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The Need for Values in Science: The Contingency Argument

Given these choices, research in the natural and technological sciences cannot be partitioned into one section which is open to situationally contingent selections and contextual influences... and another which consists of the internal, objective and standardised execution of the necessary enquiry. Since choices exist throughout *the process of experimentation, there is no research core which, even in principle, is left unaffected by the circumstances of production.* – Karin Knorr-Cetina[1]

In the doing of science, whether for use or for pure curiosity, scientists must make choices... It is precisely in these choices that values, both epistemic and, more controvesially, nonepistemic, play a crucial role. – Heather Douglas[2]

[1] *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford: Pergamon Press, 1981).

[2] "Rejecting the Ideal of Value-Free Science," in *Value-Free Science? Ideals and Illusions*, ed. Harold Kincaid, John Dupré, and Alison Wylie (Oxford: Oxford University Press, 2007), 120–41.

3.1 The Ubiquity of Contingency and Choice in Science

Scientists are constantly faced with decisions in the course of their practice, unforced choices about how to proceed. What should I investigate? How should I do it? What methods to use, what data to collect, what hypotheses to test? To say that these are choices is to say that there is more than one open option. To say that the choice is "unforced" is to say that no factor decisively settles the matter, shows one of the options to be best all-things-considered, at least, from the perspective of the scientific inquirer at the moment the choice is made. When we are dealing with science, it is natural to think of the things that can force the decision in terms of "epistemic factors," i.e., evidence, logic, and standards for what constitutes a good theory, method, or high-quality evidence as such. Epistemic factors alone fail to fully determine many of the choices that scientists make.

Epistemic factors are any considerations in judgment that refer only to the features of a part of inquiry that make it good for producing knowledge.

Contingencies are steps in the process of inquiry that reasonably could go in multiple directions. Retrospectively, they reasonably could have done differently. They are **decision-points** that could potentially be made the subject of reflective choices.

Other moments in the scientific process do not consist of explicit choices, but they are nonetheless *contingent* moments, i.e., there is more than one way inquirers could reasonably go at that point. Many of the race scientists mentioned in Chapter 1 likely never questioned the assumptions about racial hierarchy imported from society; nevertheless, they reasonable could have questioned them, should have questioned them, and so the continuation of these racist assumptions is contingent. These, too, can be thought of as choices potentially, or in principle. All genuine actual choices are contingencies, but all contingencies are potentially choices, even if in practice they are or must be settled by community-wide conventions.

Using the account of scientific inquiry developed in the previous chapter, we can delineate more clearly the kinds of choices scientists face, in terms of the decision-points that arise in each phase of scientific inquiry:

Problem-selection and framing. What perplexities are worth inquiring into in the first place? (e.g., Should I study treatments for infections diseases plaguing Sub-Saharan Africa or treatments for erectile dysfunction?) How should we identify and frame the problem of inquiry? (e.g., Are we studying the link between *race* and *intelligence*, or are we studying the influence of the complex of socioeconomic, cultural, institutional, and political factors contingently associated with race on educational outcomes?)

Suggestion of hypotheses and concepts What concepts and categories should our analysis rely on? (Should our study use *sex* or *gender*? Should our categories be binary, more than binary, or open-ended?) What hypothesis should we pose for solving the problem? (e.g., Should we hypothesize that primate groups are structured by male-dominance relations, or by more complex relationships involving males *and* females?)

Observation How should we gather data? What methods? (Should we use animal models or human subjects? Should we do laboratory studies or field studies?) How should we characterize the data? (Does this sample contain a tumor or not? Should we model the data linearly?)

Reasoning How should we connect up this question with larger theories and conceptual frameworks? (Do we need to supplement the experimental results with an account of the underlying mechanism?)

Experiment and testing What is the bearing of this evidence on the hypothesis? How strongly does the evidence support / challenge the hypothesis? Is the evidence sufficient to accept or reject the hypothesis? (Should we use significance-testing or Bayesian methods? Should we aim for a p-value of 5% or 1%? How certain are we in our assignment of evidential weights or probabilities to the hypothesis?)

Rather than looking at the functional phases of inquiry, we could also delineate them according to the temporal progression of inquiry, from *planning inquiry* (problem-selection and methodology) and *framing* concepts, hypotheses, and problem-statements, to the *conduct* of inquiry proper (collecting and processing data, reasoning, conducting experimental tests, accepting a hypothesis), to finally the *impact* of science on society, culture, and philosophy. Either way, many of the choices so delineated are unforced. While there many be various epistemic factors in the favor of one way or the other of making a choice, at least at the point the decision is originally made in inquiry, there is no all-things-considered best option.

A potential problem with this way of talking about contingency and choice in science is that it does not always accurately reflect the lived experience of scientists; scientists do not always experience contingencies as decision-points. Rather, they experience themselves as doing things the natural or usual way, as following external constraints, or searching for the right way to do things. Erik Fisher and his collaborators, in their "Socio-Technical Integration Research" (STIR) program, have found that researchers do not always see themselves as decision-makers. However, through repeated engagements about what the researchers are doing, why, and what their options are, they have shown that scientists can learn to recognize the often implicit decision-points in their research, to see themselves as choice-makers, and to reflect on the consequences and stakeholder interests that might be at play in those decisions.[3] Even when they do explicitly make choices in the process of inquiry, they often do not report them *as* choices, merely describing what they they actually did without acknowledging alternative options.[4]

It is through contingency and the choices it opens up that the social and ethical responsibilities of scientists come to the fore. Scientists have special responsibilities as scientists, to their colleagues, their students, their research subjects, and to the facts. But they also share the same responsibilities we all have to consider the implications and consequences of the choices that they make, who might be affected and how. It is here that the need for value judgments in the scientific process arises.

This claim, that scientists need to make value judgments, that values are a part of the scientific process, may strike you as powerfully counterintuitive. Does science not seek to go beyond the subjective opinion, to seek an objective, evidence-based understanding of the world, perhaps even, if we're lucky, getting us closer to the truth? Should we not, then, try our best to keep human values out of the equation? If you share these concerns, you are in good company. Philosophers of science and scientists over the past century at least have worried about the influence of values over science as a destructive, biasing factor. Many have sought to defend the "ideal of value-free science," according to which scientists ought to strive to eliminate the influence of values over science. Of course, scientists are human, and thus imperfect, but the scientific process includes checks and balances that limit the impact of their biases.

These concerns, while understandable, are ultimately mistaken. For

[3] Erik Fisher and Daan Schuurbiers, "Socio-Technical Integration Research: Collaborative Inquiry at the Midstream of Research and Development," in *Early Engagement and New Technologies: Opening up the Laboratory*, ed. Neelke Doorn et al., vol. 16, Philosophy of Engineering and Technology (New York: Springer, 2013), 97–110.

[4] Douglas, "Rejecting the Ideal of Value-Free Science," 123.

one, they are based on a misunderstanding of the nature of values and value judgment, though I will save that argument for the following three chapters. For another, the need for value judgment is an inescapable feature of the scientific process, as this chapter will demonstrate. The burden of the rest of the book is to elaborate a new ideal of values in science according to which the pervasive influence of values on science is neither destructive nor biasing but a positive influence.

3.2 The Role of Value Judgments in Science

In recent years, the view of philosophers of science working on these problems has shifted against the value-free ideal, in favor of the idea that science must incorporate values and value judgments. These arguments have by and large left our understanding of values (at least, "non-epistemic" values) unchanged. In this section, after briefly reviewing the value-free ideal, I distill the core of the strongest arguments against the value-free ideal into what I will call "*the contingency argument*." I then defend the contingency argument against a host of motivations for thinking that science is our ought to strive to be value-free. In the following section, I compare the contingency argument to other arguments for values in science in the literature.

3.2.1 The Value-Free Ideal

While aspects of the notion that science should be value-free are older,[5] the current formulation of the value-free ideal for science can be traced to two distinctions made in the middle of the twentieth century. The first is between the "logic of discovery" or "context of discovery" and "logic" or "context of justification." Everything involved with the messy, human processes by which inquiry actually proceeds was lumped with the former. The logic of justification, by contrast, concerned only the relations between the evidence and the theories that inquiry produced, which could be evaluated independent of that messy process. In other words, it concerned only the moment of inference. It was thought that values were inevitable in discovery, but that values would play a corrupting role in the latter.[6]

The second key distinction is between the ordinary, extra-scientific type of values (social, ethical, political, religious, etc.) and so-called "epistemic values." These are epistemic factors that are important to evaluating scientific theories, such as simplicity, precision, or fruitfulness for future research, or to evaluating the fit of theory and evidence, such as accuracy and scope.[7] They are understood as values because they are not explicit rules or criteria, but factors that are applied somewhat more loosely, requiring evaluation and judgment calls, weighted differently by

[5] Robert Proctor, *Value-Free Science?: Purity and Power in Modern Knowledge* (Cambridge, Mass.: Harvard University Press, 1991).

[6] Heather Douglas, *Science, Policy, and the Value-Free Ideal* (Pittsburgh: University of Pittsburgh Press, 2009), Ch 3; Heather Douglas, "Values in Science," in *The Oxford Handbook of Philosophy of Science*, ed. Paul Humphreys (Oxford University Press, 2016).

[7] Thomas S. Kuhn, "Objectivity, Value Judgment, and Theory Choice," in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 320–39; Ernan McMullin, "Values in Science," in *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association 1982*, ed. Peter D. Asquith and Thomas Nickles (East Lansing, MI: Philosophy of Science Association, 1983), 3–28; Heather Douglas, "The Value of Cognitive Values," *Philosophy of Science* 80, no. 5 (2013): 796–806.

different scientists. Within the context of justification, it was thought that these epistemic values would be fine, while the rest, "nonepistemic values," would be corrupting. Since what we've called the "value-free ideal" really says that only epistemic values are allowed in the context of justification, we might instead want to call this ideal, "the ideal of epistemic purity."[8] I will, however, stick with conventional phrase.

[8] Justin Biddle, "State of the Field: Transient Underdetermination and Values in Science," *Studies in History and Philosophy of Science* 44, no. 1 (2013): 124–33, `http://dx.doi.org/10.1016/j.shpsa.2012.09.003`.

3.2.2 The Contingency Argument

The Contingency Argument demonstrates the pervasive value-ladenness of science based on the role of contingency and choice in the scientific process.

The contingency argument depends on the role of decision-points in science described above, and goes as follows:

1. Scientific inquiry has many contingent moments.
2. Each contingent moment is a decision-point, i.e., it is potentially an unforced choice.
3. These choices often have implications and consequences for things that we care about, including ethical and social, as well as political, cognitive, and aesthetic values.
4. Value judgments should settle choices with ethical and social implications & consequences.
5. Thus, value judgments should be used to decide scientific contingencies.

The ubiquity of contingent moments in science tells us there are decisions to be made. In general, there is the possibility that the decisions will affect things we value, through the implications of those choices or their consequences. There's a general moral responsibility to consider ethical and social implications and consequences of our decisions, if they can be foreseen. If what you're doing affects other people or what they care about, you ought to take that into consideration. Therefore, scientists have a responsibility to make value judgments about scientific contingencies, and everywhere science is genuinely contingent, it is in a sense value-laden.

As described in §3.1, contingencies are any moves or moments in inquiry that are genuinely open, where reasonable inquirers could disagree about the way to proceed. While there may be various considerations that bear for and against the options, there is no factor available to the inquirer at the time the choice must be made that decisively settles the question. Of course, in practice, they are settled, and often without any consideration or realization that they are open. Yet every genuine contingency is a decision-point in that it could be taken up as an explicit decision. There is a decision point wherever there are open options about which scientists could *potentially* make an explicit decision. Though it may be conventional practice in the lab to use one measurement technique, if there are other reasonable techniques that could legitimately be used, the use of the one technique is in a certain sense a choice, even if it is made habitually or unreflectively. Insofar as values are relevant to the choice, that choice is (normatively speaking) *value-laden*, even if no explicit consideration of values was made in making that decision, even if there was no explicit decision at all.[9] Scientists should be more sensitive than they are to these decision-points and the options that exist for their practices; this should be a central aspect of training in "research ethics" or "responsible conduct of research."

[9] The importance of recognizing that these claims are normative ones is made clear by Daniel Steel, "Climate Change and Second-Order Uncertainty: Defending a Generalized, Normative, and Structural Argument from Inductive Risk," *Perspectives on Science*, forthcoming.

Once these decision points are recognized, it follows from our ordinary responsibilities to consider the implications and consequences of our actions and decisions, that scientists must make value judgments in the course of scientific inquiry.[10] Nothing about the practice of science or the profession of scientist absolves or screens the scientist from these ordinary responsibilities.

[10] One of the important virtues of Heather Douglas's work on values in science is that she emphasizes that much of the role that values play in science flows from the ordinary responsibilities of scientists as people, not from their special duties as scientists.

3.2.3 Practical Reasons and the Activities of Inquiry

According to the image of science described in the previous chapter, the practice of science consists first and foremost of inquiries into problems of prediction, explanation, and control. The distinctive value of science is two-fold: first, its tools and techniques, empirical and conceptual, are particularly adept at resolving problems and inadequacies that arise in our attempts to predict, explain, and control the world around us; second, science helps us anticipate such problems and inadequacies through its attempt at systematicity. Though science also involves more foundational or synoptic aspects—such as the attempt to provide a theory-of-everything or to outline a complete scientific worldview—those are relatively peripheral aspects of scientific practice, dependent on the success of the central practices of pragmatic, problem-solving inquiry.

The practice of scientific inquiry, as a practice, consists of distinctive types of actions, e.g., the action of choosing concepts, of proposing hypotheses, of collecting evidence, of accepting, rejecting, inferring, asserting, or endorsing hypotheses. The decision-points in each of these activities are decisions about how to act. As such, these decisions require *practical reasons*—and practical reasons are typically values. If I pursue a certain hypothesis, I need to know not only the epistemic virtues of the hypothesis, or the information that makes it plausible, but a reason or motivation to do so—perhaps I think that the hypothesis, if true, will help some good to be achieved; perhaps I deem in most likely to find support, and I have reason to play it safe in hopes of getting results for sure; perhaps it occurred to me first, and I feel any hypothesis will do. Or, if I am trying to decide whether to accept and publish my results, the fact that the hypothesis has a certain amount or strength of evidence in its favor does not compel me to assert it, *no matter how strong the evidence*. I need a further reason to take the affirmative step of asserting it, whether that be an ethical value, or the desire for credit, or mere whimsey.

The contingency argument is a kind of negative argument: we have responsibilities because our choices have implications and consequences for things we care about, and so we must choose carefully. But recognizing empirical science as a form of practice, of practical inquiry, provides a complementary, positive argument, which we might call *the practical reason argument*. Because each contingent moment involves a decision about what to *do*, it requires or implies a value that motivates that course of action. Both the contingency argument and the practical reason argument build from the ubiquity of choice and values in science. Wherever decision-points exist in a practice, practical reasons are needed as a factor in the decision. All decisions to act, explicit or implicit, require consideration of practical reasons, that is, of values. Those reasons may be obscure to the non-specialist in highly technical inquiries, but they exist all the same.

The Practical Reason Argument argues that since settling contingencies in science consists of making choices between courses of action, and actions require or imply practical reasons (values), every such choice is value-laden.

There is a potential problem in the view sketched in this section and the last. As I have defined decision-points, it may seem as if there is an endless series of such points, and the demand for their explicit recognition and reflective value judgment about each will quickly bring inquiry to a standstill. Habit is as important to the functioning of inquiry as reflection; the smooth functioning of inquiry, as with any practice, requires that some potentially open questions remain tacit, intuitive, implicit. The recognition of decision-points is an open-ended matter, but open-endedness need not become endlessness. What is required is that inquirers not only be sensitive to any decision-points, but that they be sensitive to *significant* decision-points, points rendered significant by their meaning and impact on our values.[11] But there is already built in a kind of built-in check on this issue, as the decision to continue identifying and explicitly reflecting on contingencies itself has consequences for things that we care about, especially if it slows inquiry to a halt.

[11] Compare Philip Kitcher, *Science, Truth, and Democracy* (Oxford University Press, 2001) on "significant questions."

3.2.4 The Contingencies are Epistemically Significant

The defender of value-free science might acknowledge the role of choice presupposed in these arguments, but attempt to confine the role of values in such choices to mere feature of the process of scientific discovery without any impact on the *content* of science. We might acknowledge that values play a significant role in every step of science, up to and including the decision to assert some result by publishing it in a scientific journal. But these are all just part of the process of discovery; the philosophically significant questions have to do with the content, not the process.

What matters, on this view, are the products of inquiry—evidence, analysis, argument, hypothesis, theory—and the logical relationships between them. Values have no role to play here. The guardian of the epistemic purity of science will point out that values do not appear in scientific reports and publications; whatever the role of values in the process, the results are intended to stand on their own. The strength of evidence for the hypothesis, the quality of the analysis, the soundness of the argument are what matter to the credibility of the science. Whatever idiosyncratic role the scientist's values play drops out.

Philosophers of science in the middle of the twentieth century posed the distinction between the "context of discovery" and "context of justification," and many will no doubt recognize its reappearance here. Subsequently, many have come to reject the distinction on grounds independent from our discussion of values in science. While the distinction was used to limit philosophy of science to the logical analysis of the content of science, these philosophers have exhibited the philosophical richness in the history and practice of science. As an objection to the thesis that science is value-laden, there are at least two problems with the context distinction.

First, the products of inquiry are clearly contingent on the decisions made during the process of inquiry. We can only assess the evidential support for the hypotheses and theories that are actually proposed. We can only assess that support relative to the base of evidence that is actually gathered. If we grant that the process of theory generation is value-laden, even if the comparative assessment of two theories were totally value-free, rational, and done in light of a large base of evidence, the chosen theory will be value-laden. Even if the decision procedure in the context of justification is value-free, in this case it only tells us which is the epistemically better among the available alternatives, each of which is value-laden.[12] A series of such choices will result in a value-laden pattern of knowledge. And values further pattern our knowledge in terms of what problems we do and do not attempt to research, what concepts we choose to use, a variety of decisions that produce the evidence, and so on.

[12] Kathleen Okruhlik, "Gender and the Biological Sciences," *Canadian Journal of Philosophy* 24, no. S1 (1994): 21–42 works this argument out in detail with reference to the role of values related to gender in the biological sciences.

Second, we should not concede the point that the judgments of credibility in the context of justification are in fact value-free. The evaluation of the products of inquiry is just as much a matter of unforced choice as any decision in the process of inquiry. The decision to accept or reject a hypothesis on the basis of the evidence is an unforced choice, because such decisions are always (in non-trivial cases), ampliative or inductive, thus uncertain, thus unforced. Even if you hold, as Richard Richard C Jeffrey[13] did, that it is not the job of scientist to accept or reject hypotheses, but only to assign probabilities to a hypothesis (and its negation) in light of the evidence, the choice is still value-laden. First, there is second-order uncertainty, i.e., uncertainty about the assignment of the

[13] "Valuation and Acceptance of Scientific Hypotheses," *Philosophy of Science* 23, no. 3 (1956): 237–46.

probability to the hypothesis; such uncertainty implies unforced choice, which in turn implies value-ladenness. Second, there are risks associated with the pragmatics of probability assignment, and the choice to make a probability assignment, rather than to accept or reject, can definitely impact how results are communicated and understood by various audiences. If one attempts to replace a precise probability assignment with a range over a confidence interval (or credibility interval for Bayesians), then there is the problem of third-order uncertainty, and even greater risks in the pragmatics of communicating probabilities. What's more, there is the meta-level choice about whether to assign probabilities as a Bayesian, frequentist, likelihoodist, etc., which, given that they are all live options in the philosophical and statistical literature, clearly seems like an unforced choice.

3.2.5 These Decisions Cannot be Deferred

Another way to oppose the contingency argument for the value-ladenness of science, and to recapture some central element of the value-free ideal, is the *deferred decision response*.[14] The deferred decision response acknowledges that value judgments are necessary to many decision-points in science. What it hopes to show, however, is that the value-laden decisions can be deferred, held open during the course of inquiry, and settled only after the fact, once inquiry has concluded, by the relevant parties who are applying the results. The Jeffrey response can be reinterpreted as a very basic form of this strategy, as it argues that the decision to accept or reject a hypothesis be deferred to whoever seeks to *apply* scientific knowledge, not to the scientist themselves.

[14] The concept of the "deferred decision response" was first worked out with my co-author Joyce Havstad. See references below.

One serious concern that motivates some version of the deferred decision response is what we might call "*the democratic objection*" to value-laden science. The worry comes from the special role that science is accorded in our societies, its unique epistemic authority in matters of public policy, education, technology, and culture. If science is value-free, this mean it is neutral between the many competing value-commitments that our pluralistic, democratic society contains, and thus its authority poses no problem for democratic legitimacy. If we allow science to be value-laden, and we allow scientists to apply their own idiosyncratic value judgments to the various decision-points, then the authority of science becomes problematic, as science itself becomes partisan, and its loses its democratic legitimacy. We stand between the dangers of technocratic tyranny or losing the valuable tools that science provides.

More sophisticated versions of the deferred decision response have been proposed than the Jeffrey version. For instance, science policy scholar Roger Pielke, Jr.[15] has proposed a model of science advising for policy that he calls "*the honest broker of policy alternatives*." On

[15] *The Honest Broker: Making Sense of Science in Policy and Politics* (Cambridge: Cambridge University Press, 2007).

the honest broker model, the appropriate role for scientists is not to settle questions but to multiply plausible policy options, where the range of alternatives provide scientifically justifiable means to a range of policy ends, with the ends to be filled in by democratically accountable policymakers.[16] A related but even more sophisticated deferred decision approach can be found in Ottmar Edenhofer and Martin Kowarsch's[17] *pragmatic-enlightened model* of scientific policy assessment. Edenhofer and Kowarsch acknowledge more clearly than Pielke the value-ladenness of science, and they use a cartographic metaphor for the multiple paths through the scientific decision-points that must be made to resolve a scientific question. Ordinary scientific inquiry, with all the decisions settled, represents a single path through the landscape of possible choices. The role of the democratically-responsible science advisor, however, is to identify multiple paths through the landscape, each path representing different objectives or values that policymakers might choose.[18] Despite the good intentions and sophisticated nature of these approaches, however, the deferred decision response in all its forms is unworkable.

[16] For critique of the honest broker model, see Matthew J. Brown and Joyce C. Havstad, "Neutrality, Relevance, Prescription, and the Ipcc,"., under review.

[17] "Cartography of Pathways: A New Model for Environmental Policy Assessments," *Environmental Science & Policy* 51 (2015): 56–64.

[18] For a sympathetic critique of the pragmatic-enlightened model, see Matthew J. Brown and Joyce C. Havstad, "Inductive Risk, Deferred Decisions, and Climate Science Advising," in *Exploring Inductive Risk*, ed. Kevin Elliott and Ted Richards, forthcoming. The criticisms below are based on ideas worked out in that paper.

The first problem with this response is that the ubiquity of decision-points in inquiry makes deferral impracticable. An implicit assumption of each version of the deferred decision response is that the relevant decisions are few enough that a manageable number of options can be presented to policymakers, school boards, members of the public, or whoever is making decisions based on the information scientists provide. But there are many decisions, large and small, throughout the scientific process, and so just as many potential places that value judgment enters. It cannot be decided in advance whether any particular decision-point is socially relevant, and there are generally many such potential moments.

What's more, there is no reason to think that a small number of objectives or values can be used to chart pathways through these decisions. For example, what research questions to focus on depends on value judgments about which type of knowledge it is useful to have. Do we want more pharmaceuticals, or more knowledge about the benefits of diet and exercise? Decisions about what is permissible with animal research subjects depends on values related to animal rights and welfare. Decisions about whether to publish certain results depends on assessments of, among other things, the consequences of error[19]. There is no reason to think that one's views regarding what is useful to know, on animal rights, and evaluation of the consequences of a specific error are closely correlated. The pathways through the landscape multiply and multiply.

[19] Richard Rudner, "The Scientist Qua Scientist Makes Value Judgments," *Philosophy of Science* 20, no. 1 (1953): 1–6; Douglas, *Science, Policy, and the Value-Free Ideal*.

What's more, the technical nature of many of the decision points makes expert judgment ineliminable. In order to replace expert judgment with the judgment of a policymaker or other layperson requires not only that they fill in their own values, but that they understand what is at stake well enough in order to do so. In order to understand, they must become as technically sophisticated as the scientist, which seems impracticable, or the issue must be sufficiently explained or simplified so that they can make a judgment, which risks oversimplification and distortion. For example, when it comes to the complex decisions concerning modeling assumptions in climate science, the idea of deferring the decisions, and of non-experts understanding precisely what is at stake in each decision, is at least impractical.[20]

The democratic objection raises an important issue; we want our policymaking to be democratically legitimate, accountable to the public, and representative of the range of our values. This legitimate concern, unfortunately, cannot be addressed through the deferred decision response; matters are not that easy. There are too many decisions, and technical expertise is ineliminable from making judgments about most of them, in practice if not in principle. Part of the difficulty comes from asking an epistemic ideal to do a ethical-political job. What will make science advising democratically legitimate has to do with the influence of public values on science (discussed in Chapter 5), the role of values in the social structure of science (Chapter 10), and the way science is applied through technology and policy (Chapter 11).

In one of the least well-appreciated discussions in Heather Douglas's *Science, Policy, and the Value-Free Ideal*,[21] Douglas discusses the epistemic authority of science and the potential problems it poses:

> On the basis of the value-free nature of science, one could argue for the general authoritativeness of its claims. But an autonomous *and* authoritative science is intolerable. For if the values that drive inquiry, either in the selection and framing of research or in the setting of burdens of proof, are inimical to the society in which the science exists, the surrounding society is forced to accept the science and its claims, with no recourse. A fully autonomous and authoritative science is too powerful, with no attendant responsibility... Critiques of science's general authority in the face of its obvious importance seem absurd. The issue that requires serious examination and reevaluation is not the authority of science, but its autonomy. Simply assuming that science should be autonomous, because that is the supposed source of authority, generates many of the difficulties in understanding the relationship between science and society.[22]

The value-free ideal could manage to combine the authority and autonomy of science in a democratically legitimate fashion. Given that the ideal is unattainable and undesirable for the reasons already laid out here (as well as the arguments provided by Douglas), insisting on the social autonomy of science, that is, insisting that it be unaccountable to social and ethical values, is to risk at best its haphazard impact on our society, if not its outright abuse. And we see that abuse often when science is brought into the public sphere.

In Douglas's view, the acknowledgement that science is not and should not be value-free is a step *towards* improving the democratic legitimacy of

[20] See Justin Biddle and Eric Winsberg, "Value Judgements and the Estimation of Uncertainty in Climate Modeling," in *New Waves in Philosophy of Science*, ed. P. D. Magnus and Jacob Busch (Palgrave Macmillan, 2010), 172–97; Eric Winsberg, "Values and Uncertainties in the Predictions of Global Climate Models," *Kennedy Institute of Ethics Journal* 22, no. 2 (2012): 111–37; Kristen Intemann, "Distinguishing Between Legitimate and Illegitimate Values in Climate Modeling," *European Journal for Philosophy of Science* 5, no. 2 (2015): 217–32 for explanations of the value-ladenness of climate modeling assumptions. Gregor Betz, "In Defence of the Value Free Ideal," *European Journal for Philosophy of Science* 3, no. 2 (2013): 207–20 and Wendy Parker, "Values and Uncertainties in Climate Prediction, Revisited," *Studies in History and Philosophy of Science Part A* 46 (2014): 24–30 dispute these claims, but I think Steel, "Climate Change and Second-Order Uncertainty." clearly shows that, taken as a *normative* argument rather than a merely descriptive one, climate modeling involves values.

[21] *Science, Policy, and the Value-Free Ideal.*

[22] Ibid., 7–8.

science, especially science advising and policy-relevant science. Douglas devotes the last two chapters of her book to carefully considering the ways that scientific integrity and democratic legitimacy can be maintained when value-laden science plays a role in the policy process (these chapters are often missed by those who raise the democratic objection to Douglas's view). It will be an important matter to keep in mind as we consider the source and role of value judgments in science in later chapters.

3.2.6 The Responsibilities of Scientists qua Scientists

Another strategy for responding to the contingency argument is to question its third premise, by denying that scientists have a responsibility to consider the full implications and consequences of their actions when doing scientific inquiry proper. Something about the responsibilities associated with the scientists' special social role, or the special norms of scientific practice, exempts the scientist from their ordinary, general moral responsibilities. This *moral exemption response* to the contingency argument usually draws a distinction between "pure" or "basic" science and "applied" science, or between scientific inquiry aimed at belief and scientific inquiry aimed at acceptance (for a purpose). When it comes to creating scientific knowledge, the only responsibilities scientists have are the ones distinctive to scientific practice, mainly epistemic concerns.

On this account, scientists may have to make value judgments when they are interacting with the public (as in science advising, science communication, education, application), but when they are engaged in scientific practice as such, their responsibilities are to the practice itself. Real science is pure science, focused only on creating knowledge. The main responsibility of scientists is to the evidence, and ultimately to the truth. They also have special responsibilities to their colleagues, their students, and their research subjects. But they do not have larger social responsibilities in the main part of scientific practice. Values come in only in the context of application.

There are several problems with this approach. First, the distinctions between types of science or scientific inquiry that the approach presupposes are untenable. There are not two types of scientific inquiry or method, one for pure science and one for applied science. There are of course differences between every field and every specific inquiry, but the differences do not support such a weighty distinction. What's more, the linear model of pure science to applied science to application is untenable. According to this model, pure science produces scientific knowledge, and applied science works out its consequences for particular contexts of application, in a form that can be used by technologists, policymakers, or the public.[23] Several decades of research on the nature of engineering science, technological innovation, and the role of science

[23] Some would add another step of "translational science" between applied science and application proper.

in policy have undermined the linear model. It turns outs, the workings of applied science and engineering are a lot more complicated and interesting than the linear model suggests; in many cases, applied science independently generates new knowledge in which basic science plays a minor or nonexistent role. Applied science even drives developments in basic science.

There is an implicit and problematic value hierarchy implicit in the way the moral exemption response uses the distinction between basic and applied science. The retort to the contingency argument's claim that science is value-laden is something like, "Well, only applied science." Implicitly, pure or basic science is treated as the only "real" science. Not only is this hierarchy increasingly difficult to maintain, given the more realistic pictures that are emerging of the relation between the areas we pick out as "applied science" and "pure science," but the valuation actually seems topsy turvy. It is success in application, in growth of prediction and control of the world, that seems the clearest marker of progress in science. It is much more difficult to assess progress for pure science, and some have argued that there is no adequate measure of progress for pure science.[24] It is at least tendentious to treat "pure science" as if it is the whole of science, or even the central case; "applied science" involves a lot of science, perhaps even what is most central and valuable in science as a whole, and such science is undisputedly value-laden.

[24] Heather Douglas, "Pure Science and the Problem of Progress," *Studies in History and Philosophy of Science A* 46 (2014): 55–63; Daniel Sarewitz, "Saving Science," *The New Atlantis*, no. 49 (2016): 4–40.

Second, even science for its own sake has external impacts on things that we value. For instance, consider the impact of Darwin's evolutionary theory on society. As Philip Kitcher[25] shows, public awareness of the theory was highly disruptive; many figures who were absolutely convinced by Darwin's arguments became demoralized and melancholy at how they understood the view to refute their religious worldview. Furthermore, there was cultural uptake of the theory of evolution by Social Darwinists and the eugenics movement. So, too, the public reception of the quantum theory has created some significant social impacts; some are quite comforted by the return of mystery or agency they think it brings to the world, while others are quite despondent at the way they believe the theory robs the world of order and sense. We need not endorse the popular interpretations of these basic theories in order to acknowledge them as impacts of the theory, some of them anticipatable (some of them actually anticipated by Darwin and by physicists). Nor should we conclude from the fact that there are values issues that should be taken into account, that therefore there is something wrong about what Darwin or Heisenberg did in publishing their theories. Generally, the public reads basic science, the media reports on it, and it has an impact; the open decisions made throughout the scientific impact should therefor factor in that impact.

[25] *Science, Truth, and Democracy.*

The moral exemptionist may reply that this begs the question. It may be true that science for its own sake has broader impacts, but those impacts should not be a consideration for scientists, whose role is to seek evidence and truth. The side effects of that search are not their concern. We should be very careful here; there are very few roles in society that allow those in the role to ignore the effects of their actions on others. Lawyers, for example, are protected by attorney-client privilege, even when keeping that information secret has a harmful effect on society. Likewise with doctors and therapists with their patients. Their role involves a duty of confidentiality that prevents them from disclosing information that in other circumstances they might be required to divulge.[26]

[26] Note, however, that this privilege is not in any case unlimited.

But scientists are not like lawyers, and should not want to be. What makes attorney-client privilege work is that lawyers are part of an adversarial system, where the defense and the prosecution argue opposing sides of a question, where their arguments are judged by impartial third parties. If attorneys could be required to testify about everything they know about their client, it could easily undermine this process.[27] Doctors owe confidentially to their patients because of the special autonomy and rights to privacy patients have concerning their own body. Scientists do not have such roles or responsibilities. What begs the question is the assumption that the role of values in science *necessarily* interferes with the search for truth. Though it is the case that *some* uses of values so interfere, proper normative guidance for the use of values in science aims to prevent such a conflict, even to show how value judgment can promote the search for truth, as I will show at length in later chapters. There are no special aspects of the job of scientists that exempt their ordinary moral responsibilities. What's more, as science is largely a publicly supported enterprise, and scientists have a special role as trustees of our social store of knowledge, one could argue that they are *more* responsible than the average private citizen to consider the social and ethical impact of their actions.[28]

[27] Cf. Douglas, *Science, Policy, and the Value-Free Ideal* p. for a more detailed discussion of and response to the legal analogy.

[28] For more on the idea of science as a public trust, see Mark B. Brown, *Science in Democracy: Expertise, Institutions, and Representation* (Cambridge, Mass.: MIT Press, 2009); Matthew J. Brown, "The Democratic Control of the Scientific Control of Democracy," in *EPSA11 Perspectives and Foundational Problems in Philosophy of Science*, ed. Vassilios Karakostas and Dennis Dieks (Dordrecht: Springer, 2013), 479–92; Matthew J. Brown and Joyce C. Havstad, "The Disconnect Problem, Scientific Authority, and Climate Policy," *Perspectives on Science* 25, no. 1 (2017): 67–94.

What would be necessary to insulate basic science and scientific practitioners from the responsibility to consider values would be, in fact, a radical change to the institutions of science and the role of science in society. What would be necessary is something like the complete seclusion of science from society, except in approved forums with extra levels of oversight from representatives of the public. Basic science would have to be developed in secret, kept hidden from the public, so that its impact on society could be minimized. This could successfully screen scientists from the responsibility to make value judgments. To continue to screen scientists from this responsibility as science was applied or made public, separate boards of overseers would have to examine the kinds of questions the contingency argument raises, and make the relevant value judgments on behalf of the public, so that the scientists could avoid them. It seems to me that the downsides of such a radical change are obvious and decisive.[29]

[29] Douglas makes a similar argument in her forthcoming Descartes Lectures.

3.3 Comparison with Other Arguments

As I have said, *the contingency argument* attempts to distill work from a variety of directions that challenges the value-free ideal. In order to better understand the argument, then, I will discuss a variety of other approaches to the value-ladenness of science, and compare them to the contingency argument.

3.3.1 The Descriptive Argument

One historically influential argument against the ideal of value-free science is *the descriptive argument*, which relies on the results of historical, sociological, or psychological investigation into science or scientists. These investigations show that, in fact, there are a variety of influences of values on science, and they raise the question about whether value-free reasoning is possible for human beings. Since science, as it exists, is in fact value-laden, and since it may not be possible for humans to reason in an "unmotivated" or value-free way, the ideal of value-free science is at best irrelevant.

NEEDS WORK: This argument played an important role in early feminist science studies[30] [Give some real examples.] It has also been used by sociologists of science. [More examples.] Discussions related to motivated reasoning form another kind of descriptive argument against the value-free ideal. [Expand]

[30] See Douglas, "Values in Science."

This argument is not related closely to the contingency argument. But it also does not deeply challenge the value-free ideal. The value-free ideal is a normative ideal for scientific practice, not a description of what scientists are actually doing. Unrealized or even unrealizable ideals can provide valuable guidance to practitioners. Striving to exclude values from their reasoning, introducing social checks and balances on the idiosyncratically biased reasoning of individuals, and conventional standards that avoid potentially value-laden questions are all ways that scientists can be guided by the value-free ideal even if they do not reach it. Although values are to some degree ineliminable, they are regarded as biasing factors whose impact should be minimized.[31] The contingency argument represents a stronger alternative to the value-free ideal *qua ideal*, according to which it is not even desirable for scientists to strive to be value-free.

[31] I dispute the view that all values are biases in Chapter 4.

3.3.2 The Conceptual Argument

Another classic argument for the value-ladenness of science is *the conceptual argument*, which proceeds as follows:

1. Scientists face must choose the concepts with which to frame and analyze their subject-matter, as well as definitions of and assumptions about those concepts.
2. Often these concepts have evaluative content or assumptions built in to them, implicitly or explicitly.
3. Everyone, scientists included, has a responsibility to consider the meaning and consequences of their use of evaluative language.
4. Insofar as such concepts are in use, scientists have a responsibility to make value judgments about them in order to guide conceptual choice.

A variety of concepts with evaluative content or assumptions play a role in scientific inquiry, especially in the biological, psychological, and social sciences. Concepts like race, sex, gender, wealth, wellbeing, health, disease, intelligence, family, divorce, abuse, trauma, learning, and many others are ineliminable from science, and involve inextricable combinations of descriptive and evaluative meanings.

Hilary Hilary Putnam[32] follows Bernard Bernard Williams[33,34] in calling such concepts "thick ethical concepts." For example, Putnam considers the case of the concept "cruel" and such concepts as a counterexample to the fact/value dichotomy. Putnam argues that ascriptions of cruelty involve both a value-judgment (in this case, a negative evaluation of the person judged cruel) as well as a description of behaviors or dispositions.

> What is characteristic of "negative" descriptions like "cruel," as well as positive descriptions like "brave," "temperate," and "just"... is that to use them with any discrimination one has to be able to identify imaginatively with an *evaluative point of view*.[35]

Competence in the descriptive use of terms like "cruel" requires familiarity and facility with their evaluative use. Putnam regards Amartya Sen's work in economics as a paradigm example of a scientist meeting their responsibilities when working with concepts that defy the fact/value dichotomy.

John John Dupré[36] considers the case of "violence." Sociologists and psychologists may come up with operational criteria for attributing or quantifying the violence of groups or individuals. And yet the claim that "The United States is a violent country" or "Sam is a violent child" are also express evaluations of the character of that nation or person. While Dupré acknowledges that it may be true that we could replace "violent" with a technical term with the same operational criteria but devoid of the evaluative meaning, it is nonetheless undesirable, because the evaluative meaning is crucial to the *significance* of the science in

[32] *The Collapse of the Fact/Value Dichotomy and Other Essays* (Cambridge, MA: Harvard University Press, 2002).

[33] *Ethics and the Limits of Philosophy* (Cambridge, Mass.: Harvard University Press, 1985).

[34] Though Williams coined the term, he did not originate the idea. Williams himself claims to have picked it up from Philippa Foot and Iris Murdoch in the 1950s. See Putnam, *The Collapse of the Fact/Value Dichotomy and Other Essays*, 159n19 and Pekka Väyrynen, "Thick Ethical Concepts," in *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta, Winter 2016 (`http://plato.stanford.edu/archives/win2016/entries/thick-ethical-concepts/`, 2016) §1 for discussion of potential historical antecedents.

[35] Putnam, *The Collapse of the Fact/Value Dichotomy and Other Essays*, 39.

[36] "Fact and Value," in *Value-Free Science?: Ideals and Illusions*, ed. Harold Kincaid, John Dupré, and Alison Wylie (Oxford: Oxford University Press, 2007), 27–41.

question, and especially its bearing on potential actions. As Dupré puts it:

> Once we move away from the rarified environments of cosmology or particle physics, we are interested in scientific investigations that have consequences for action. And this undoubtedly is why, while often paying lip service to operationalized or technical concepts, scientific language often gets expressed in everyday evaluative language... Evaluative language expresses our interests, which, unsurprisingly, are things we are interested in expressing. When we describe things, it is often, perhaps usually, in terms that relate to the relevance of things for satisfying our interests.[37]

[37] Ibid., 30.

And indeed, it is this relevance for our interests that determines what counts as successful operationalization or technical clarification of our everyday language. Thus a shard distinction between fact and value for most of our scientific conceptual repertoire is both untenable and undesirable.[38] Dupré pursues more detailed examples in evolutionary psychology and economics to demonstrate the point.

[38] Ibid., 31.

Clearly, the conceptual choice argument is an instance of the contingency argument. Conceptual choice is one particular decision-point in scientific inquiry, one of particular consequence for how problems and hypotheses are framed and for how evidence is characterized and analyzed. The evaluative content of thick evaluative concepts is one way in which the meaning and consequences of conceptual choice impact values and require value-judgments. It may be possible for purely descriptive terms to also have consequences for values, though that does not seem generally true in the way it is for thick concepts.

3.3.3 The Underdetermination Argument

Another classic source of arguments for the value-ladenness of science are the problems referred to as "the underdetermination of theory by data." Underdetermination is actually a number of different problems about the relation of theory or hypothesis and data, observation, or evidence, but each version identifies some kind of *gap* between the two, a gap that cannot be filled by traditional logic. Hence, the use of underdetermination in arguments for the value-ladenness of science are sometimes called "*the gap argument*."[39] The gap may consist in merely the uncertainty of any ampliative inference, sometimes called "Humean underdetermination." The gap may be what Helen Longino calls the "semantic gap" between hypotheses and data, caused by the different languages of theory and evidence, or by the lack of "formal relations of derivability," or the fact that data do not come with their "evidential relevance" specified in advance.[40] Or the gap may be a result of the multiple rival theories (explicit or unconceived) that are equally well supported by the data.[41]

There are thus many types of underdetermination or gap argument for the value-ladenness of science. In general they proceed as follows:

[39] Kristen Intemann, "Feminism, Underdetermination, and Values in Science," *Philosophy of Science* 72, no. 5 (2005): 1001–12; Kevin C. Elliott, *Is a Little Pollution Good for You?: Incorporating Societal Values in Environmental Research*, Environmental Ethics and Science Policy Series (New York: Oxford University Press, 2011); Matthew J. Brown, "Values in Science Beyond Underdetermination and Inductive Risk," *Philosophy of Science* 80, no. 5 (2013): 829–39.

[40] Helen E Longino, "How Values Can Be Good for Science," in *Science, Values, and Objectivity*, ed. Peter Machamer and Gereon Wolters (University of Pittsburgh Press, 2004), 132; Helen E Longino, "Values, Heuristics, and the Politics of Knowledge," in *The Challenge of the Social and the Pressure of Practice: Science and Values Revisited*, ed. Martin Carrier, Don Howard, and Janet A Kourany (University of Pittsburgh Press, 2008), 70.

[41] Kyle Stanford, "Underdetermination of Scientific Theory," in *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta, Winter 2009 (`http://plato.stanford.edu/archives/win2009/entries/scientific-underdetermination/`, 2009).

1. A gap exists between scientific theories or hypotheses and the possible or available evidence—because for the need of auxiliary hypotheses linking the two, or because there are alternatives compatible with the evidence, etc.
2. Scientists thus must make choices about how to fill the gap in order to decide whether a hypothesis or theory is supported (or falsified) by the evidence, or to choose among competing alternatives, etc.
3. The different ways of filling in the gap often have implications and consequences for things that we care about, including ethical, social, political, cognitive, and aesthetic values.
4. Everyone, scientists included, has a responsibility to consider the implications and consequences of their choices and actions.
5. Insofar as such implications and consequences can reasonably be anticipated, scientists have a responsibility to make value judgments about them in order to guide decisions about how to fill in the gap.

In this schematic form, the argument is agnostic between different types of gap, and different notions of gap-filling. So, e.g., Longino claims that the gap is filled by in the first instance by a variety of background assumptions about the nature of the instruments, about the ontology of the subject-matter, about proper scientific method and technique (*ibid*), which are to some extent arbitrary, and their choice can legitimately mediated by cognitive and social values. Others have broader, fuzzier notions of what the gap-filling consists in.

[DECIDE: Do I need to discuss Nelson, Anderson, Biddle, Elliott?]

The underdetermination argument can be interpreted as a descriptive or as a descriptive argument.[42]In its normative form, the underdetermination argument for values in science is clearly an instance of the contingency argument. The decision-points consist of the various options for filling in the gap between hypothesis and data. These choices are unforced, because the background assumptions, instrumental choices, and other elements are themselves usually tested only indirectly. On the other hand, the metaphor of *the gap* suggests the possibility of narrowing or closing the gap. As the total evidence base increases, as clever tests between rivals are devised, as linking background assumptions themselves accumulate direct and indirect evidence in their favor, the gap narrows and the relevance of the underdetermination argument for values in science closes. To rely exclusively on this argument is to adopt what I have called "*the lexical priority of evidence*," a problematic principle that I will discuss in the next chapter.[43]

[42] Intemann, "Feminism, Underdetermination, and Values in Science"; Douglas, "Values in Science."

[43] See also Brown, "Values in Science Beyond Underdetermination and Inductive Risk."; Daniel J Hicks, "A New Direction for Science and Values," *Synthese* 191, no. 14 (2014): 3271–95.

3.3.4 The Inductive Risk Argument

The argument from inductive risk (AIR) derives from William James' observation that "Believe truth! Shun error!" represent two different epistemic commandments, and that the two are generally in tension with one another.[44,45] The AIR is one of the most influential and important argument against the value-free ideal of science in the contemporary discussion. This is due almost entirely to the work of Heather Douglas,[46] who has drawn from earlier presentations of the argument by C. West Churchman, Carl Hempel, and especially Richard Rudner. Rudner argues that it follows from the fact that scientists accept or reject hypotheses that they make value judgments:

> For, since no scientific hypothesis is ever completely verified, in accepting a hypothesis the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis. Obviously our decision regarding the evidence and respecting how strong is "strong enough", is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis.[47]

There are different ways to elaborate Rudner's argument, which as Douglas[48] has shown, applies not only to acceptance and rejection of hypotheses, but any of the ampliative inferences in science. In particular, there are two main versions of the argument, depending on whether you focus on the role of *error* or the role of *decision to accept* in the argument.

[44] William James, "The Will to Believe," *The New World* 5 (1896): 327–47; P.D. Magnus, "What Scientists Know Is Not a Function of What Scientists Know," *Philosophy of Science* 80, no. 5 (2013): 840–49.

[45] Magnus, "What Scientists Know Is Not a Function of What Scientists Know." thus refers to the AIR as the "James-Rudner-Douglas Thesis."

[46] "Inductive Risk and Values in Science," *Philosophy of Science* 67, no. 4 (2000): 559–79; *Science, Policy, and the Value-Free Ideal.*

[47] Rudner, "The Scientist Qua Scientist Makes Value Judgments," 2.

[48] "Inductive Risk and Values in Science."

3.3.4.1 AIR 1: The Error Argument

Here is perhaps the most common way to understand Rudner's argument, the way that informs Douglas's earlier presentations of the argument:[49]

1. Scientists make judgments about whether to accept or reject hypotheses.
2. These choices are uncertain (i.e., they involve inductive or ampliative inferences).
3. Because the choice is uncertain, we must make an unforced choice about whether there is sufficient evidence to accept or reject the hypothesis.
4. The choice of standards of sufficient evidence often creates a non-negligible risk of error (e.g., false negative or false positive error).
5. These errors often have implications and consequences for things that we care about, including ethical, social, political, cognitive, and aesthetic values.
6. Insofar as such implications and consequences can reasonably be anticipated, scientists have a responsibility to make value judgments about them in order to guide decisions about standards of evidence.

This is clearly an elaboration of a specific form of the contingency argument. To rephrase:

1. Scientists are faced with unforced choices about whether to accept or reject hypotheses, through their choice of standards for sufficient

[49] Ibid.; Douglas, *Science, Policy, and the Value-Free Ideal.*

evidence, because the choice is uncertain (inductive).
2. Accepting or rejecting a hypothesis has a non-negligible chance of error, and those errors often have implications and consequences for things that we care about, including ethical, social, political, cognitive, and aesthetic values.
3. Everyone, scientists included, has a responsibility to consider the implications and consequences of their choices and actions.
4. Insofar as such implications and consequences of error can reasonably be anticipated, scientists have a responsibility to use value judgments about them in order to guide decisions about standards of evidence.

Note, also, that like the underdetermination argument, AIR 1 has a mechanism through which values become increasingly less relevant to science. Insofar as uncertainties can be reduced by the collection of more evidence, the applicability of the argument decreases. Likewise, the argument is inapplicable in cases where the hypothesis has no significant social and ethical implications, or when they cannot be anticipated. It seems that this form of the AIR, if relied on exclusively, again commits us to a version of *the lexical priority of evidence.* I will argue in the next chapter that we should thus be wary of relying too much on this argument.

3.3.4.2 AIR 2: The Pragmatic Argument

There is another way to elaborate this argument, one which I see, e.g., in Heather Douglas's later presentations of the argument (e.g., in her forthcoming Rene Descartes Lectures):

1. Scientists make choices about whether to accept or reject hypotheses.
2. Evidence, logic, and epistemic values tell us the strength of evidential support for a hypothesis, but that strength is always limited for non-trivial inductive hypotheses.
3. The decision to accept, infer, assert, or endorse a (non-trivial, ampliative/inductive) hypothesis is an action taken under uncertainty.
4. No amount or strength of support necessarily compels us to assert, infer, etc.
5. Instead, we require some sort of practical reason (i.e., values) concerning sufficiency conditions for asserting, inferring, etc.
6. Where there are foreseeable consequences of error, these are among the relevant practical reasons.

This is not only a version of the contingency argument, but fits with the pragmatic strengthening of the argument discussed above. In this version of the argument, the need for values is ubiquitous. Even when analyzing cases from particle physics, such as the identification of the Higgs boson, the argument encourages us to think about the inductive risks, and to search for the practical reasons for accepting a certain sufficient evidence level, 5-sigma or 6-sigma, say.[50] Rather than commit to the lexical priority of evidence, this version of the argument sees the role of values as ubiquitous in science, but playing a systematically different functional role than evidence plays. There is a cost to this ubiquity claim.[51] AIR 1 draws our attention to cases where the social and ethical consequences of science are most significant, and the need for value judgments most pressing. AIR 2 has us attend to them in a more diffuse way. On the other hand, significant issues are less likely to slip through the cracks, decision-points throughout the process become more salient, and less obvious social and ethical consequences and implications may be found. AIR 2 places a higher burden and diffuses our efforts, but it may be a burden worth meeting, and it may be worth focusing more of our efforts.

[50] Kent W. Staley, "Decisions, Decisions: Inductive Risk and the Higgs Boson," in *Exporing Inductive Risk*, ed. Kevin Elliott and Ted Richards (Oxford University Press, forthcoming).

[51] Thanks to Joyce Havstad for pointing this out to me.

3.4 Why We Need Further Guidance

Having established the value-ladenness of science, the impossibility, undesirability, and irresponsibility of holding science to an ideal of value-freedom and epistemic purity, have we achieved an adequate understanding of values in science? No. The fact that science requires value judgments does not settle the issue of *how* values should be used, and more importantly, how they should *not* be used. There are obvious worries here, about the ways that values can lead to bias and wishful thinking, about the preservation of scientific integrity and objectivity. We need new normative guidance for values in science, now that we have overcome the value-free ideal. However, many previous attempts to provide alternatives to the value-free ideal have failed for lack of an adequate theory of values and value judgments. In the next chapter, I discuss those failings. In chapters 5-6, I provide an account of values and value judgments. In chapter 7, I lay out an alternative ideal that provides the normative guidance we need, and in the rest of the book I show how to apply that ideal in a variety of decision-points in science and in philosophy of science.