

## Virtues of Science and Citizenship

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**Abstract:** Who counts as a legitimate epistemic authority? We argue that answering this question for either science or politics involve comparable cognitive demands. We make our case by way of two orthodoxies concerning the practice of science and the relationship between science and policy. A powerful criticism of one of these orthodoxies suggests a way of approaching the other, and that interpretive stance leads to, and ultimately sustains, our thesis. The justification for privileging any deliberative outcome, in science or public life, importantly presumes certain epistemic virtues for both science and citizenship.

We begin with a puzzle: in science as in politics, it is reasonable to hope that more voices, and more meaningful opportunities for dissent, can liberate us from the tyranny of received wisdom and vested interests. Yet more voices – especially acrimonious dissenters – can distract and confuse, sometimes deliberately. Think here of industry-funded research challenging an emerging scientific consensus on the mechanisms and risks of anthropocentric climate change: here it is difficult, even for the relevant scientists, to disentangle reasonable disagreement from mere partisan obfuscation.

In science as in politics, then, we want dissent and disagreement, but we want the right kinds of dissent, and we imagine a clear but often-unarticulated distinction between constructive and unhelpful disagreement. The former we embrace; the latter we suppose is driven by error, dishonesty, or mere partisanship.

Rather than drawing this distinction ourselves, as philosophers and theorists, can we simply trust that experts in their varied fields are best situated to formulate this distinction, as appropriate to their areas of expertise? What counts as reasonable dissent and constructive disagreement will vary by field of concern, and those steeped in those epistemic traditions are best situated to formulate the appropriate standards. Perhaps those elites need our help to live up to the standards they set, but the standards are theirs, not ours.

Still, however reasonable this commitment to epistemic pluralism across the varied sciences, it seems simply to restate the puzzle, and raises another question. How do we foster the right kinds of

dissent, the better forms of disagreement? How do we recognize legitimate epistemic authority for any given elite when they tell us which kinds of dissent are legitimate, which suspect in their field? Mere deference to expertise simply avoids these difficult questions.

The difficulty of these questions, might drive us at least to assert an asymmetry thesis: however difficult it is to specify generic standards of epistemic authority across the sciences (let alone for political disagreement and policy debates), we can at least agree that certain generic features attributed to science -- conceptual rigor and coherence, careful empirical testing using public and repeatable methods, independent verification -- have been extraordinarily successful, however we define success and utility. Armed with that folk pragmatism about scientific method, we should embrace diversity and disagreement in politics and policy, certainly, but then ensure that those disputes, and especially resulting policies, are informed by good science.

What counts as good science? The advocate of this orthodoxy might allow that science itself needs to be more inclusive in certain respects -- no unquestioned deference to scientific elites, no monolithic standard of valid scientific knowledge -- but they will likely appeal to the folk conception just mooted, and reject the equivalence we suggested at the outset, between inclusive aspirations in both science and politics. The two things are very different beasts, and their relationship is clear: science should inform politics, but not the other way around. In science as in politics, we should not blindly defer to authority claims, but the folk conception of scientific method gives us reasonable ground to privilege many scientific knowledge claims as authoritative for political purposes. The reverse is not true.

We challenge this: something like the reverse is indeed plausible, because the appeal of the folk conception of science depends on underlying epistemic properties that are also desirable in political judgement. The kind of reasoning that we admire across the sciences is in fact just the kind of reasoning we ought to admire in politics, and vice versa. Making this (rather sweeping) case requires, however, that we challenge a durable orthodoxy in understanding science: the priority of theory to practice. Happily, the grounds for rejecting that view are legion. Our move is to show that those grounds suggest certain (institutional) virtues common to both scientific practice and democratic citizenship (broadly conceived). Pierre Duhem once called for the cultivation of good judgement in scientific thinking.<sup>1</sup> Our approach suggests that, in science as in citizenship, we ought simply *to*

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<sup>1</sup> “We are thus led to the conclusion so clearly expressed by Claude Bernard: The sound experimental criticism of a hypothesis is subordinated to certain moral conditions; in order to estimate correctly the

*cultivate good judgement*, although with the emphasis on cultivation: ours turns out to be a distinctly institutional view that favours certain epistemic habits, not a restrictive account of personal character.

To see why, let us explain the motivating puzzle more fully, and show how it might inspire the asymmetry thesis.

### *The Promise and Peril of More Voices*

In politics, the powerful regularly trammel the weak, the rich ignore the poor, the advantaged discount luck and exaggerate merit when justifying their privileges, and so on. In the various sciences, exercises of brute power and biased understandings are perhaps less dramatic, but still present: think of prestige effects in grant allocations and publication decisions, and of the kinds of gendered distortions in cell biology exposed so powerfully by Evelyn Fox Keller (e.g. 1995). This goes some (but not all) of the way in explaining why the history of science is a history of sometimes spectacular misunderstandings.

In both science and politics, then, diversity and dissent are vital. But diverse voices can also confuse, and here is where one might insist on an important distinction: in politics, acrimonious dispute is rooted in clashes of values and interests, whereas in science, ideally, we seek the best explanation in light of reliable evidence generated by careful, public, and repeatable interventions in the world. When political acrimony intrudes in science, informing the subjects and directions of research, dissent is suspect because of this clear asymmetry in method and intent: in science we seek to explain, whereas in politics, we strive to win over opposing partisans. At its best, policy-making can overcome these pugilistic motives and orient laws and policies toward the common good, but it cannot do that effectively if the science appealed to originates in - or worse, deploys the strategic and rhetorical methods of - partisan political disputation.

Again, consider the case of industry-inspired, and sometimes industry-funded challenges to anthropocentric global climate change. There are, to be sure, real and constructive debates in climate science over competing modeling approaches, and how best to evaluate various kinds of evidence in assessing the performance of different models. There are certainly important debates as well, over how to weigh risks and respond to uncertainty when considering policy responses (e.g. Broome 20xx). It is, however, often difficult (and not simply for lay readers, but for scientists themselves) to distinguish between good-faith efforts at advancing scientific understanding of anthropogenic climate change, on

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agreement of a physical theory with the facts, *it is not enough to be a good mathematician and skillful experimenter; one must also be an impartial and faithful judge*" (1906, 218, our italics).

the one hand, and sceptical research funded by industry, with the express motivation of discrediting findings and interpretations that threaten their commercial interests.

Given this situation, it would certainly do no harm if scientists, legislators, and citizens more generally were reliably able to chart a course between the dangerous shoals of complacent consensus and partisan distortion. At the very least we ideally want trusted elites – scientists, judges, journalists – to meet this burden, and we hope that citizens might typically do better than chance at identifying the epistemic competence of these elite actors. If the constituent institutions of science and democracy can foster the weaker condition, and at least lean toward the stronger hopes for careful informed judgement, then that would be a good thing. But how to strike that happy mean? how do we sort the (reasonable) wheat from the (partisan) chaffe?

### *The Asymmetry Thesis*

How to structure the institutions of both science and democracy to resist biases, distortions, and exploitation, and to instead favour reasonable scepticism and constructive engagement across differences in backgrounds and beliefs? What counts as reasonable dissent? What counts as provisionally but firmly settled scientific consensus? We might forgive those who throw up their hands here and conclude that these questions point to the limits of inclusion, and the corresponding need to defer to the epistemic authority of expertise and experience: we ought generally, as a default epistemic posture, to privilege scientific knowledge claims in considerations of policy, for the reasons already proffered: science seeks provisional but stable consensus on empirical facts and convincing explanations, and admits of independent public scrutiny. Political debate is rooted in very different motives, and so appeals to different strategic and rhetorical methods. That contrast alone is enough, we might think, to justify at least provisional deference to scientific knowledge claims that are shown to have arisen from the scientific method, properly applied.

On this folk view, scientists – working as researchers, distinct from their status as citizens – inform policy-making by playing a variety of roles in a complex institution. Publicity and replication are the primary mechanisms by which the institution of science generates impartial knowledge via rigorous explanation. The virtues of particular scientists are less important, and indeed some motivation bias on the part of scientists may improve the performance of science as an institution, by encouraging research in unpopular fields and seemingly implausible hypotheses.

Beyond the realm of science, citizens inform policy by way of various mechanisms through which they voice their needs, preferences, and values. They elect representatives, vote in referenda, wave placards, post acrimonious missives to blogs and newspaper comment sections. Scientific knowledge can inform how citizens understand and express their judgements in these and other fora. Science also informs at various stages of policy formulation and implementation—think here of federal agencies and legislative committees.

According to this asymmetry thesis, however, the relationship between science and policy is largely asymmetric: science informs, but is typically not informed by the broader public sphere, or the various agencies and officials of government. As Philip Kitcher puts the point, “scientific inquiry sets the standards for the acceptability of belief” (2008, 11), not the other way around. Science, in short, is – and ought to be – above the fray of political bargaining and moral controversy, and the implied reasons seem to be the ones on offer here: the characteristic motives and methods of science are different from politics.

### *Against Asymmetry*

A range of powerful arguments have suggested that science is not (nor ought to be) insulated from the influence of everyday values. We say “everyday” values to highlight the fact that these theorists are not meaning merely to resist the characterization of science as value-free. Advocates of the stated orthodoxy can easily admit their use of suitably scientific *cum* epistemic values, such as truth, empirical adequacy, predictive power, or simplicity. Those who challenge this orthodoxy counter that science is implicated in more than a narrow range of epistemic values, but also in the same sorts of value disputes that characterize our public lives.<sup>2</sup>

We can locate another strand of this criticism in work by feminist philosophers of science who have called into question the sharp distinction between epistemic and non-epistemic values (e.g., Rooney 1992, Longino 1996). Relatedly, Heather Douglas (2000) has pointed to the crucial role that

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<sup>2</sup> An extreme version of this challenge can be found in the Strong Programme of Bloor, Barnes, *et al.*, which calls attention to the ineliminable influence of sociological factors in the construction of scientific knowledge (including, but not limited to, what gets to *count* as scientific knowledge). This line of thought has, in the hands of Collins and Evans (2002), branched into a more recently influential (and less extreme) discussion—one that bears a family resemblance to our current aims by privileging a conception of the citizen-scientist.

non-epistemic values play when scientists have to make judgments in response to *inductive risk*.<sup>3</sup> An obvious example is judgments about statistical significance related to rare but devastating complications of some medical treatment. Consider too the myriad judgments that must take place in developing and interpreting exceedingly complex models of climate change. Part and parcel of these judgments is taking into account the inductive risk of under- or over-alarming a world whose preparations for the future depend on an established level of tolerance for noisy data, and considerable sensitivity of one's model to alternative specifications and boundary conditions. In such cases, responsible judgement will draw on non-epistemic values—for example, making sure you are not artificially discounting the lives of those who live in coastal regions, islands, and low peninsulas.

### *Another Thesis, Another Orthodoxy*

So we have good reason to think that this approach to the mooted dilemma - the promise yet peril of more voices - has serious problems. We, however, mean to deny a specific part of the asymmetry thesis: that the sciences should inform politics, but not the other way around. So, while we can happily embrace these penetrating criticisms of the thesis, we need to show that, again, the kind of reasoning we ought to admire in politics can indeed inform scientific practice across a broad swathe of the natural and human sciences, and vice versa: that the motives and methods characteristic of good science are appropriate for much of political life as well.

Why might we, as a default presumption, privilege scientific knowledge claims? Again, a powerful and popular folk intuition appeals to the epistemic structure of scientific knowledge production: coherent, internally consistent theories yield a family of interrelated testable conjectures, that are then subject to rigorous attack, both at the conceptual foundations, and their empirical implications. Notice here that what seems to make scientific claims so attractive to so many is not simply their empirical grounding, as in the folk conception as characterized so far, but the fact that empirical regularities are (i) derived from plausible theoretical conceptual structures, and (ii) the empirical tests of the resulting conjectures satisfy conditions of publicity and replicability. Both of these requirements seem to be important: even rigorous public replication of findings will not be considered scientific if it lacks plausible conceptual foundations. After all, the asymmetry thesis derives its intuitive force not simply from the effort to test one's empirical claims against evidence. Much political debate does that, albeit in ways that often-dramatically fail to meet even basic standards of statistical inference. Nor is the force of the folk view rooted in the possibility of falsification by

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<sup>3</sup> Hempel's term for the possibility that one is wrong about one's scientific conclusions (specifically, the acceptance or rejection of a hypothesis).

empirical study: good social-scientific policy evaluation does that, tailoring observational studies and even crafting experimental designs to make plausible causal inferences. Yet in spite of this growing methodological sophistication of the social and behavioural sciences, many of us (experts and lay commentators alike) seem more ready to defer to knowledge claims grounded in physics than in arguments about, say, political culture or rational expectations. Why? Climate scientists and economists often deploy similar computational and statistical techniques to make forecasts of enormously complex systems, and yet it seems safe to say that we trust the climate science more readily than the computable equilibrium models of national employment figures or growth rates. Again, why? Perhaps because we think the physical models are rooted in more robust conceptual foundations?

This may explain why, for so long, philosophers of science focused on high theory, almost always with Physics as the exemplar; the particulars of testing were something of an afterthought. But that has changed. Ian Hacking and Evelyn Keller, among many others, have shown us that the distinction between conceptual theorizing and experimental practice is often messy and reciprocal. Understanding the complex interplay between these activities gives us a richer understanding of how science – and scientists – actually work. Outside of some rarified domains of high theory in physics, scientists do not simply derive increasingly complex (and hopefully testable) implications from self-evident axioms (that sometimes just happen to be plausible descriptions of, say, some fundamental element or process in the natural world).

Theory-construction is, to be sure, often formal and deductive; but often our intuitions are formed after repeated observations of some phenomena, or the results of a series of experiments, perhaps designed to test some other conjecture entirely. It is now almost a truism of philosophy of science to assert that ‘facts’ (and ‘observation reports’ about them) are ‘theory laden’: they never simply speak for themselves. But it is also the case that theories are often ‘fact laden’, insofar as they are deeply implicated in our varied encounters with the world. Theories rarely ‘cohere’ or ‘inform’ without a dense background of reasonably settled facts.

Against that background, we work up our formal descriptions into a coherent model of some mechanism or cluster of interrelated mechanisms, and then we return to the world, making interventions and manipulations to test our model. To anticipate our argument: the second orthodoxy, when buttressed by the folk conception of science, needs to account for the intimate reciprocity between theory, empirics, and practice in so much of science. Much of science is not a process of axiomatic reasoning to generate testable hypotheses; it is, instead, a complex process of reflective equilibrium-seeking.

### *Wide Reflective Equilibrium for Science and Citizenship*

So, we want to embrace a plausible rationale for part of the folk conception of science: the widespread intuition that any claim to privilege scientific knowledge depends on not just empirical confirmation that satisfies publicity and replication; it must also have plausible conceptual grounding. Again, that seems to be a plausible account of the justification implicit in the first orthodoxy: science satisfies the dual constraints of coherent conceptual foundations tested by public and replicable interventions in the world. Moral, religious, and ideological debates characteristic of politics simply do not admit the same conceptual and evidentiary grounds.

If, however, *that* is the reason one offers for privileging scientific knowledge claims - that they are valid insofar as they satisfy these two constraints, conceptual and empirical - then we ought to be clear on how that validity in fact emerges, not from the priority of High Theory, but from something more complex and coherentist in justificatory structure.

Interestingly, lurking in an influential treatise of political philosophy, there is a model – constructivist in aims and coherentist in justification – that well-captures how science often proceeds, and also how ethical reasoning might profitably proceed when we ask how best to evaluate the claims we make on others, and upon shared resources. The treatise in question is *A Theory of Justice* by John Rawls (1999). The constructivist and coherentist approach central to that work seems, in our judgement, to demand a certain orientation towards knowledge-related and value-related claims. To see this, we will elaborate the Rawlsian variant of Nelson Goodman’s account of coherentist justification.

Rawls in fact says very little explicitly about reflective equilibrium and coherentist justification. He describes the former as a process of moving from considered moral judgements made under favourable conditions, to a theory which interprets and orders those judgements. When the veil of ignorance is introduced it becomes clear that this equilibrium-seeking is *wide*, insofar as it proceeds against a background of settled “general” facts about human societies and social theory. Following Norman Daniels, we can take the rawlsian standard to involve some “set of considered moral judgements,” a corresponding “set of moral principles” that interpret and organize those judgements, and “a set of relevant background theories” which inform, *but do not determine*, the interpretation and ordering of our moral judgements.

On the latter, we can glean some sense from Rawls that reflective equilibrium in some sense justifies our principles and their ordering, by virtue of how they hang together in a stable, mutually

supporting way. Late in *Theory*, Rawls explains that

“Philosophers commonly try to justify ethical theories in one of two ways. Sometimes they attempt to find self-evident principles from which a sufficient body of standards and precepts can be derived to account for our considered judgements. A justification of this kind we may think of as Cartesian. It presumes that first principles can be seen to be true, even necessarily so; deductive reasoning then transfers this conviction from premises to conclusion. A second approach (called naturalism by an abuse of language) is to introduce definitions of moral concepts in terms of presumptively non-moral ones, and then to show by accepted procedures of common sense and the sciences that the statements thus paired with the asserted moral judgements are true. ... I have not adopted either of these conceptions of justification” (Rawls, 1999, p. 506).

For Rawls, theorizing about justice involves constructing a favoured *conception* of justice, which is an interpretation of the *concept* of justice. “A conception of justice,” Rawls tells us, “cannot be deduced from self-evident premises or conditions on principles; instead its justification is a matter of the mutual support of many considerations, of everything fitting together into one coherent view” (507).

To be sure, critics have vigorously attacked this coherentist model of moral justification. David Haslett (1987) in particular charges Rawls with circularity: either our principles need some foundation, or our considered judgements must be grounded in some way; these things cannot justify one another. If we justify our principles in terms of their success in clarifying and organizing our judgements, then we must provide some account of why those judgements are morally privileged.

Haslett preempts a possible rejoinder: considered judgements are not obviously like observation reports in science, which at least purport to be about an independently real external world. In the realm of preferences and values, we surely cannot make the same claim about considered moral judgements, at least not without embracing either moral intuitionism or realism. A coherence model of scientific justification involves testing theories in part by their strength in predicting subsequent observation reports, but there is no uncontroversial analogous relationship between our moral principles and considered moral judgements.

I think the answer to this objection is simply to accept that some considered judgements *are* morally privileged. This gambit is consistent with the emphasis Rawls places on favourable conditions for sincere and informed judgement, and the elaboration of those considered judgements in light of our distinctive moral powers: a sense of justice and a conception of the good.

This move grounds reflective equilibrium in some account of privileged inputs: the right sort of considered judgements, arrived at in the correct way. We still judge our final theory in terms of coherence between principles, judgements, facts, and reasonably uncontentious explanatory theories, but the circularity objection loses much of its force, because considered moral judgements on this approach do now have a privileged status, roughly analogous to the initial credibility granted to (properly generated) observation reports in science.

Taking this position does, however, seem to conflict with Rawls's own apparent concession to the folk conception of science and the asymmetry thesis: for Rawls, science was not a part of public reason, but a source of some of the reliable 'general facts' that inform wide reflective equilibrium ...

### *Concluding Thoughts*

Here, then, is the crux of our argument: the same personal orientation toward evidence and argument gives us an account of privileged inputs in moral justification for political questions, as for properly generated claims of fact in science.

In an age of vast information flows of wildly varying quality, we want people – as scientists and citizens – to recognize trustworthy evidence and sound arguments; but we also want them to be simultaneously sceptical, while also taking seriously a range of views that pass thresholds of reliability and soundness. Perhaps most vitally, we want them to apply this critical attitude to the very standards of trustworthiness and soundness they adopt in their deliberations and judgements.

In this way, the justification for privileging any deliberative outcome, in science as surely as in public life, importantly presumes these epistemic virtues, for both science and citizenship. Furthermore, these are the very virtues implicated in sincere and careful reflection and introspection, under favourable epistemic conditions, used to arrive at considered judgements about fact and value, which are in turn well-modeled as reasonably stable "provisional fixed points" generated through a process of wide reflective equilibrium-seeking.

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Origins of differences in climate sensitivity, forcing and feedback in climate models

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