PUBLIC TRUST IN SCIENCE: EXPLORING THE IDIOSYNCRASY-FREE IDEAL

Marion Boulicault (MIT) and S. Andrew Schroeder (Claremont McKenna College)

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Please contact the authors before citing.

I. INTRODUCTION

What makes science trustworthy? That is, on what basis should anyone, and especially 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 one proposed answer to this question: the trustworthiness of science is based at least in part on its independence from the idiosyncratic values, interests, and ideas of individual scientists. That is, science is trustworthy to the extent that following the scientific process would result in the same conclusions, regardless of the particular scientists involved.

We analyze this answer – dubbed by Boulicault the 'idiosyncrasy-free ideal' (IFI) (Boulicault, unpublished manuscript) [1] – by looking at philosophical debates about inductive risk.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 and the *democratic values* proposal. Comparing and contrasting these proposals, we show how each involve trade-offs, and we analyze the implications of these trade-offs for the question of public trust in science.

Before moving to the main argument of the paper, we should clarify that we will be working from a rather deflationary sense of "trust". When we ask whether the public should trust scientists, we only mean to ask whether members of the public should accept scientists' 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). Several philosophical analyses of trust in science have worked from these sophisticated accounts of trust (Almassi 2012; Irzik and Kurtulmus *forthcoming*). We, however, work from the simpler and more deflationary account – what many philosophers would call (mere) deference or reliance -- in part because it is our sense that informal, non-philosophical discussions of trust in science often have this deflationary account in mind, and also because the two main philosophers we will be engaging with employ deflationary accounts (Schroeder forthcoming; John 2015, 2017).

II. TRUSTING SCIENCE IN THE FACE OF INDUCTIVE RISK

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

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

Here is a familiar account of the trustworthiness of science which has received a fair amount of attention from philosophers: science is trustworthy because it deals only in facts, and not values. More specifically, this view, known as the 'Value-Free Ideal' (VFI), holds that if science is to be trustworthy, certain values – social, political and other "non-epistemic" values that do not "promote the attainment of truth" (Steel 2010, 17) – should not influence justificatory scientific reasoning, i.e. reasoning involving the justification of our beliefs in the truth or falsity of scientific claims (Elliott 2011, 304).[2] 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. However, non-epistemic values cannot play a role in a scientist's justification for accepting or rejecting a hypothesis without violating the VFI, and thereby undermining trustworthiness.

It is important to note that the VFI is an ideal. Justificatory reasoning may never in fact be completely free of values. Scientists, after all, are human, and may never be able to entirely cleanse their reasoning of the influence of non-epistemic values. We may therefore never have scientific reasoning that is completely trustworthy (Rudner 1953). But the idea behind the VFI is that, the closer science can get to value-freedom, 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 viability of the more fundamental fact/value distinction upon which the VFI is based (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 is based on 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').[3] 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). The trade-off 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 – 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*

[2] It is worth noting that, in the VFI literature, the term 'value' is not always clearly or consistently defined. For instance, it is not always clear whether 'value' refers to personal preferences or social norms, or whether values can be held by individuals, groups or institutions (or all of the above).

[3] 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 seemed to many to provide the foundation for its trustworthiness.[4] 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 instead 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 response, 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.[5]

Isaac Levi (1960) was one of the first philosophers to propose something like the IFI as a response to the argument from inductive risk (though he doesn't explicitly distinguish the IFI from the VFI) (Boulicault, unpublished manuscript). He argued that, even if scientists depend on non-epistemic values in their justificatory reasoning, the trustworthiness of science is maintained 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" (Levi 1960, 356). For Levi, the key to trust in science is therefore not value-freedom, 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*" (1960, 356, emphases added).

Wilholt (2013) can also be interpreted as endorsing the IFI when he proposes "methodological conventionalism" as a solution to the problem of inductive risk. When faced with inductive risk, Wilholt argues, what is important is that all scientists assess those risks in the same way. He suggests that the best way to achieve uniformity is through shared methodological conventions (e.g. 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 impersonal science.

But why think that the IFI provides a good foundation for trust? In other words, why think that idiosyncrasy-free science is science worth trusting? Wilholt (2013) focuses on the case of trust between scientists, and his primary argument for the IFI is pragmatic. Adherence to the IFI makes science trustworthy because it increases the ease and efficiency with which scientists can understand and assess each other's claims. If each scientist were to manage inductive risk in her own idiosyncratic way, then other scientists would face coordination problems in deciding whether and how to rely on other scientists' conclusions, i.e. they would have to engage in arduous and careful examination of other scientists'

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[4] For examples of philosophers who see the absence of values in science as historically tied to its trustworthiness, see Elliott and Resnik (2014), Schroeder (forthcoming), and Longino (1990).

[5] Naomi Scheman describes something very close to the IFI when she discusses a view of science in which scientists act as interchangeable "surrogate knowers" for all of us (Scheman 2011, 41).

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).

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) observes that the majority of contemporary scientific research is conducted by research groups and communities. (He 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.)[6] He argues that 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 fundamentally community-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.

Finally, the IFI seems suited to at least partially answer one of the major concerns that the public voices when questioning the trustworthiness of scientific research: the worry that scientists manipulate their results to justify their preferred conclusions. Medical researchers have been and continue to be accused of producing results friendly to the pharmaceutical industry (Bhattacharya 2003.; Fugh-Berman 2013). Oreskes and Conway (2010) describe how, beginning in the 1970s, the tobacco and oil industries mounted a concerted effort to present scientific conclusions in ways that promoted 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.

There is much more that could be said about the IFI, particularly regarding its relation to notions of objectivity, its role in philosophical discussions of inductive risk, and the history of how it arose as a distinct ground for trust in science. For instance, although we maintain that the VFI and the IFI are independent ideals, they are not mutually exclusive (i.e. one can maintain both ideals at once) and are likely best understood as conceptually and historically tied. In fact, we suspect that this explains why several philosophers, including Levi, seem to run the two ideals together.[7] However, in the remainder of this chapter, we set these broader points aside and focus on questions tied to *implementation*: how might the IFI be implemented in practice, and what might be the implications for the public trustworthiness of science?

[6] Some might take Wilholt's point further and argue that modern 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.

[7] See Boulicault (unpublished manuscript) for a detailed discussion of these issues.

As we saw above, both Levi and Wilholt argue that the way to avoid idiosyncrasy is through shared conventions for managing inductive risk. (This is surely not the only way to avoid idiosyncrasy, but it is one of the more obvious ways.) But what, specifically, should those conventions be? Might some conventions 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 we adhere to *some* conventions; 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 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:

> [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, Wilholt seems to hold that 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. He doesn't, however, say much about what determines 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* (HES) approach, and Andrew Schroeder's *democratic values* (DV) approach. In the following section we consider each proposal, highlighting their implications for the question of public trust in science.

III. IMPLEMENTING THE IFI: TWO PROPOSALS

a. John's High Epistemic Standards

In a pair of recent articles (2015, 2017), Stephen John looks at the public communication of scientific results, considering how scientists ought to handle decisions involving inductive risk. Suppose, as suggested by Rudner (1953), Douglas (2009), and several others, scientists take a "floating standards" approach to inductive risk: they vary the level of certainty they require before accepting a claim, in response to the perceived importance of false positives versus false negatives versus failures to reach any conclusion. This approach creates a problem for the public trustworthiness of science: if a scientist uses floating standards, I may have legitimate reason to distrust that scientist's conclusions, even if I regard her as an epistemic superior. For example, suppose an acknowledged scientific expert reports that a particular insecticide depletes wild bee populations. On the floating standards approach, to know whether I should accept that claim, I need to know how the scientist's tolerance for epistemic risk compares to my own. 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, while fully acknowledging her epistemic expertise.

What is the solution to this problem? Like Levi and Wilholt, John argues that the appropriate response for preserving trust in the face of inductive risk – at least when it comes to public communication of scientific claims – is for scientists to reject floating standards in favor of fixed ones. John, therefore, endorses the IFI. Like many other proponents of the IFI (e.g. Wilholt), John's arguments for the IFI are based primarily on pragmatic considerations, especially 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 (and thus trustworthy, John implies) only if they are based on fixed standards (2015, 87-89).

Why should those fixed standards be *high* – reporting claims only when they are supported by very strong evidence? Because, John argues, 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.

b. *Schroeder's Democratic Values*

Schroeder (*forthcoming*) considers a slightly broader problem. Rather than focusing on inductive risk in particular, Schroeder considers the question of public trust in value-laden science more generally. In addition to managing inductive risk, Schroeder believes that scientists must also make value-laden determinations when structuring quantitative measures, classifying phenomena, determining study endpoints, and so forth. 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.

Schroeder's proposed implementation, however, is very different from John's. Members of the public often have different values from one another, and thus 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 procedures that yield a kind of political legitimacy.[8] 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, if the appropriate democratic procedures were carried out properly.

[8] Schroeder's proposal has some similarities with Helen Longino's conception that scientific claims should be subject to "transformative criticism," which is a kind of democratic process that she argues is required for objectivity and, therefore, trustworthiness (Longino 1990, 76).

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 whether to protest others' use of insecticides, to support policies that permit or ban insecticides, or even to use insecticides myself. Even if I don't know the precise standards the scientists employed (perhaps because I haven't read or couldn't understand the scientific report), the knowledge that those standards were reached through politically legitimate processes, may make it reasonable for me to accept the claims that flow from them as a basis for public decision-making and, Schroeder argues, some private decision-making.

c. 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. The first thing to note is that each proposal satisfies the IFI, but in different ways. Both the HES and DV proposals take discretion away from individual scientists, by telling them how to handle inductive risk choices (for HES) or value choices more generally (for DV). Thus, each seeks to prevent the idiosyncrasies of an individual scientist from affecting her results.

But HES and DV diverge when it comes to eliminating other forms of idiosyncrasy. In his rejection of "floating standards" for inductive risk, John isn't simply worried about variance between scientists; he is also worried about variance between cases, topics, or political contexts. The HES 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 DV 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. The HES view therefore prevents the particular features of individual topics, areas of study, or political contexts from impacting the way scientists handle inductive risk, while the DV view doesn't. It is thus possible to interpret the HES approach as not simply a different *implementation* of the IFI, but as embodying a different and more robust *interpretation* of the IFI: on the DV approach, the term 'idiosyncrasy' refers only to those features that vary between scientists, whereas on the HES approach, it refers to many additional sources of variance.

This difference, we believe, is useful in framing the strengths and weaknesses of each proposal. As John acknowledges, the most serious objection to the HES 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 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 often plenty of reason to avoid it. Learning that some climate policy is 85% likely to leave many major cities under water is, for many of us, sufficient reason to reject it. Under the HES proposal, it would be inappropriate to report such claims, even if the public overwhelmingly would view the information as decision-relevant. This seems problematic.

John considers whether his proposal might be able to get around this problem, by allowing scientists to report such results in hedged manners, or in private or "unofficial" settings – thus informing the public about such matters, without compromising the high epistemic standards he believes are important for publicly-communicated science. He concludes that creating venues for such communication is "likely to be both practically and morally complex" (John 2015, 90). We agree, and so we take this to be a serious concern for the HES proposal, at least for now.[9]

In contrast, Schroeder's DV 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 major population centers, the public will presumably want to act on probable but somewhat uncertain claims, and so the DV proposal will permit scientists to report claims that are far from certain. But on matters of less importance, such as whether drinking coffee marginally increases cancer risk, or on matters of no 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 the DV view, as they place significant burdens on both scientists and the public. First, any worked-out version of the DV 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 international significance and impact of much scientific research -- consider, for example, climate science -- 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 deliberative democracy exercises and citizen science programs. But his suggestions are (as he admits) extremely sketchy, and it seems likely that implementing them will be challenging, resource-intensive endeavors.[10]

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.

[9] 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 HES proposal will need to be supplemented by another account to handle those choices. Schroeder's DV 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.

[10] For some of the challenges involved in bringing public deliberation into scientific research, see (OpenUpSci 2019; Sample et al. 2019). It is worth noting, though, that the many of the challenges facing the DV 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 DV proposal hinges on the success of democracy-based approaches to decision-making more generally. Putting this point in the language of trust, the DV view underwrites trust only to the extent that democratic processes more generally underwrite trust.

d. Different visions of science

Which proposal – HES or DV – provides the better foundation for public trust in science? To answer this question, it might help to see the HES and DV proposals as each 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. The HES approach seems 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. The DV view doesn't line up so neatly with this vision. The adoption of the DV approach calls for the communication of 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, the DV approach results in a conglomeration or mixture of claims, each of which is based on context-dependent values and levels of certainty.

A second vision of science sees its goal as more to do with action than with faithful representation or 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 HES 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 usually 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 ought, at least as a public, to act on. The DV view could therefore be interpreted as prioritizing an action-oriented view of science over a representational one.

We can sum up this contrast by saying that the HES approach is motivated by a vision of science as aiming at the production a set of highly certain facts, while the DV approach reflects a vision of science that is more practical or action-oriented. If that's right, does it tell us which approach we ought to prefer? Research in science and technology studies suggests that this question might be more complicated than it first appears (Latour 1983; Bensaude-Vincent et al 2011). The line between these two visions of science – one of science as representation and the second of science as control or (inter)action – might be surprisingly porous. That is, it might be that the goals of representation and action/control are inter-related in deep and complicated ways.[11]

Setting aside whether the distinctions between these visions can or should be maintained, it is certainly the case that the HES and DV approaches offer different concrete suggestions for the organization and practice of modern science. Which approach would result in a science more worthy of public trust? Rather than choosing between them, in the next, final section of the paper we show how the two approaches might be combined in a way that we believe has the potential to build on the strengths of each.

IV. A HYBRID APPROACH

a. Our Proposal

[11] In fact, one could imagine a more radical pragmatist interpretation of the DV view (one that goes beyond Schroeder's formulation) that entirely rejects a distinction between representation and action, i.e. it sees representational knowledge as inherently practical, action-oriented and political (e.g. see Jasanoff (2014)). On this more radical interpretation, the distinction between the scientific goal of facts that accurately represent the world, and the goal of acting, manipulating or controlling aspects of the world based on these facts, is blurred.

We think it is plausible that a comprehensive implementation of the IFI may involve a context-dependent trade-off between the resource-intensive but democratically grounded DV approach, and the pragmatically simpler HES approach. While we don't have the space to work out the details here, we can offer a first cut that appears promising. In cases where science has no clear practical implications, it seems appropriate – in terms of engendering trust – to omit the intensive public deliberation and input required by the DV approach in favor of the relative simplicity, transparency and directness of the HES approach. In other words, when there are no clear practical implications of research, the democratically-grounded, action-oriented DV approach isn't especially valuable, whereas the simplicity and transparency of the HES approach is. Concretely: when deciding whether or not to report that a new moon has been discovered orbiting Jupiter, scientists ought to employ high epistemic standards, since such research doesn't have direct or obvious practical consequences for the public.

In cases, though, where there is a near-consensus that less-than-certain information is highly actionable – situations involving potentially lethal toxins, or predicted catastrophic weather events – trading the simplicity of the HES approach for the more complicated but democratically-grounded and action-oriented DV approach is likely the best means of warranting public trust in the outcome, since what the public ultimately cares most about here are the consequences of knowledge – protection from toxins or extreme weather events. Furthermore, in cases like these, some of the more serious objections to the DV approach lose much 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. The context-dependence of the DV approach is therefore less problematic here.

Putting this together, we cautiously suggest that in situations where scientific discoveries have no clear practical significance, or where their practical significance is agreed to be relatively minor, scientists ought to adopt the HES proposal, grounding trust in the simplicity, transparency and uniformity assured by only reporting highly certain claims. In cases, though, where discoveries potentially do have major practical significance *and* there is a democratic consensus that significantly less-than-certain information is practically relevant, scientists ought to work from those lower standards as the DV proposal would recommend, trading uniformity and simplicity for more resource-intensive democratic processes. What remain, then, are what we find to be the hardest cases: cases where there is disagreement about whether science has practical significance, or about whether significantly less-than-certain results are practically relevant. (John's case of insecticides and bee populations may be of this type.) These cases, we think, pose serious problems for both the HES and DV proposals – which, perhaps, reflects the complexity of and tension between the different visions of science motivating them – and may constitute the crux of the disagreement between John and Schroeder.

b. *Possible Objections*

Though we think our proposed hybrid approach is promising as a ground for public trust in science, it of course faces some challenges. We'll briefly comment on two. The first challenge concerns the relationship between our hybrid approach and John's HES approach. Throughout, John argues that it is important that scientists' standards 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 HES approach?

We agree there is a tension here, but perhaps the tension is not as serious as it may appear. We presume that John would at least allow epistemic standards to vary by scientific field. High-energy physics, for example, typically uses a five-sigma standard 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. If, however, John admits at least this much variation into the HES approach, we are optimistic that it will be possible to work out the details of a hybrid approach like ours, so that it can retain the benefits of HES in cases where high standards are employed, while allowing for varying standards in cases of urgent public concern.

A second challenge to our approach may be harder to resolve. Part of the appeal of IFI-based approaches is that they take value decisions 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 justificatory decisions, but introduces the need for a new kind of decision: decisions about the practical significance of scientific research. On our approach, someone must assess the potential practical importance of different research questions, in order to determine which warrant a DV approach and which warrant an HES approach. How should these decisions be made, and by whom? If the answer is that these decisions should be made based on a quick, informal assessment of public sentiment or of general intuitions – e.g. "surely discovering a planet's moon will have little practical significance, at least compared to research on new medical therapies" – then there is a risk that the intuitions, ideas, and values of those with the loudest and most powerful voices will end up having an outsize influence on the course of science, and in particular on the scientific knowledge that the public will have access to. This is a problem since, as many have argued – notably feminist theorists, race theorists, and environmental justice activists – the marginalized and disempowered often have very different views (and some, e.g. standpoint epistemologists, would argue epistemically superior views) about what counts as practically significant (Hartsock 1998; Fadahunsi 2017)**.** If, to avoid this result, decisions about practical significance are instead made through a more careful and systematic survey of public opinion, then the hybrid approach starts to look very much like the DV approach. At the very least, it will inherit some of its most serious problems, such as its resource-intensive nature.

V. CONCLUDING THOUGHTS

The driving questions of this paper are not new ones. Nor are these questions specific to science.[12] 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 taken to be 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 & Conway, 2011), '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 looming "crisis of trust" between scientists and the public (Czerski 2017), it is not entirely clear on what grounds scientists can claim to deserve the public's trust. 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 a long history (Boulicault, unpublished manuscript) and has been discussed and critiqued (Scheman 2011), we don't think it has received the kind

[12] Questions of trust can be posed in any instance of epistemic dependence, i.e. any circumstance in which there is knowledge exchange or dependence 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.

of detailed consideration it deserves, particularly in the inductive risk literature. The IFI has been treated as though it is a fixed, monolithic ideal for science but, 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, but there are surely many other possibilities. 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 of addressing broader questions about the foundations for public trust in science.

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