PUBLIC TRUST IN SCIENCE: EXPLORING THE IDIOSYNCRASY-FREE IDEAL

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I. INTRODUCTION

What makes science trustworthy? On what basis should the public trust scientists when they claim that smoking causes cancer, that the earth is divided into tectonic plates, or that bee populations are declining? This chapter examines a compelling answer to that question: the trustworthiness of science is based in part on its freedom from the idiosyncratic values and interests of individual scientists. That is, all else being equal, a trustworthy science will be one in which scientists all converge on the same conclusions, regardless of their personal values and interests.

We analyze this answer—dubbed by Marion Boulicault the "idiosyncrasy-free ideal" for science (Boulicault, unpublished; cf. Boulicault, 2014)—by looking at philosophical debates concerning inductive risk. We examine two recent proposals for handling inductive risk, each of which offers a method of avoiding idiosyncrasy: the High Epistemic Standards proposal as put forward by Stephen John, and the Democratic Values proposal as put forward by Andrew Schroeder.[2] We show how each proposal involves different trade-offs, and draw out the implications of these trade-offs for the question of what makes science trustworthy.

First, a note about what we mean by 'trustworthy.' The meaning we have in mind is a deflationary one, what many philosophers would call (mere) reliance. When we ask whether the public should trust science, we only mean to ask whether the public should accept scientific claims as true, or as bases for action. This is a much weaker sense of trust than is common in the contemporary philosophical literature, where analyses of trust often include additional conditions. It is common, for example, to hold that warranted trust requires that the trustor have expectations about the trustee's motives, such as that they include good will towards the trustor (Baier 1986; cf. Almassi 2012; Irzik and Kurtulmus *forthcoming*). We work from this deflationary account because it is our sense that informal, non-philosophical discussions of trust in science often have this account in mind, and also because the two main philosophers with whom we engage employ deflationary accounts (Schroeder forthcoming-a; John 2015, 2017).

II. TRUSTING SCIENCE IN THE FACE OF INDUCTIVE RISK

a. The Value-Free Ideal and the Argument from Inductive Risk

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[2] When referring to the co-authors of this chapter individually, we will, awkwardly, use the third person, as we see no better alternative.

To understand the appeal of the idiosyncrasy-free ideal, it helps to look first to a more familiar ground for the trustworthiness of science. According to the value-free ideal (VFI) for science, science is trustworthy because it deals only in facts, and not values. More specifically, the VFI holds that for science to be trustworthy, non-epistemic values—social, political, and other values that do not "promote the attainment of truth" (Steel 2010, 17)—should not influence justificatory reasoning concerning the truth or falsity of scientific claims (Elliott 2011, 304). The thought is that excluding such values secures a kind of objectivity for science, which is a mark of trustworthiness.[3] VFI proponents readily admit that non-epistemic values can and should influence non-justificatory reasoning, e.g. reasoning about which research program to pursue, or about what to do with our scientific knowledge once we've discovered it. They are also clear that the VFI is an ideal. Scientists are human, and so it may not be possible to entirely cleanse scientific reasoning of the influence of non-epistemic values. But the idea behind the VFI is that, the closer science can get to eliminating the influence of non-epistemic values in justificatory reasoning, the more trustworthy it will be.

The VFI has been vigorously challenged. Some have argued that it is impossible to draw sufficiently robust boundaries between epistemic and non-epistemic values (Douglas 2000, 560; Rooney 1992, 18). Others, particularly feminist scholars, have questioned the assumption implicit in the VFI that facts and values are in opposition to each other (Anderson 1995), and even the viability of the fact/value distinction itself (Nelson 1990). Here, we focus on a particular challenge to the VFI known as the argument from inductive risk.

First formally articulated by Richard Rudner (1953) and C. West Churchman (1956), the argument from inductive risk starts from the fact (which follows from the nature of inductive reasoning) that evidence never deductively entails the truth of a hypothesis. Because of this, scientists face a trade-off between two types of risk whenever they decide whether to accept or reject a hypothesis: if they require more certainty before accepting a hypothesis, they increase their risk of failing to accept a true hypothesis (known as a 'false negative'); if they require less certainty, they increase their risk of accepting a false hypothesis (known as a 'false positive').[4] Crucially, the trade-off between these "inductive risks" is extra-evidentiary: empirical evidence cannot tell you how certain you should be before accepting a hypothesis based on a body of evidence. In Wilholt's words, the decision of how to balance these inductive risks is "underdetermined by the aim of truth" (2013, 252) and therefore must be made by appeal to non-evidentiary factors. In particular, inductive risk theorists argue, it should be made by appeal to the relative importance of avoiding false positives versus avoiding false negatives on some issue, which requires a (non-epistemic) value judgment. Thus, even in the ideal, justificatory scientific reasoning by its very nature cannot be value-free, and the VFI must be rejected.

b. The Idiosyncrasy-Free Ideal as an Alternative Ground for Trust

[3] Though we believe this generally characterizes what a range of scholars have in mind, it is important to note that these concepts are used in a variety of ways in the literature. 'Value' is not always clearly or consistently defined, for example, and there are a number of competing conceptions of scientific objectivity (Reiss and Sprenger 2014). See Douglas (2004), Koskinen (2018), Elliott and Resnik (2014), Schroeder (forthcoming-a), Anderson (1995) and Longino (1990) for discussions of the connections between trust, values, and objectivity.

[4] Technically, the choice is more complicated, involving at minimum a third option of neither accepting nor rejecting the hypothesis, and thus the choice actually involves a trade-off between three factors: the reliability of positive results, the reliability of negative results, and the method's power (which is the "the rate at which a method or type of inquiry generates definitive results, given a certain amount of effort and resources") (Wilholt 2016, 227). For the purposes of this paper, however, all that matters is that an extra-evidentiary trade-off of some kind must be made, and thus we stick to the more simplified formulation of accept vs. reject.

Science's freedom from non-epistemic values has served as a traditional foundation for its trustworthiness. However, if the argument from inductive risk holds, non-epistemic values are essential to scientific reasoning. Does it thus follow that science, even in the ideal, is not trustworthy? Unsurprisingly, this is not the conclusion that most proponents of the argument from inductive risk endorse. Instead, most contend that the argument only shows that value-freedom can't be the right foundation for trust in science. Some inductive risk theorists have proposed alternative foundations for trust. For example, Douglas (2009) endorses a "value-laden ideal" that grounds trust not on the *type* of values (i.e. epistemic vs. non-epistemic values), but on the *kind of role* that values play in science.

In this chapter, we examine a different alternative to the value-free ideal, one that connects trustworthiness not to the influence of *values*, but rather to the influence of *idiosyncrasy*, i.e. of factors (values-based or otherwise) that can vary from scientist to scientist. Boulicault (unpublished manuscript) calls this the *idiosyncrasy-free ideal (IFI):*

> **IFI:** In the ideal, justificatory scientific decisions are made in a way free of idiosyncrasy, i.e. free from the influence of particular features of individual scientists.

The IFI can be traced back at least to Isaac Levi (1960). Levi argued that, even if scientists depend on non-epistemic values in their justificatory reasoning, science remains trustworthy so long as any two scientists, when presented with the same evidence, would make the same decisions, i.e. that "two different investigators [given the same evidence] would not be warranted in making different choices among a set of competing hypotheses" (1960, 356). For Levi, the key to trust in science is therefore not freedom from values, but freedom from idiosyncrasy. Trust in science "does not depend upon whether minimum probabilities for accepting or rejecting hypotheses are a function of values, but upon whether the canons of inference *require of each scientist that he assign the same minima as every other scientist*" (Levi 1960, 356, emphasis added).

More recently, Torsten Wilholt (2013) has provided one of the more detailed accounts of the IFI in the inductive risk literature. Like Levi, Wilholt argues that when faced with inductive risk, what is important is that all scientists balance those risks in the same way. He suggests that the best way to achieve uniformity is through shared methodological conventions–conventions like "only accept a hypothesis with a p value of at least 0.05." If all scientists adhere to the same methodological conventions, then any scientist faced with the same evidence, regardless of her idiosyncrasies, will make the same decisions and reach the same conclusions. On Wilholt's view, trustworthy science is non-idiosyncratic science.

Why think that the IFI provides a good foundation for trust? Why think that idiosyncrasy-free science is (all else being equal) science worth trusting? One reason is that the IFI helps address a major concern voiced by the public when questioning the trustworthiness of scientific research: the concern that scientists manipulate their results to justify their preferred conclusions. Medical researchers have been accused of producing results friendly to the pharmaceutical industry (Bhattacharya 2003.; Fugh-Berman 2013). Oreskes and Conway (2010) describe how the tobacco and oil industries have mounted a concerted effort to present scientific conclusions in ways that promote their bottom lines. Economists regularly accuse one another of allowing political views to dictate their economic conclusions (Jelveh, Kogut, and Naidu 2018; Thoma 2016). And the heart of the so-called "Climategate" scandal was the claim that climate scientists were manipulating their results in the service of a left-wing agenda ("Climategate" 2010).

The IFI can't guarantee that social or political views never influence scientists' conclusions. (To let go of the VFI is to acknowledge that values can legitimately play a role in scientific reasoning.) But it can guarantee that (at least in the ideal) particular scientists can't manipulate results in ways that are favorable to their preferred values. The IFI means that scientists on both sides of an issue–those paid by tobacco companies and those aiming to protect public health; those who attend left-wing political rallies and those at home in politically conservative communities–have to play by the same rules.

Wilholt (2013) also offers arguments in favor of the IFI as a ground for trust between scientists. Adherence to the IFI increases the ease and efficiency with which scientists can understand and assess each other's claims, thereby facilitating trust. If each scientist managed inductive risk in her own idiosyncratic way, then scientists would face coordination problems in deciding whether and how to rely on other scientists' conclusions. They would have to engage in arduous examination of other scientists' methodologies to determine how that particular scientist balanced inductive risk. Inductive risk decisions would be "extremely cumbersome to track and take account of by peers. Not only can value judgments vary considerably from individual to individual, it is also usually difficult to guess another person's value judgments on a given subject matter" (Wilholt 2016, 230-1). Although Wilholt's argument focuses on the case of trust between scientists, it could be extended to the case of public trust. While it might be "extremely cumbersome" for a scientist to determine how another scientist balanced inductive risks, that task could be nearly impossible for a lay person (Scheman 2011). The IFI guarantees a uniformity that can assist the public in understanding scientific results, thereby providing a foundation for trust in those results.

Another reason why the IFI might seem an appropriate foundation for trust is the fact that science is an increasingly social and collaborative endeavor. Wilholt (2016) notes, for instance, that an article on one of the particle detectors of the Large Hadron Collider lists 2,926 authors, and that in the US, the average number of authors per paper in the medical sciences increased from 3.7 to 6.0 between 1990 and 2010.[5] In collaborative research, trust in the group doesn't simply supervene on trust in individuals, and so we need an account of trustworthiness that is group-based. Given that scientific communities are "constituted by shared methodological standards" (Wilholt 2016, 222), it seems plausible that adherence to methodological standards should play an important role in an account of the trustworthiness of science.

III. Implementing the IFI

a. Wilholt's framework

We have seen, then, that the IFI plausibly provides a foundation for public trust in science, and Wilholt suggests that idiosyncrasy can be avoided through shared conventions for managing inductive risk (i.e. for determining the degree of certainty necessary for accepting a hypothesis). But what, specifically, should those conventions be? Might some conventions for managing inductive risk provide better foundations for trust than others?

At some points Wilholt seems to suggest that the answer to this question is 'no.' All that matters is that scientists adhere to *some* conventions for managing inductive risk; the content of those conventions is not especially important:

> [W]ith regard to the aim of facilitating reliable assessments of the trustworthiness of other researchers' results, it is crucial that everyone within the community sticks to the same standards and thus the same

[5] Some might take Wilholt's point further and argue that science is necessarily social and collaborative. Consider, for instance, that modern science is partly premised on the notion of replicability, which in principle requires the participation of more than one scientist. As Helen Longino (1994, 143) would put it, a "Robinson Crusoe" scientist simply isn't possible given the nature of the scientific method. Scientific knowledge is, Longino argues, inherently social knowledge.

> limitations on DIR [distribution of inductive risks], *but not which particular DIR it is that is set as an ideal*. (Wilholt 2016, 231, emphasis added)

He goes on to suggest, however, that the content of the conventions does matter, saying that they should be grounded in value judgments concerning the relative importance of false positives versus false negatives:

> [The conventions] also represent the research community's collective attempt to find the right balance between power and the two types of reliability. In that sense, they also represent an implicit consensus (or at least an implicit compromise position) of the community with regard to the question of how valuable the benefits of correct results and how grave the negative consequences of mistakes typically are for the kinds of research procedures that are subject to the standards at issue. (Wilholt 2016, 231-2; *cf.* 2013, 248)

Ultimately, we think the best interpretation of Wilholt's view is as follows: it is important that conventions lie within a certain range of acceptability, but that within that range, the details of the convention aren't especially important. Wilholt doesn't, however, say very much about how to determine that range of acceptability. Two recent articles, however, do offer more detailed proposals for how such conventions could be set: Stephen John's *High Epistemic Standards* approach, and Andrew Schroeder's *Democratic Values* approach. In the remainder of this chapter we will consider each of these proposals, highlighting their implications for the question of public trust in science.

b. High Epistemic Standards

The lesson of the argument from inductive risk is that evidence does not determine how confident a scientist should be in a hypothesis (60%? 95%?) before accepting it. In other words, it does not determine a standard for balancing the inductive risk of accepting a false hypothesis or rejecting a true one. What, then, should the standard be? According to some—e.g. Rudner (1953) and Douglas (2009)—we should reject fixed standards and instead adopt "floating standards": different scientists may use different standards for balancing inductive risk based on the perceived importance of false positives versus false negatives.

While this floating standards approach has its appeal, it creates a problem for the public trustworthiness of science. If a scientist uses floating standards, I may have legitimate reasons to distrust her conclusions even if I regard her as more knowledgeable than me about the domain at issue. For example, suppose a scientist reports that a particular insecticide depletes wild bee populations. On the floating standards approach, to know whether I should accept the scientist's claim, I need to know her standards—I need to know how she balances inductive risks. If she is relatively tolerant of false positives (perhaps because she believes the collapse of wild bee populations would be catastrophic), then she might endorse that claim upon being 90% convinced it is true. But suppose I am much more worried about false positives (perhaps because I believe that the loss of wild bees would not be so serious, while the lower agricultural yield caused by a failure to use effective insecticides would be devastating). I might think that we should require near-certainty before accepting that an effective insecticide depletes wild bee populations. Thus, it may be rational for me to distrust the scientist's conclusions (even while acknowledging her expertise in the domain).

How might this problem be addressed?[6] John (2015, 2017), echoing Levi and Wilholt, begins by arguing that a critical step to preserving trust in the face of inductive risk is for scientists to reject floating

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[6] Transparency might appear to provide an easy solution: rather than reporting simply that the insecticide depletes bee populations, scientists could report that *given a particular balance of inductive risks*, they have concluded that the insecticide depletes bee populations. Or, relatedly, they could hedge their assertions, saying only that they are

standards in favor of fixed ones, at least when publicly communicating scientific claims. John, therefore, endorses the IFI. His arguments are based primarily on pragmatic considerations concerning communication between scientists and the public. Because there are typically multiple audiences who hold a variety of different epistemic standards and whose identities are *ex ante* unknown to scientists, it is impossible for scientists to tailor their inductive risk decisions so as to keep their value choices in line with all possible users of the scientific knowledge produced (2015, 85). Further, it would take a great deal of time, energy, and expertise for members of the public to dig into the details of scientific reports to determine what particular standards and values the scientists used. Thus, scientific results can be readily interpretable to the public only if they are based on fixed standards (2015, 87-89).

Interpretability, however, is not sufficient for trust. To ground public trust, John argues that those fixed standards must be high—reporting claims only when they are supported by very strong evidence. Only high epistemic standards guarantee (without the need for arduous investigation) that an individual can trust that a particular result meets her own epistemic standards. If I know that scientists demand higher certainty than I would to accept a claim, then whenever scientists report a claim as true, I can safely accept it as well. Thus, fixed, high epistemic standards ground trust by ensuring that scientists' claims are likely to meet most, and ideally all, people's demands for certainty (2015, 88; 2017, 167). On John's proposal, trustworthiness is secured when scientific standards play the role of a very fine sieve, ensuring that only claims that meet nearly everyone's standards are communicated to the public.

c. Democratic Values

Schroeder (forthcoming-a) considers a broader problem than inductive risk: public trust in value-laden science more generally. In addition to managing inductive risk, scientists must make value-laden determinations at other stages of the research process. There doesn't seem to be any value-free way, for example, of creating a comprehensive measure of population health (Murray and Schroeder 2020), or of creating a classification system for instances of violence against women (Merry 2016). For roughly the same pragmatic reasons as John, Schroeder believes that it is important for the trustworthiness of science that the standards used in making these decisions be set in a way that will allow the public to quickly and easily assess their practical relevance. He, like John, argues that this can best be accomplished by taking these decisions out of the hands of individual scientists – as the IFI suggests.

But, also echoing John, Schroeder thinks this isn't enough. Even if all scientists are using the same standards, if the values that lie behind those standards have nothing to do with my values, there is no clear reason for me to trust scientific results based on those standards.

Schroeder's solution to this problem is where he diverges from John. Members of the public often have different values from one another, and so scientists can't choose standards that reflect everyone's values. We are thus faced with a situation where scientists must base their work on values that will reflect the values of some members of the public, while failing to reflect the values of others. In a democracy, when situations arise where important public decisions must be made in a way that reflects some citizens' concerns but not others, we typically (or at least ideally) invoke democratic procedures. Schroeder therefore argues that when scientists must make value-laden decisions in the course of their work, they should typically base their primary conclusions on democratic values–values arrived at through

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90% confident that the insecticide depletes bee populations. Proposals like these have been discussed extensively in the literature. See Jeffrey (1956) and Betz (2013) for canonical defenses of this sort of approach, and McKaughan and Elliott (2018) for a more moderate proposal. For a variety of reasons, we don't think transparency solves the problem of public trust in value-laden science, and the main authors we will discuss agree (Schroeder forthcoming-a; John 2018). Accordingly, we will for the remainder of the paper assume that transparency isn't, by itself, an adequate solution to challenges posed by inductive risk.

procedures that yield a kind of political legitimacy. Schroeder's hope is that even though democratic procedures may yield values that conflict with my own (I may, essentially, get outvoted in my attitudes towards inductive risk or other value choices), I can nevertheless accept scientific results grounded in those values as a basis for certain kinds of actions, if the appropriate democratic procedures were carried out properly.[7]

Take, for example, John's case of insecticides and bee populations. Imagine that I am personally not very worried about the loss of wild bee populations, and accordingly would prefer acting to reduce insecticide use only if the evidence it harmed bees were near certain. If, however, scientists employ epistemic standards arrived at democratically, I might nevertheless accept their claim that insecticides deplete wild bee populations as a basis for my own decision-making about, for example, whether to protest others' use of insecticides, to support policies that permit or ban insecticides, or perhaps even to use insecticides myself. Even if I don't know the precise standards the scientists employed (because I haven't read or couldn't understand the scientific report), the knowledge that those standards were reached through a fair, politically legitimate process may make it reasonable for me to accept the claims that flow from them as a basis for decision-making.

d. High Epistemic Standards, Democratic Values, and the IFI

John and Schroeder offer different proposals for grounding public trust in the conclusions of value-laden science. Each proposal satisfies the IFI, but in different ways. That is, each proposal takes discretion away from individual scientists by telling them how to handle inductive risk choices (for John's proposal) or value choices more generally (for Schroeder's proposal). Thus, each seeks to prevent the idiosyncrasies of an individual scientist from affecting her results.

John's and Schroeder's proposals diverge, though, when it comes to eliminating other forms of idiosyncrasy. In rejecting "floating standards," John isn't only worried about variation between scientists; he is also worried about variation between cases, topics, or political contexts. John's proposal directs all scientists studying the potentially damaging effects of insecticides on bee populations to use the same (high) standards as scientists studying the potentially damaging effects of herbicides on frog populations, and the same (high) standards as scientists studying bee populations in other countries, or at other times. On Schroeder's proposal, however, epistemic standards could vary between the cases, if democratic procedures show that the public has different attitudes towards protecting bee versus frog populations. And they could also vary from political context to political context, if the members of one political community have different values than the members of another political community. John's view therefore prevents the particular features of individual topics, areas of study, or political contexts from impacting the way scientists handle inductive risk, while Schroeder's doesn't. It is thus possible to interpret the High Epistemic Standards approach as not simply a different *implementation* of the IFI, but as embodying a different and more robust *interpretation* of the IFI: on the Democratic Values approach, the term 'idiosyncrasy' refers only to those features that vary between scientists, whereas on the High Epistemic Standards approach, it refers to many additional sources of variation.

This difference, we believe, is useful in framing the strengths and weaknesses of each proposal. As John acknowledges, the most serious objection to his view is that:

> [L]imiting scientists' public assertions only to claims which meet high epistemic standards may leave them unable (properly) to say very much at all… [S]cientists may often be in a position where they are the only

[7] The precise form of these procedures is a complicated matter, requiring careful work in political philosophy. As Schroeder (forthcoming-a; forthcoming-b) notes, however, there is no reason to think these procedures will always or even usually be simple majoritarian ones.

> people aware that certain claims, although not well-enough established to warrant "public" assertion, are well-enough established to warrant action by others in the community. Remaining silent in such cases may seem an unacceptable abrogation of moral duty. (John 2015, 89)

It is uncontroversial that we should sometimes act on the basis of claims about which we are far from certain. Learning that some substance is 70% likely to be a serious carcinogen is plenty of reason to avoid it. Learning that some climate policy is 85% likely to leave many major cities under water is, for most of us, sufficient reason to reject it. Under the High Epistemic Standards proposal, it would be inappropriate for scientists to publicly report such claims, even if the public overwhelmingly would view the information as decision-relevant. This is obviously problematic.

John thinks that his proposal may be able to get around this problem. He suggests that scientists could report less-than-certain, decision-relevant results in private or "unofficial" settings. In this way, the public would be informed, without compromising the high epistemic standards of publicly or officially communicated science. He admits, however, that creating venues for such unofficial communication is "likely to be both practically and morally complex" (John 2015, 90), in part because policy-makers and the public may be liable to confuse (or be confused by) scientists' "official" and "unofficial" statements, in cases where they diverge. We agree, and so we take this to be a serious concern for John's proposal.[8]

In contrast, Schroeder's proposal doesn't have this problem, since it allows standards to float from case to case or context to context (though not from scientist to scientist). When it comes to dangerous carcinogens and the destruction of cities, the public will presumably want to act on probable but somewhat uncertain claims, and so the Democratic Values proposal will permit scientists to report claims that are far from certain. But on matters of low importance, such as the extent to which drinking green tea stains tooth enamel, or on matters of no obvious immediate practical relevance, such as the discovery of an additional moon orbiting Jupiter, it seems likely that (for reasons similar to those given by John) the public will want scientists to adopt much higher epistemic standards.

At the same time, these differing standards also yield some of the most serious problems for Schroeder's view, as they place significant burdens on both scientists and the public. First, any worked-out version of the Democratic Values view will have to answer broad conceptual questions such as: what is the relevant public whose values ought to be democratically assessed on some particular issue? Given the cross-national and international significance of much scientific research, this is a difficult question to answer. Once an appropriate public has been determined, scientists (or others working on their behalf) must then actually determine the values of this public. Schroeder offers some suggestions for how this might be done, including the use of deliberative polling and citizen science programs. But his suggestions are (as he admits) speculative, and implementing them will be challenging, resource-intensive endeavors.[9]

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[8] We believe a second concern with John's proposal, which he does not discuss, is that it can't easily be applied to many other cases in which science is arguably value-laden. Unlike attitudes towards inductive risk, there is no clear way to order many other decision factors on a scale from more to less epistemically conservative. If, as Schroeder and many others have argued, the value choices that need to be made by scientists go beyond inductive risk, and if these value choices also raise issues connected to public trust, then John's proposal will need to be supplemented by another account to handle those choices. Schroeder's proposal, on the other hand, can be extended to all value choices. We set aside this concern in the paper, though, and accordingly will focus only on cases of inductive risk.

[9] For some of the challenges involved in bringing public deliberation into scientific research, see OpenUpSci (2019) and Sample et al. (2019). It is worth noting, though, that the many of the challenges facing the Democratic Values view are no more (and no less) serious than those facing any democratic approach to public decision-making. Thus, in a sense, the success of the Democratic Values proposal hinges on the success of democracy-based approaches to decision-making more generally. Putting this point in the language of trust, the Democratic Values view underwrites trust only to the extent that democratic processes more generally underwrite trust.

The public will also face challenges when it seeks to interpret scientific conclusions grounded in democratic values. If scientists report, for example, that an insecticide depletes bee populations, a member of the public can trust that that claim has been established to the level of certainty demanded by the public. But if she wants to know precisely what that level of certainty is—because, perhaps, she herself holds unusually high or low standards—she will have to dig into the details of the study. Wilholt, John, and Schroeder all agree that this is no simple task, and in many cases the complexity of scientific research may make it close to impossible for most non-experts.[10]

e. Different visions of science

Which proposal—John's or Schroeder's—provides the better foundation for public trust in science? To answer this question, it helps to see each proposal as motivated by a different vision of science. According to one vision, the goal of science is the accumulation of a store of highly certain facts about the world, i.e. a store of truths (or as close as we can get to truths) that we can all rely on with confidence in deciding what to believe and how to act. John's approach is in line with this vision: when scientists employ high epistemic standards, what results is a uniform body of highly certain and thus dependable claims; claims that can be imported and easily applied to different contexts. Schroeder's view doesn't line up so neatly with this vision. His view calls for scientific claims to be tailored to the specific value-laden context in which the claims are produced and communicated. Thus, rather than a uniform body of highly certain claims, Schroeder's approach results in a conglomeration or mixture of claims, each of which is based on context-dependent values.

A second vision of science sees its goal as more to do with action than with fact accumulation. On this view, the goal of science is to improve our lives and to facilitate our interactions with the world around us. Here, the situation is reversed. As we saw earlier, when scientists employing high epistemic standards fail to report a conclusion, it would be inappropriate to conclude that action is not called for. (Knowing that a substance is 70% likely to be a serious carcinogen is plenty of reason to avoid it.) If, though, one accepts the legitimacy of democratic processes, then scientific conclusions grounded in democratic values are arguably ones that we as a public ought to act on. The Democratic Values approach could therefore be interpreted as prioritizing an action-oriented view of science.

Each of the proposals seems to align with a different—and appealing—vision of science. Rather than choosing between the Democratic Values and High Epistemic Standards approaches, therefore, in the final section of the paper we show how they might be combined in a way that builds on the strengths of each.

IV. A Hybrid Approach

a. Our Proposal

We think an attractive implementation of the IFI can be arrived at through a context-dependent trade-off between the more flexible but more resource-intensive Democratic Values approach, and the pragmatically simpler High Epistemic Standards approach. While we don't have the space to work out the details here, we will sketch such a proposal.

[10] One way of putting a key difference between John's and Schroeder's views is that they make different kinds of information accessible to the public. On the High Epistemic Standards approach, the public can quickly ascertain the specific standard used, but it would be very hard for them to know how that standard compares to the public's values. On the Democratic Values approach, the public can quickly ascertain the relationship between the standard employed and the public's values, but it would be much harder for them to know the specific standard used.

First, consider scientific research that has little predictable practical significance. Examples might include the potential discovery of a new moon orbiting Jupiter, or a finding that a particular exercise regimen is marginally more efficient at facilitating weight loss. In such cases, the main drawback of the High Epistemic Standards approach isn't present, since there isn't any serious cost in scientists failing to report a result that is probable but far from certain. Further, in such cases the benefits of the Democratic Values approach aren't especially significant, and so arguably don't justify the resources required to implement it. Overall, then, in cases involving research whose predictable practical significance is relatively small, it seems pragmatically appropriate to omit the intensive public deliberation and input required by the Democratic Values approach in favor of the relative simplicity and transparency of the high epistemic standards approach.

Contrast that with cases where research has clear practical importance–for example, research involving potentially lethal toxins or catastrophic weather events. In such cases less-than-certain information can nevertheless be highly actionable. These are precisely the cases where the High Epistemic Standards approach faces its most serious objections, and where the public input called for by the Democratic Values approach is most critical. For scientists' claims on a matter of great public importance to be trustworthy to the public, it would seem reasonable for the public to insist on having input into the level of certainty the scientists employed. Thus, in such cases, it seems that trustworthiness requires sacrificing the simplicity of the High Epistemic Standards approach for the more complicated but democratically-grounded and action-oriented Democratic Values approach, since what the public ultimately cares about in these cases are the consequences of knowledge–e.g. protection from toxins or extreme weather events.[11]

We think, then, that in cases where research has little to no practical significance, scientists can best ground public trust through the simplicity, transparency, and uniformity assured by only reporting highly certain claims; while in cases where research has great practical significance, they can best ground public trust by deferring to the public to set the levels of certainty their research must meet. What remain, then, are the cases in the middle: cases where there is either widespread agreement that some research has moderately important consequences (e.g., a finding that drinking coffee modestly increases lifetime cancer risk), or disagreement about the practical significance of research (e.g., findings about the precise stage of development where a human fetus can feel pain, or about the likelihood that a development project will lead to the extinction of an endangered lizard species). These cases, we think, pose problems for both John's and Schroeder's proposals.

To address these middle cases, we tentatively propose using a modified version of Wilholt's conventionalism. Recall that Wilholt argues that it is critical that all scientists manage inductive risk in the same way, but that within a certain range of acceptability, it doesn't especially matter what specific standards they use. We propose that within the middle ground we have identified, it is less important whether scientists employ high epistemic standards or democratic values; it is more important that all scientists approach the research in the same way. Scientists collectively need a relatively clear way of distinguishing research questions that should be governed by high epistemic standards from research questions that should be governed by democratic values. But, as long as that standard calls for research of clear high practical importance to be governed by democratic values and research of clear low practical

[11] It also seems likely that in many such cases, some of the more serious objections to the Democratic Values approach lose some of their force. There is presumably less need, for example, to conduct a resource-intensive focus group to determine whether the public wants to know about substances 70% likely to be lethal toxins, and it is more probable that different publics are likely to reach similar conclusions on such issues.

importance to be governed by high epistemic standards, it is much less important how or where it draws the line between the two types of research.[12]

b. Possible Objections

Though we think our proposed hybrid approach is promising as a foundation for public trust in science, it faces challenges. We will briefly comment on two. The first challenge questions whether our hybrid approach can really claim to combine the best aspects of each view–in particular, whether it can capture the advantages of the High Epistemic Standards approach. John argues that it is important for scientists' standards to be fixed, including across cases. It isn't enough for scientists to *sometimes* use high epistemic standards; in order to ensure that scientific results can be readily interpretable by the public, John thinks it critical that scientists *uniformly* employ high epistemic standards. Our hybrid approach allows for the use of lower standards in certain cases. Doesn't it therefore forfeit core benefits of the High Epistemic Standards approach?

We agree there is a tension here, but think the tension is not as serious as it appears. We presume that John would at least allow epistemic standards to vary by scientific field. High-energy physics, for example, typically uses an incredibly stringent five-sigma standard–accepting a chance of error of less than one in three million–when assessing new discoveries (Staley 2017), and it seems to serve them well. It would be virtually impossible, though, to make any discoveries in behavioral psychology that meet that standard. Rather than telling physicists to employ lower standards or telling psychologists they're out of a job, we presume John would permit standards to vary by discipline, since some hypotheses can be explored (and therefore proven) to a much higher degree of certainty than others. To retain the benefits of his High Epistemic Standards approach, therefore, John will need some way to clearly distinguish and flag research employing the extremely high standards characteristic of high-energy physics from research employing standards more appropriate to behavioral psychology. A hybrid approach like ours could use a similar mechanism to clearly mark off research governed by high epistemic standards from research governed by democratic values. That, we believe, could preserve the benefits of the high epistemic standards approach for research governed by those high standards.

A second challenge to our approach questions the practicality of creating a convention of the sort we propose above, in a way that truly avoids idiosyncrasy. Central to the appeal of IFI-based approaches is that they take value judgments out of the hands of individual scientists, grounding trust by ensuring that the justification of scientific claims is independent of individual idiosyncrasies. Our hybrid approach upholds this ideal regarding decisions about inductive risk, but introduces the need for a new kind of decision: decisions about the practical significance of scientific research. We have argued that research of high practical importance should be governed by democratic values, while research of low practical importance should be governed by high epistemic standards. That means, though, that someone must decide, for any given research question, into which category it falls.

We have already suggested conventionalism as a solution here. But can conventions be established in a way that avoids the sort of idiosyncratic judgment the IFI warns against? Our experience with certain aspects of Institutional Review Boards (IRBs) leads us to believe that this can be done reasonably well. In the United States, IRBs are responsible for ensuring that scientific research meets ethical standards

[12] In saying that it is much less important where the line is drawn, we don't mean to suggest that all ways of drawing the line are valid. Distinctions could easily be made, for example, in a sexist or racist way – a real concern, given the long history of sexist and racist research practices – which would obviously be unacceptable. This points to the importance of thinking carefully how such a convention is created. For example, a convention created with the input of a wide range of voices, including voices traditionally marginalized within science, is far more likely to be a just one (see e.g. Longino 1990).

concerning the protection of human research subjects. Most IRBs begin their evaluation process by classifying research into one of three categories: research exempt from review; research subject to expedited review; and research requiring a full review. Although the federal regulations governing this classification are complex, our experience has been that in a large majority of cases there is no disagreement about the proper classification for any particular study.[13] We are therefore hopeful that, in a similar fashion, criteria could be devised that would demarcate research to be governed by high epistemic standards from research to be governed by democratic values. Of course, no criteria of this sort will be perfectly clear. There will be cases in which it is ambiguous which standard should apply to a given research question. But, keeping in mind that the IFI is an ideal rather than a strict requirement, we don't regard that as a serious objection to our proposal. IRB regulations go a long way towards reducing idiosyncrasies in the classification of research as exempt from review, subject to expedited review, or subject to full review. If criteria could be drawn up that did an equally good job of assigning research to high epistemic standards versus to democratic values, we would regard that as a reasonable implementation of the IFI.

V. CONCLUDING THOUGHTS

The driving question of this paper is not a new one. Nor is it specific to science.[14] But questions of trust have a particular character and salience in the context of modern Western science for two reasons. First, science is often portrayed as the ultimate paragon of trustworthiness (Fabiola 2018), to the extent that people who don't trust science are commonly deemed irrational. Second, science is a domain where trust is unavoidable, since the extreme complexity and technical nature of much of modern science means that most non-scientists are unable to directly evaluate scientific evidence (Scheman 2011). No wonder then, that perceived threats to the trustworthiness of science–corporate influences on science (Oreskes and Conway 2010), 'p-hacking' controversies , cases of scientific racism (Carroll 2017; Stein 2015), and accusations of liberal biases in science (Inbar & Lammers 2012)–are so unsettling.

With the growing consensus among philosophers of science that the VFI must be rejected, and worries about a "crisis of trust" between scientists and the public (Czerski 2017), there is an urgent need to understand what grounds or could ground public trust in science. In this chapter, we have considered one general solution to this problem–the IFI–which grounds trust in freedom from the idiosyncrasies of individual scientists. Though that general approach has an important history (Boulicault, unpublished manuscript) and has been discussed and critiqued (Scheman 2011), we don't think it has received the kind of detailed consideration it deserves, particularly in the inductive risk literature. As the significant differences between John's and Schroeder's proposals show, there is more than one way to implement the IFI. We have proposed and briefly explored a third option, combining John's and Schroeder's proposals. We hope that future work will explore other ways of making science idiosyncrasy-free, with the aim of both shedding further light on the IFI as an ideal for science, and on the question of what makes science trustworthy.

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[13] For the regulations, see 45 CFR 46 (2016). Many IRBs produce clearer and somewhat simplified versions to facilitate their own work–e.g. see University of Southern California (accessed Aug 11 2020). To be clear, we are not endorsing all aspects of the IRB process. We focus solely on the initial determination concerning the appropriate level of review, and the extent to which existing guidelines ensure that the decision is made in a non-idiosyncratic way.

[14] Questions of trust can be posed in any instance of epistemic dependence, i.e. any circumstance in which there is knowledge exchange between individuals or groups. For instance, one might wonder what makes the court system worthy of public trust, or why we should trust the claims of journalists.

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