again, Douglas’ response here would be to suggest that such research should not be performed. The reason is that there is a slight possibility that this information would get into the wrong hands, and the result of this happening is so grave that such (even theoretical) research should not be performed to begin with.

Conceivably, every decision a scientist makes, including bare hypothesis acceptance, has some ethical implication, even if very small. Moral issues clearly come to the fore in the choice of a research area. We have seen the potential, moral ramifications of even a purely theoretical search for a doomsday device. Or to take a different example, faced with the choice to find a cure for cancer or to study the mating habits of arachnids, it is arguable that a scientist should engage in the former inquiry, if (as seems likely) learning about a spider’s mating habits offers no redeemable practical benefits. Even the choice to study pure mathematics can be morally significant—if, say, by doing math one becomes inhibited about harming people. Similarly, moral issues are clearly relevant to the choice of
an empirical methodology, such as we noted above in requiring the (informed) consent of people if they are to act as research subjects in clinical research. In recruiting people for clinical trials, it is now standard to require that subjects give their consent by reading and signing a form written in comprehensible language and explaining in sufficient detail the possible, negative side effects of participation. A different sort of norm on research methodology concerns the treatment of graduate students: they should be reasonably compensated and provided with a safe working environment, even if not doing so would generate better evidence for a hypothesis. Inspired by these sorts of cases, Douglas’ view is that every scientific decision is at the same time a moral choice. As she says, “no convincing argument has been articulated to give scientists even an occasional moral exemption from the considerations of the consequences of their work” (2009, p. 79). For Douglas, there is no (morally) value-free science: in her words, “science is saturated with values” (2009, p. 113).

To be sure, it can hardly be contested that choosing a research area or choosing a research methodology are value-laden exercises. But for Douglas’ thesis of the value-saturatedness of science to be at all controversial it must go a step further: her claim must be that the question whether there is sufficient evidence to believe a hypothesis is itself partly determined by moral factors. Such a claim is defended in Rudner (1953). Each of the two hypotheses, “the drug is safe” and “the belt buckles are non-defective,” share a similar degree of evidential support, but for Douglas and Rudner whereas the latter hypothesis is sufficiently confirmed, the former isn’t, all because of a moral responsibility to protect people from using unsafe drugs and the relative, moral innocuousness of owning a defective belt buckle. Such a position on how values can saturate the confirmation of scientific hypotheses (it is nothing less than the uncertainty argument above) is quite contentious (or so I have argued), much more so than the value-saturatedness of deciding what research to engage in or what methodology to adopt. To this extent, Douglas’ discussion (2009) is somewhat misleading. Her targets seem to be the likes of Hermann Lübbe and Percy Bridgman who apparently advocate complete moral impunity for scientists. She quotes Lübbe who says that scientists should enjoy “a morally unencumbered freedom from permanent pressure to moral self-reflection” (1986, p. 82; quoted by Douglas 2009, p. 75). Similarly, in Douglas’ words, “Bridgman suggests that we must relinquish … claims of social and moral responsibility on scientists so that they can produce [the] valued end [of scientific knowledge]” (2009, p. 76). If Lübbe and Bridgman are contending that the pursuit of knowledge in and of itself can license such activities as constructing a doomsday device or performing Nazi-style human experimentation, then their views are to be rejected, just as Douglas does. But the rejection of such
activities is uncontroversial and does not address the subtler question of whether the evidential status of a hypothesis is itself a valuational question. Douglas’ argument for the moral, value-ladenness of science is thus problematic in that it is largely based on her intuitions, such as those described above, illustrating the moral error of ignoring the foreseeable negative consequences of scientific activity. With those intuitions in the background she asserts that “the distinction between making a general empirical claim and making a [moral] judgment about the safety of a scientific process seems dubious at best” (2009, p. 78), and that there are “no tenable grounds” for scientists to enjoy any moral exemptions relating to such a distinction (2009, p. 79). More detail about her view can be found in her review of possible objections to her position, objections she thinks are based on “rather desperate attempts to articulate some kind of moral exemption from a general responsibility we all share” (2009, p. 79). In what follows we critically examine both these objections and her responses to them.

4.1. The First Objection to Douglas: The Authoritative Status of Science Will Suffer if It Is Burdened by Moral Values

Here the worry is that, if scientists start importing moral concerns into their assessments of hypotheses, hypotheses will come to be accepted not because of what the evidence says but because such acceptances address these moral concerns. The situation here is reminiscent of a problem encountered in ethics, cases where one has the option of telling someone the truth, but where doing so has certain negative effects. For example, a doctor may learn that a patient is HIV positive and is considering whether to inform the patient of this. Let the situation be one where the doctor foresees that, by telling the patient the truth, the patient will lose her composure and commit a number of dangerous, reckless acts, and that the consequences of lying to the patient are much less worrisome. One might argue here (and presumably Douglas would argue in this way since she tends to argue, like Rudner does, as a consequentialist) that the doctor’s best option is to lie to the patient. Now return to the original case, that of hypothesis acceptance by a scientist. Should the doctor (thinking as a scientist) accept the hypothesis that the patient is HIV positive, where such an acceptance leads her to advise the patient of this fact? Apparently the Douglas/Rudner answer is that the doctor should not accept the hypothesis, given the dire consequences of doing such. Indeed, the doctor should perhaps accept the opposite hypothesis that the patient is not HIV positive, given the moral benefits of doing this and assuming that these benefits outweigh the benefits of remaining silent on the issue. At this point the reader will notice the grave hazard there is in importing moral values
(or any values) into the mechanics of hypothesis acceptance. At the extreme, hypotheses could then be accepted simply because accepting them makes everyone happy and accepting their negation makes everyone sad. Or again one will accept a hypothesis, and consider one epistemically warranted in doing so, if this acceptance means one will avoid a threat to one’s well-being, despite what the evidence says. All one needs to do is raise the level of the threat sufficiently. Again, as Douglas suggests, for scientists (or for prospective knowers of all kinds) there is no moral exemption from the general moral responsibilities we all share. But surely, so the objection goes, we should in such a case express great cynicism about the (epistemic) authority of scientists, given that they are promulgating hypotheses and theories based on their perceived (and perhaps quite idiosyncratic) sense of moral duty. Indeed, there need be no restriction here on promoting hypotheses only if they are true. One’s moral duty may be achieved by advocating a hypothesis known to be false, even with certainty. As such, where moral values influence scientific decision making, one is led to question the authoritative status of science.

Douglas’ reply to this objection is to express doubt about the authority of science altogether. She remarks, “no particular piece of science is obviously authoritative anymore, as debates over sound science and junk science rage” (2009, p. 79). Earlier in her book she refers to and discusses academic debates concerning the presumed authority of science (2009, pp. 5–13). Whereas the Science Wars focuses on whether science has any epistemic authority to begin with, the junk science/sound science debate deals with the problem of identifying which science amongst various contenders has this authority (assuming one of them does). Douglas’ contention is that there is enough controversy as regards which science to accept as to throw the entire question into a state of uncertainty. It is then this “endemic uncertainty of science” (along with “science’s importance for public decisionmaking”) that underlies for Douglas the value-ladenness of science, or what she calls science’s lack of epistemic “autonomy” (2009, p. 13). Without a doubt we can grant with Douglas that there is plenty of uncertainty in science. And though Douglas doesn’t make reference to the traditional realism/antirealism debates in the philosophy of science, one might supplement her case for the endemic uncertainty of science with the reservations expressed by antirealists deriving from the so-called pessimistic induction, the fact that over time most every scientific theory, even the most celebrated and fundamental of such, has suffered subsequent rejection. On her view, the value-free ideal “has done nothing” (2009, p. 79) to mitigate the characteristic uncertainty of science.

But surely one can see the benefit generated with moral value-freedom when we reflect on the case of the doctor, discussed above, who is mulling
over whether to accept the hypothesis that a patient is HIV positive. The right position seems clear: that if the evidence meets the relevant factual standards, the doctor should believe that a patient is HIV positive. The doctor may find such a belief to be exceedingly painful, say, if the patient is her daughter. But the pain of that knowledge and the moral obligation not to cause herself pain unnecessarily should not alter the doctor’s conviction, so long as this conviction is motivated by the factual accuracy of the test results. Along these lines we echo the view of Isaac Levi in a passage negatively quoted by James Gaa. Levi remarks:

by insisting that scientific inquiry has objectives quite distinct from those of economic, political, moral, etc. deliberation, [we reject] the point of view which holds that the scientific propriety of a man’s beliefs is dependent, at least in part, on moral, political, economic, etc. consequences of his having these beliefs. (Levi 1973, p. 16; quoted in Gaa 1977, p. 523)

Moral values should not play a role in assessing the confirmatory status of a hypothesis, if the evidence is strong enough, and clearly the science of HIV infection has developed to the point where uncertainties in the test results are of no concern. This is not to deny that, if the fate of the world depended on the accuracy of this result (perhaps because an evil genius has rigged a doomsday device that will detonate if the doctor’s diagnosis is mistaken), then the doctor should be circumspect in her evidential assessment of her hypothesis. But that’s only to say that science is not value-free in that unique circumstance, not to say that it is not value-free in most other, non-tragic circumstances. It might further be true that the doctor, despite her belief that the patient is HIV positive, is hesitant about advising the patient of this fact given a suspicion that the patient, in her despair, will commit a dangerous act (a judgment that nevertheless depends on one’s ethical theory; again, Douglas tends to be consequenti-
alist about such matters). But even if the doctor decides for moral reasons to lie to the patient about her HIV positive status for moral reasons, there is no reason to expect the doctor to lie to herself about this fact. We would surely look with disfavor at the doctor’s status as a scientific authority if she lied to herself and then to us on issues of medical fact, just for fear of avoiding a negative, practical consequence.

Could Douglas respond here that the science regarding HIV testing is so uncertain as to disrupt a doctor’s claim to be an authority on the occurrence and dangers of HIV infection? I doubt she would go this far, given that the current science of HIV testing is viewed with a great deal of assurance. Elsewhere, in fact, she claims that it is the authority of scientists that makes it so important that they be careful in their judgments since
the general public will take their judgments to be largely true (2000, p. 563). One might reasonably suppose that this acceptance of scientific authority is a result of scientific activity arriving at the correct view much of the time. If, alternatively, scientists are adopting moral opinions that lead them to avow false claims, we should anticipate that their authority will be diminished, as the objection we are considering suggests. Thus, Douglas’ position is not helped by advocating a general skepticism or anti-realism regarding scientific claims.

Here is another objection to Douglas’ advocacy of the value-ladenness of science that she considers and then rejects (2009, p. 79).

4.2. The Second Objection to Douglas: With the Loss of (Moral) Value-Freedom, Science Will Become Less Objective

This objection is similar to the worry we have just been discussing. Where moral values play a role in scientific reasoning, moral obligations may lead scientists to distort the truth and science will become less objective (clearly, the same problem could be said to arise for social, political, aesthetic, and other non-epistemic values, though Douglas’ focus is solely moral values). In response, Douglas argues that objectivity is not compromised by the impact of moral values. Alternatively, she endorses what she calls “detached objectivity” (2009, p. 122) according to which moral values are prohibited from having a direct role in evidential reasoning, one where such values themselves count as evidence for a claim. In this way she takes herself to answer the concern that the search for truth will be sidetracked by personal interests. Comparatively she rejects the viewpoint she calls value-free objectivity, one that advocates the complete removal of moral (and other non-epistemic) values from evidential reasoning. On her view, it is permissible, even obligatory, for moral values to play an indirect role in scientific inquiry, where the relevant science possesses some element of uncertainty and values are needed to bridge the confirmatory gap. But Douglas believes that science is inevitably uncertain. Thus, for her, the distinction between a direct and indirect role for values in scientific investigation becomes irrelevant to the preservation of scientific objectivity since with the uncertainty of science the influence of values is inevitable and acceptable in every case. Indeed, values could be said to play a decisive role in the making of scientific judgments. All that is required is for a scientist to have a large enough stake in a particular outcome of an (uncertain) scientific investigation. In effect, then, Douglas’ position faces a dilemma. If she regards scientific judgments as inevitably uncertain, moral values are able monopolize scientific inquiry and we lose the objectivity of science. Even if such values do not play a direct evidential role, they can still fundamentally guide scientific reasoning by exploiting the uncertainty underlying such reasoning. On the other hand, if scientific
judgments are indeed candidates for certainty, the evidential role of values is, for Douglas, excluded—their indirect evidential role is excluded because there is no uncertainty to exploit, and Douglas already excludes a direct role for values. So on Douglas’ account, either value-ladenness undermines the objectivity of science, or science is value-free, after all. (An analogous concern with Douglas’ direct/indirect distinction is raised by Gregory Morgan 2010).

At this point Douglas could reply that, apart from detached objectivity, there are other ways in which science can be said to be objective that are, for their part, compatible with value-ladenness. To this end she lists six other senses of ‘objectivity’ (leaving open the possibility of there being even more). On her view “what holds all these aspects of objectivity together is the strong sense of trust in what is called objective” (Douglas 2009, p. 116). What does it mean for a knowledge claim to be objective and so trustworthy? “Objectivity,” she explains, “does not ensure truth, but rather ensures the claim was the result of our best effort to accurately and reliably understand the world” (2009, p. 117). For example, she approvingly cites “manipulable objectivity,” the experimental strategy suggested by Ian Hacking in which we use a theoretical entity to manipulate objects in the world, as a method one can use to arrive at trustworthy, reliable accounts of the “objects around us” (2009, p. 118). Similarly, she cites the strategy of “convergent objectivity,” where independent empirical methods converge on the same observed result, as a technique that removes “the possibility of illusion” (2009, p. 120). Still, whichever strategy she considers there is always the chance that in applying this strategy valuational considerations have a way of steering results. As a case in point, with manipulable objectivity there is always residual uncertainty about whether one is in fact manipulating the theoretical entity under consideration or manipulating something else (a problem ameliorated by Hacking’s minimalist semantics for theoretical entities though not entirely removed); accordingly, given this indelible element of uncertainty, values can always play a role in guiding the judgment that one is manipulating a particular theoretical entity. A similar problem arises for the other senses of objectivity Douglas considers (for instance, there will always be some uncertainty about whether convergent empirical methods are truly independent). It follows that the worry cited earlier recurs: we don’t want values to play a direct evidential role, yet in playing even an indirect evidential role they are able to become decisive after all, so long as the participant scientists have a big enough stake in a particular outcome. As a result, none of the senses of objectivity Douglas mentions can succeed in dispelling the hazard posed by the value-ladenness of science since each of them, when applied, contains an element of uncertainty, and so each of them is influenced indirectly, and thus ultimately, by values.
Let us now look at how Gaa (1977) argues for the (moral) value-ladenness of science and rebuts potential objections to his view.

5. Scientists as Moral Agents: The Moral Argument (Gaa)
James Gaa offers two arguments against the (moral) value-freeness of science. On the first argument, we assume a scientist is educating a student about scientific methodology. In the course of this education, he counsels the student to only take into account epistemic factors when making a decision about a hypothesis. For instance, he advises the student to resist accepting a hypothesis because it may be his favored view or because the truth of the hypotheses would be profitable to him. On Gaa’s view, such advice to take into account only epistemic factors in scientific investigation is not itself epistemic advice, on pain of circularity—it is nothing less than an ethical recommendation. As he remarks, “if one chooses a decision procedure involving only epistemic objectives, one must do so on the basis of societal or ethical values, and not on epistemic grounds” (1977, p. 531). It follows that science is irrevocably laden with ethical values: it’s as simple as noting that the recommendation to seek the truth is itself an ethical, valuational claim.

Is this argument persuasive? To illustrate why not, consider a simple, analogous argument. Are dogs value-laden? That is, to know what a dog is, to understand how a dog’s internal physiological mechanisms work, and so on, must one factor in values as components of dogs? Of course, it does not make much sense at all to say that values are a part of dogs, along with their more familiar physical components. Dogs contain bones, blood, cartilage, cerebral hemispheres, and so on. No values of any kind form a part of a dog’s constitution. Still it is true that some people value dogs enormously, as much or more than they value science. So by analogy, when we say that people value science or value the truth, it doesn’t therefore follow that values are a component of science or the truth. There is no valid inference from “X is valued” to “values are a part of X,” just as there is no valid inference from “dogs are valued” to “values are a part of dogs.”

Gaa’s second argument is somewhat more compelling. It is similar to Douglas’ moral argument discussed above, but has a significant reflexive quality. Gaa uses the example of a scientist who is asked by policy-makers to determine the feasibility of a certain kind of biological weapon created from a pathogenic bacterium. The scientist deliberates on whether to carry out this task and recognizes that, if an affirmative answer is arrived at, the result might be disastrous for humanity if the biological weapon is ever used. Particularly, the result might be disastrous for science itself since the weapon, if used, could wipe out all scientists and so halt scientific
research. It follows that the scientist should take (moral) values into account in her decision about whether to carry out this research, for fear of her work being completely self-defeating. Now, Gaa acknowledges that this sort of cataclysmic case is unlikely. Still, the sort of scientific research where there is momentous risk is common enough, he thinks, to cause worry in the general public, which means that scientists need to be morally pro-active or else the public will lose their trust in science, leading to “an increased loss [for scientists] of autonomy (in the sense of self-control), decreases in funding levels, and all the effects that would attend such changes” (1977, p. 532). In short, scientists must consider moral values in their research if they are to gain permission from the public to carry out their research.

Gaa’s argument concerns the case where a scientist is deliberating on whether or not to engage in a particular research program and, as I have suggested, it is uncontestable that the choice of a research program has a clear valuational component, especially where we are considering the manufacture of highly lethal, biological weapons. To this extent, it is easy for even those who advocate the value-freeness of science to grant Gaa’s point. Scientists should attend to the disvalue of engaging in research that could end all research. But that doesn’t say anything about the use of moral or other valuational principles in theorizing about how to develop such a weapon. The epistemic signficance of valuational principles in the actual development of this weapon is quite unclear. Similar to how we argued above in assessing Rudner’s uncertainty argument, the epistemic evaluation of the technology underlying the design and implementation of biological weapons naturally requires the deployment of various background assumptions. Yet that doesn’t mean that these assumptions need to be valued. To be relevant, they need only be accepted as true for the evaluation to proceed. Thus, Gaa’s second argument is correct so far as it goes. It simply doesn’t go far enough to challenge the value-freeness of science.

Similar to Douglas, Gaa also looks at, and rejects various possible objections to his position. Again, our strategy is to critically examine these objections and his responses to them.

5.1. The First Objection to Gaa: Where the Truth of a Hypothesis has Great (Moral) Value, the Standards for Acceptance are Highly Reduced

As Gaa notes (1977, p. 533), the situation described here is the mirror image of the situation described in Rudner’s original argument. In Rudner’s original argument, the risk of harm raises the evidential standard for when the safety of a drug can be rationally accepted. By contrast, the situation described in the objection is one where the prospect of a great benefit lowers the evidential standard for rational acceptance. We might imagine the case
of someone considering the hypothesis that she will win a multi-million dollar lottery if she buys a ticket. Given the possibility of winning a lot of money—we might even conjecture that the money will be used for noble charitable uses, so that many other people are thereby benefited,—the quality of evidence needed for her to rationally accept the hypothesis, “I will win,” is correspondingly reduced, just as it was raised with the prospect of great harm. But is it legitimate to say that the ticket holder is rational to accept the hypothesis that she will win the lottery, given, let’s say, she owns one ticket out of a thousand?

Gaa’s response to this objection is to suggest that there should be a balance in scientific decision making between epistemic and ethical goals, that there is good reason for scientists “not to slight the attainment of their epistemic objectives in this kind of situation” (1977, p. 534). Thus, presumably, the lottery player would consider that, although the potential payout is great, the chance of winning is so exceedingly low as to make accepting the hypothesis of winning unreasonable to accept, perhaps at any stake. Yet such a response for Gaa is puzzling since he is otherwise skeptical about the existence of a viable epistemic/non-epistemic value distinction (1977, p. 524), just as Douglas is (2009, p. 90). Indeed, the point of Rudner’s original argument is to show that epistemic, scientific questions collapse in part to non-epistemic, valuational questions. But an epistemic/non-epistemic distinction is needed if Gaa is to talk about a balance between epistemic and ethical (i.e., non-epistemic) goals. He suggests that, if scientists completely ignore epistemic objectives, the result might be a “lack of support for any research which could not be justified on the grounds of practical or moral use alone” (1977, p. 534). Thus it seems that the existence of ‘pure’ epistemic objectives is sensible for him after all. This reading of Gaa is confirmed when he states later in his paper:

the position [in this paper] is that no matter which objectives are considered to be characteristically, or properly, epistemic, scientists should also consider other sorts of consequences of their decisions, i.e., in relation to a more inclusive set of objectives [(i.e., including practical objectives)]. (1977, p. 538)

So it seems that, for Gaa, where moral values have the potential to disturb the epistemic quality of a scientific judgment, his recommendation is to allow epistemic values more sway in the resulting judgment. But this view sounds suspiciously similar to the view of those who defend the value-freeness of science. In fact, if for Gaa there are pure epistemic objectives, one might imagine that under the right circumstances a scientific decision could be made for solely epistemic reasons. This is a possibility Gaa considers in the next objection.
5.2. The Second Objection to Gaa: It Is Conceivable that there Are Scientific Hypotheses that Have No Ethical Consequence at the Time of Their Acceptance

Levi (1960) makes a similar point. He provides examples of scientific research “for which practical objectives are difficult to specify,” such as the investigation into “the hypothesis of an expanding universe” (1960, p. 350). Gaa for his part does not dispute the possibility of such ethically irrelevant research; his examples include “hypotheses concerning the colors of particular stars” and “the gestation period of an obscure species of tree frog” (Gaa 1977, p. 534). Nonetheless, such a possibility for Gaa:

only shows that some of a scientist’s decisions and other activities may be irrelevant (as far as can be told at the time) to the ethical interests of society—and, it should be added, irrelevant to the work of many other scientists, as well. (1977, p. 534)

So for Gaa it’s not impossible for scientists to carry on their investigations in a value-free manner. It’s simply that value-free inquiries are not ethically (or socially) relevant, by which I take Gaa to mean that such inquiries don’t solve any ethical (or social) problems. On the other hand, where scientists have an obligation to attend to the moral and social problems that afflict society, purely epistemic, value-free research could be an obstacle to carrying out this obligation. In Gaa’s words, “autonomy (i.e., value-freeness) is tantamount to the claim that scientists qua scientists are not free to act ethically, if it interferes with the attainment of their ‘epistemic’ objectives” (1977, p. 351). For her part, Douglas’ overall assessment of value-freeness is the same: she sees the value-free ideal as endorsing the ability of scientists to exempt themselves from moral responsibility. As she comments, unfortunately for the proponent of value-freeness, “scientists must weigh the consequences of error in their work” (2009, p. 85).

Yet these assessments of value-freeness are surely too harsh, and even somewhat irrelevant. On behalf of (non-epistemic) value-freeness, Levi says that he rejects “the point of view which holds that the scientific propriety of a man’s beliefs is dependent, at least in part, on moral, political, economic, etc. consequences of his having these beliefs” (1973, p. 16), once more, the sort of comment that draws Gaa’s scorn (1977, p. 523). But Levi is not suggesting here that scientists should completely set aside ethical standards for the sake of the pursuit of truth. His point is only that the evidential assessment of a scientific claim should proceed independently of non-epistemic issues. The believer in value-free science can easily recognize and abide by what they perceive to be their moral obligations to society, and Gaa and Douglas are wrong to suggest otherwise. For example, it is no abandonment of epistemic ideals to reject a research project aimed at
developing a doomsday device. Building a doomsday device is simply not the sort of research project most people consider valuable to pursue. But it would be an abandonment of epistemic ideals and scientifically unacceptable if one used ethical criteria in the evidential assessment that a doomsday device is technologically feasible, say, if one argued that a component of this device would need to be manufactured by a certain company since one made a promise to this company to promote its products. Neither Gaa nor Douglas say enough in showing that (moral) values play an evidential role in the assessment of hypotheses. They tend to focus purely on the role of moral values in the decision to accept a hypothesis, while ignoring the more problematic issue of whether (moral) values can serve as evidence for a hypothesis, whether directly or indirectly.

5.3. The Third Objection to Gaa: If Science Is Necessarily Value-Laden, Scientific Methodology Would Have to Address Ethical, Social, Political, and Other Valuational Considerations of all Sorts, which Is an Unreasonable Burden

Normally, science education simply involves training students in the highly technical aspects of scientific methodology. Non-epistemic values (except perhaps for cognitive values) play no obvious, essential role in this education: students are too busy learning the science to get sidetracked with the ethical, social, political and other such dimensions of their work. If anything, they might be required to take an ethics course that involves learning the codes of ethics relevant to their chosen field. Otherwise, it might be recommended that they take an elective philosophy course on applied ethics only marginally related to their interests. Clearly, then, incorporating ethical topics as a central part of science education would be an enormous curricular shift. It would be especially disruptive if the suggestion were made that ethical factors should be expected to play a part in evidential considerations, that is, if students were taught to consider in evaluating the evidence for hypotheses the potential impact of their work on public well-being. Science curricula are so packed with learning the fundamental, technical details of their fields that, even if such ethical training (unlikely as it is) were relevant, there wouldn’t be near enough time for students to adequately learn the relevant ethical theory.

Gaa responds to this objection by suggesting a link to a similar issue that arises when one considers the ethical implications of new technology. He remarks:

perhaps an effective way of making the requisite determinations would be to emulate or extend the recently emerged field of technology assessment, the purpose of which would be to evaluate
the impact of developments in ethically important areas of research. (1977, p. 535)

This approach would take the task of appraising the ethical impact of scientific inquiry out of the hands of the day-to-day scientist and into the hands of the governmental auditor. However, Gaa elsewhere criticizes this alternative as problematic since it removes an element of self-control from the working scientist who would prefer independence from outside intervention (1977, p. 516). Indeed, it would be much more efficient if the ethical management of scientific inquiry were performed right at the point of scientific innovation by the scientist herself. Looking at the problem in this way, though, conceals an even larger problem for Gaa and for Douglas, and that is to decide what auditing process will be used to ethically assess each scientific development as it comes along. The problem is that this auditing process will at times be ethically complex, and neither Douglas nor Gaa say much to elaborate and defend their preferred ethical orientations (both seem to be consequentialists, but neither openly defend their ethical orientation).

For this reason, Gaa’s and Douglas’ exhortations that scientists should pay attention to the ethical consequences of their research and have in the past often acted abysmally by neglecting their duties rings hollow. Ethical duties are seldom so black and white as they portray them, and special expertise is often needed to determine what our moral duties are. Granted, scientists as moral citizens need to be conscious of how their work affects the public welfare. But when her work does have an effect on the public welfare, what should a scientist do? With reference to the Manhattan project, Douglas comments: “it is clear that we want scientists to think about the potential consequences of error (that is, the potential harms that occur when things do not go as planned)” (2009, p. 78). One couldn’t find a more sensible observation. But when those harms are identified, what should a scientist working on the Manhattan project do? Do we want the scientist to do anything other than make her opinions known? The moral issues brought about by the Manhattan project were enormously difficult to assess, and determining the right responses to these issues is a complicated task. The suggestion that that any scientist working on the Manhattan project had the intellectual wherewithal to correctly address these issues and could be confident in taking the right action, given the complexity of the science, the political situation, and the military exigencies of the day, is startling. Turning to the present day situation, many of the ethical issues that arise in the context of contemporary scientific research and technological innovation are similar in their complexity. This is why addressing these issues is not the sole, nor even the primary concern
of working scientists, but is addressed in an interdisciplinary fashion by scientists, ethicists, political scientists, politicians, sociologists, lawyers and many other stakeholders. Thus, as the third objection to Gaa suggests, it is unreasonable (and even unwise) to centrally place on scientists the burden of ethical assessing their work.

6. Conclusion
In this paper I have defended the value-freeness of science against two arguments, the uncertainty argument and the moral argument. In brief, the uncertainty argument fails because it falsely assumes that the background assumptions guiding scientific reasoning need to be valued. For its part, the moral argument fails for a variety of reasons. Its main flaw is that by giving moral values a role in evidential reasoning we thereby license the disruption of scientific objectivity. At the very least, too little is said by the proponents of value-ladenness to counter this possibility. Moreover, the moral argument (as used by Douglas and Gaa) tends to oversimplify moral issues (e.g., by focusing on clear cases, such as Nazi experimentation and doomsday devices). If the proponents of moral value-ladenness were to focus more directly on the complexity of the moral decisions they recommend scientists make, they might recognize the benefit of separating moral, valuational issues from straightforward, scientific, factual ones.

References


