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Editor

# Virtue Epistemology Naturalized

Bridges Between Virtue Epistemology  
and Philosophy of Science

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## Bridging a Fault Line: On Underdetermination and the *Ampliative* *Adequacy* of Competing Theories

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*Discussion of theory virtues exposes a fault-line in philosophy of science that [separates] very different visions of what the natural sciences are all about.*

– Ernan McMullin<sup>1</sup>

### 1 Introduction

Logical empiricists posited a singular logic of inquiry across all sciences, and pied-pipered social scientists to follow them by emulating the methods of the “hard” sciences. Post-positivist thinkers denounced the pied-piper, but themselves often imposed an equally rigid egalitarianism among academic fields and cognitive styles as sources of knowledge.<sup>2</sup> More careful attention to problems related to underdetermination, however, indicates a relationship between the sciences at once both less neat but far more intriguing than either of the foregoing views: A more dappled relationship where some bits of history or sociology may be more reliable than some bits of physics, and where worries about underdetermination are felt as regularly in such areas of natural science as contemporary theoretical physics and cosmology, as in the social sciences like economics where we usually locate them.

<sup>1</sup>“The Virtues of a Good Theory” (2009), 506.

<sup>2</sup>Connected with this is Sankey’s point that “while empiricists explain consensus but have a hard time with disagreement, post-empiricists emphasize dissensus at the cost of being unable to explain how agreement is arrived at. But [any] adequate philosophical model of scientific rationality must explain both consensus-formation and the existence of widespread disagreement” (1996, 1).

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Attention to the underdetermination problem in the sciences is also one among a number of bridges between philosophies of science and virtue epistemologies (hereafter VE), or so I will argue. As Ernan McMullin writes in “The Virtues of a Good Theory” (2009), “The assessment of theory is a form of inference quite different from induction over a set of observation reports resulting in a law-like generalization” (501). The verificationist conception of objectivity he points out moved under criticism (around mid-century) to a fall-back position closer to the hypothetico-deductive account; yet the assessment of theory as the critics of positivism pointed out is more often a comparison of extant rivals, and less often a sheer encounter between a stand-alone theory and an experimental test, as in the Popperian notions of a “crucial experiment” implying a quick kill of a theory that faces recalcitrant evidence as a result of disappointing test data.

The deeper reasons for these criticisms of the received view of theory choice involve a long discussion of methodological holism in response to recognition of certain kinds of worries about the underdetermination of theory by data, and again about the underdetermination of theory choice by methodological rules and standards. The former kind, sometimes called *logical underdetermination* or *Humean underdetermination*, I think of as a ‘global’ but at the same time a rather weak thesis that no theory is strictly-speaking proven or entailed by its confirming instances/predictions. The latter is a much stronger thesis, but I will argue as the same time a *localized* concern or problem rather than a global one. This is how Larry Laudan formalizes the first (“Humean”) kind in “Underdetermination Demystified” (1990, 323):

(HUD) For any finite body of evidence, there are indefinitely many mutually contrary theories, each of which logically entails that evidence.

Famously, “Quinean holism” as presented by the early Quine was tied into his support of much stronger claims than (HUD) about the underdetermination of theory by data; but he later repudiated his early views, saying that his statements about holism were both stronger than was needed to challenge the dogmas of empiricism, and stronger than he wished he would have made.<sup>3</sup>

The so-called Duhem-Quine Thesis is now widely regarded as a mistaken designation, since Duhem’s views were considerably milder than Quine’s; but for present purposes we needn’t go into these matters.<sup>4</sup> (HUD) already shows us a quite substantial sense in which theory-choice turns upon values at work in science, which is to say, upon non-deductive or ampliative desiderata, which primarily include the virtues of a good theories (theory virtues). Sometimes in

<sup>3</sup>As Laudan points out, in “Two Dogmas of Empiricism” Quine “propounded [but did not give any good reasons for believing] a thesis of normative, ampliative, egalitarian underdetermination” (334). Laudan defines and then argues against Quinean underdetermination:

(QUD) Any theory can be reconciled with any recalcitrant evidence by making suitable adjustments in our other assumptions about nature.

<sup>4</sup>For an admirably clear explanation of the differences between Duhem and Quine, and the Quinean history of retracting (QUD), see Massey (2011).

the literature these are called cognitive values. McMullin writes that “Calling them ‘virtues’ rather than ‘values’ draws attention to their status as attributes at once objective and desirable”.<sup>5</sup> As an aside to be developed later, virtue theorists of all kinds present a Janus-faced (or compatibilist) conception of the relationship between naturalism and normativity. If reasoning about the *ampliative adequacy* of theories works through thick concepts, as I think it plainly does, this arguably strongly supports such a compatibilist view, whether one prefers to characterize these concepts as cognitive values or theory virtues.<sup>6</sup>

The second kind of underdetermination problem that should especially concern us is what we’ll call *ampliative underdetermination*, or the underdetermination of theory choice by methodological standards. It is a worry that ampliative desiderata in the form of theory virtues and the good sense of the researchers themselves fail in providing a unique preference weighting in a particular situation. Alex Rosenberg (2012) argues, and I think rightly, that “The problem of empirically equivalent but logically incompatible theories becomes especially serious as science becomes more theoretical.”<sup>7</sup> But some thinkers do not want to treat this as the “local” issue I have presented it as being, and that Rosenberg’s point would seem to support. They rather want to claim that *ampliative underdetermination* is global, and associate it with a thesis of “non-uniqueness” that is supposed to impugn the legitimate functions of the theory virtues in theory-choice. I think these claims are over-wrought as much discussion of underdetermination and holism is over-wrought, but these issues will have to concern us more directly later.

It may well be that the kind of intractable, irresolvable theoretical disputes that underdetermination seems to make possible are almost never actual. Still, the logicist conception of objectivity associated with logical empiricism is already shattered by recognition that “[B]esides the test of observation, theories are also judged on other criteria: simplicity, economy, explanatory unification, precision in prediction...consistency with other already adopted theories...amount of allowable experimental error, etc.,” and that while there are disagreements and sometimes very great disagreements among theorists, “yet over time these disagreements are settled, to almost universal satisfaction,” by reasoning about *ampliative adequacy* (Rosenberg, 212; 214). For those who acknowledge the genuineness of the problem

<sup>5</sup>E. McMullin, 501.

<sup>6</sup>On virtue epistemology’s role in respect to recent calls for the “thickening” of epistemology, see the papers in the 2008 *Philosophical Papers* edition, *Epistemology Through Thick and Thin*, 37(3). These include Guy Axtell and Adam Carter (2008), “Just the Right Thickness,” which identifies and challenges an epistemological analogue of the (ethical) “centralist” thesis (of the primacy of thin concepts over thick) that Bernard Williams criticized.

<sup>7</sup>Rosenberg, 212. I lean on Rosenberg especially here because he seems to acknowledge that even strong empiricists see a substantial role for theory virtues and for ampliative reasoning more generally in theory choice. Any attempt at a more directly empiricist justification for the methodological rules we employ in theory choice, he concedes, “is circular as an argument against the threat of underdetermination,” (214) and appeals to them as *a priori* are unavailable to empiricists. Thus neither the rationalist nor the empiricist account of an algorithm of theory choice is at all satisfactory (214).

of underdetermination for the sciences—without going so far as to draw relativist implications from it—theory-confirmation and disconfirmation needs to be conceived of as utilizing a “toolbox” of theoretical virtues: “Theory choice is a continual process of iterative applications of this same toolbox of considerations in order to assess the implications of empirical observation in making theory choices” (214).

The connections we are beginning to see here between theory virtues and virtue epistemology are not merely ornamental. Both directly concern ampliative (non-deductive) reasoning in the sciences. Moreover, application of theory virtues to choice among competitors involves weighing these theory virtues against one another, and thereby calls upon what Pierre Duhem would call the *bon sens* of the scientist. Hence a condition of character is implied even where the explicit appeal is only to impersonally-framed theory virtues. Indeed both the appeal to theory virtues and the appeal to *bon sens* remind us that the scientist qua scientist makes value judgments. This is all the more so when theory assessment is a comparative choice among extant rivals. So what we will here term the *ampliative adequacy* of a theory is conceived largely as a matter of comparison of extant rival theories, and not as a question only of a single theory and its relationship to observational data.

Moreover, virtue epistemology as will be argued further is what helps us to find continuity between contexts of scientific assessment that are not beset by localized underdetermination worries, and those that are. This continuity and the attendant sense of how theory virtues and the intellectual virtues of researchers themselves aid objectivity in science, shows clearly how tying virtue epistemology into our philosophy of science allows us to accommodate ‘the turn to practice,’ while also avoiding relativism. Indeed the present view like McMullin’s directly responds to the shared assumption of logicists and their radical historicist critics, that *if* theory choice isn’t fixed by observations or some kind of observation-linked algorithm, then it is still fixed, but by non-epistemic factors, like personal bias, desire for authority, fame and fortune, etc. Virtue theory shows us how to say ‘neither, nor’ to that bogus kind of ‘either, or.’<sup>8</sup>

In this paper I urge a virtue epistemology extricated from any over-strong interpretation of holism or of underdetermination, and one that isn’t strongly committed to a particular position on the realism/anti-realism debate. We will address different concerns about underdetermination as well as different versions of virtue epistemology. I will have one (relatively independent) thesis for each of the four sections. In Sect. 2 we examine the relationship between theory virtues and personal

<sup>8</sup>Like Daston and Galison in their book *Objectivity* (2007), I would argue that the concept of scientific objectivity has a history, but that the epistemic norms that have informed scientific practice can be historicized without leading to relativism. I have elsewhere argued that considerations stemming from underdetermination problems motivate the claim that historicism *requires* agent-focused rather than merely belief-focused epistemology, and that this is partly what makes it possible to distinguish weak or moderate historicism from radical historicism about the epistemic values recognized in science. See Axtell, “The Dialectics of Objectivity,” (2012) in a special topical issue of *Journal of the Philosophy of History*, on intersections of historicism, naturalism, and virtue epistemology.

intellectual character traits and introduce a taxonomy of theory virtues that addresses both prescriptive guidance and normative assessment. Section 3 discusses a thesis that Richard Dawid argues for, which describes a “substantial shift” he sees occurring in contemporary fundamental physics: “the increasing importance of assessments of scientific underdetermination” (2011, 2).

Dawid’s thesis is a reasonable one, nicely descriptive of problems of theory choice in high energy physics and the localized reliance there on standards of *ampliative adequacy*. I argue that it thereby also indicates a need in philosophy of science to utilize a kind of virtue epistemology. But what kind of virtue epistemology, specifically? One that essentially stops with theory virtues shared by a community of inquiry, or one that appeals as well to the virtues, or *bon sens*, of good researchers themselves? That raises the issues of what we’ll call *ampliative underdetermination*. Section 4 develops these further connections between ampliative reasoning and theory choice by joining a recent debate among philosophers of science over Pierre Duhem’s account of the *bon sens* or good sense of scientific practitioners. I work out my differences from Abrol Fairweather, David Stump, and Milena Ivanova in their respective interpretations of Duhem. Section 5 develops what we termed a Janus-faced conception of the descriptive and normative aspects of theory virtues, and argues that inquiry-focused virtue epistemology coheres with and adds substantial support to meta-scientific pluralism, and to normative naturalism.

## 2 The Virtues of Empirical and Ampliative Adequacy

McMullin provides a useful taxonomy of theory virtues, a taxonomy that as one commenter puts it, “preserves the epistemic character of scientific theory without confining the epistemic values merely to first-order ‘empirical adequacy’ as van Fraassen understands it.”<sup>9</sup> Is empirical adequacy always the ‘thin’ notion associated with a test, or does it in function in scientific debates function more like set of virtue concepts? McMullin explains that the association of theory confirmation with deductive implications of observations and tests should be restricted to the synchronic and retrospective virtue of *empirical fit*. *Empirical Adequacy* actually refers to a more over-arching class of cognitive virtues than does *empirical fit*, and on close inspection contains some forward-looking sub-virtues. “Empirical fit should be distinguished from empirical adequacy, as this is defined in van Fraassen’s constructive empiricism. Empirical adequacy refers to all of the consequences of the theory, regardless of whether they have ever actually been drawn or checked against observation” (502).

McMullin sub-divides what I call the theoretical virtues of *Ampliative Adequacy* into internal, contextual and diachronic virtues. These virtues he presents as *complementary* to the central theoretical virtue of epistemic fit. Resolving a localized

<sup>9</sup>Allan (2006), 81.

situation of underdetermination through a new test providing decisive empirical advantage of one theory is the best outcome.<sup>10</sup> But where it is not to be had, then with McMullin we must “argue for the relevance of a whole series of confirmatory virtues that complement the central virtue of epistemic fit, transforming natural science from a mere saving of the phenomena to a genuinely explanatory and ontologically expansive enterprise” (2009, 502).

“Internal” virtues like *internal consistency* are basic requirements while others limit the degree of allowable ad hocness. “Contextual” virtues include *external consistency* and *consonance*, which address consistency with background knowledge, and *optimality*, which involves not only retrodiction but also comparative merit of a theory. But the most unusual and useful feature of McMullin’s taxonomy is his close attention to “Diachronic” theory virtues, including especially *fertility*, along with *consilience* and *durability*. *Fertility*, almost an executive virtue for McMullin, is Janus-faced, looking backwards to novel facts predicted and confirmed, as well as forwards to potential for the hypothesis to issue new, bold predictions. Unlike logicist metascience, defenders of the relevance of diachronic virtues like *fertility*, *consilience*, and *durability* to theory confirmation can easily maintain a lively understanding of the importance of history of science to philosophy of science. McMullin’s emphasis on fertility as epistemic desiderata opens up the *diachronic* aspects of theory assessment lost with a logicist account of scientific objectivity. But we earlier claimed that the more serious underdetermination problems are localized ones, and that these do not track the conventional distinction between soft and hard sciences. Problems of local underdetermination arise everywhere that fields of study become more theoretical and less directly experimental. In the next section we consider an example of this from contemporary theoretical physics and cosmology, and at what implications situations of local underdetermination may have for the centrality of ampliative reasoning in our conception of scientific objectivity.<sup>11</sup>

### 3 Underdetermination and Theory Choice: The Case of String Theory

The logicist’s notion that theory confirmation should be strictly rule-governed, and that accordance with this logic constitutes the objectivity of science or the rationality of particular scientists, is challenged by underdetermination problems. Scientific language and a practice based on the use of instruments makes such problems unavoidable. In actual practice, underdetermination problems are not ‘solved,’ but

<sup>10</sup>“In scientific research one always hopes for determination: that the world should *determine* the observations we make of it; that evidence should *determine* the theories we adopt; that the practice of science should *determine* results independent of the sort of society in which that practice takes place” (McMullin 1995, 233).

<sup>11</sup>Rosenberg 2012, 212.

they typically are *resolved* after a period of time. They often need to be if researchers are to continue to identify and pursue successful research strategies. It is left to philosophers to sort out the epistemic status of the chosen theory, and to debate what philosophers’ distinctions should be drawn, for instance, between theory ‘pursuit’ and theory ‘acceptance,’ and between narrowly epistemic more broadly cognitive values, etc. With these self-appointed tasks philosophers have not always done very well. As a case study of this, let us look at contemporary String Theory, and the quandary in which standard accounts of theory confirmation leave it.

Richard Dawid writes that,

The canonical understanding of scientific progress...strictly distinguishes assessments of scientific underdetermination from the core elements of scientific progress, which are (1) the development of a scientific hypothesis and (2) the empirical testing of that hypothesis... Assessments of scientific underdetermination, to the contrary, are taken to constitute mere instances of auxiliary reasoning that may be of some relevance by channeling scientific activity towards more promising investigations but do not directly contribute to the generation of scientific knowledge. Put in terms of the old conceptual dichotomy between context of discovery and context of justification, one may say that assessments of scientific underdetermination were always acknowledged as playing some role in the context of discovery but were denied any role in the context of justification.<sup>12</sup>

While philosophers and non-practioners tend to accept the canonical view, among high energy physicists themselves Dawid claims there is a substantial “shift” taking place as they increasingly question whether the canonical understanding of theory assessment is adequate for grasping String Theory’s merits. He argues that theoretical virtues must supersede strict dependence on empirical fit, and that appeals to desiderata of ampliative adequacy “amount to assertions of limitations to scientific underdetermination.” This means that dependence on ampliative desiderata should moderate the demand that a scientist be agnostic about the parts of her theory not in practice open to direct testing. Dawid of course is not suggesting that assessments of scientific underdetermination can ever replace empirical confirmation, but rather that we need an epistemology for the sciences that “can establish an intermediate epistemic status for theories that lie between ‘empirically confirmed’ and ‘pure hypothesis.’”<sup>13</sup> Dawid more constructively sees the old dichotomy between empirical confirmation and mere speculation replaced “by a continuum of degrees of credibility, where the available elements of empirical corroboration and non-empirical

<sup>12</sup>Dawid 2011, 4. “Assessments as to how likely it is that no or few alternative theories can be fit to the available data thus lie at the root of all considerations regarding the prospective viability of a so far empirically unconfirmed or insufficiently confirmed theory. We want to call such assessments ‘assessments of scientific underdetermination’ (2011, 3).

<sup>13</sup>“The emerging new paradigm moves away from an understanding...that attributes the status of mere hypotheses to scientific theories which have found no empirical confirmation. But Dawid also qualifies his claim in certain ways: “Non-empirical theory assessment thus crucially relies on empirical testing and can never fully replace it. Nor does non-empirical theory assessment award the same status to a theory as strong empirical confirmation. It is vaguer and less conclusive than the testing of theories by empirical data. Its vagueness induces the risk that its deployment might be overstretched...” (2011, 18–19).

theory assessment jointly contribute to an overall evaluation of theory's chances of being viable" (2011, 19).

Rejecting the canonical view associated with empiricism and the hypothetico-deductive model means turning in certain fields of research from objectivity through direct testing to objectivity through ampliative reasoning. Dawid's proposal helps make sense out of problems of theory choice in contemporary theoretical physics, and I hold that this shift is one that virtue epistemologies help us to articulate and implement. If we do need an altered conception of theory assessment in high energy physics and scientific cosmology, it is a conception in which reasoning through theoretical virtues plays a more central role in theory assessment.

#### 4 Duhem and the Role of *Bon Sens* in Scientific Practice

Thus far we have associated a virtue epistemology for the philosophy of science with the study of ampliative reasoning utilizing *impersonal* theory virtues. But to what extent will a virtue epistemology draw us also into study of the *personal* intellectual virtues of scientists themselves—the good sense or *bon sens* of the inquirer? I want to argue that there will always be an interesting research program with respect to the personal virtues of scientific practitioners for the reason that personal habits are always active in inquiry. Study of the personal traits and the scientists' 'doings' are always relevant when we take a practice-focused approach to scientific reasoning.

But one might push the question: Could the personal *bon sens* of the scientist ever directly contribute to the epistemic status of the theory which that scientist chooses? By-and-large the role of what Abrol Fairweather terms the "methodological cognitive character" of the scientist—"the set of abilities, skills and dispositions a scientist acquires and expresses through the structured forms of inquiry involved in applying scientific methods" (141)—simply plays a role supportive of their prowess or reliability in deductive and ampliative reasoning. To this extent I would think their study might be of interest more in a sociology than in an epistemology of the sciences. But there may be exception cases, and if there are then these cases can also be delineated by the types of underdetermination worries that dog inquiry. When localized problems become severe about what cognitive values to accept, or how to weigh them against one another (see Kuhn 1977) then inquiry is taking place under another level or type of underdetermination problem. Our primary focus becomes underdetermination of theory *choice* by methodological standards, including the theory virtues. Let us call this type *ampliative underdetermination*, and consider now the philosophical concerns it raises and the resources that virtue epistemologies have for responding to them.

Paralleling our treatment of *logical underdetermination*, I want to say with respect to *ampliative underdetermination* that the sheer possibility that ampliative criteria will not result in a "unique" choice from one scientist to the next is a global, but also only a weak claim. It is not much of a worry since it really only

restates how we got to this point: ampliative reasoning by definition does not meet deductive standards of entailment; if we cannot read theories off of their empirical consequences, the notion of an algorithm for theory choice is off the table and so to re-impose a logicist conception of rationality or objectivity is simply inappropriate for beings such as we are. *Logical underdetermination* forcefully shows us both that "falsifications do not undermine one particular statement and [that] confirmations do not uniquely support one particular set of statements" (Rosenberg, 287). Situations of local underdetermination, where multiple theories compete in some area of science, heighten the need for more holistic evaluation of evidence and of the scientific merits of the competing theories. Further, just as it is always desirable but not always possible that theory choice be based on experimental findings that confer empirical adequacy uniquely upon one theory, so I would hold that it is always desirable but perhaps not always possible to distinguish sharply between the desiderata of *impersonally-framed* 'theory virtues' and *personal* intellectual traits of good researchers themselves. This is what we can call a 'tiered' account of the epistemic relevance of *impersonal* standards and *personal* expertise or judgment.

With McMullan and against van Fraassen (1980) we have held that ampliative reasoning clearly contributes to epistemic status. And we have held that personal virtues and vices (probably both intellectual and ethical) are active and implicated in ampliative reasoning—most obviously in the kind of weighing that inference to the best explanation demands. But also on the present view, we must remain wary of "collapsing" the theory virtues into a set of personal virtues of scientists themselves. My view isn't shared by all self-described virtue epistemologist, however, and this is why I bring it up. Since there are a number of different extant versions of virtue epistemology it is unsurprising to find them running the full gamut of views in relationship to the underdetermination problem. Some authors neglect the theory virtue/personal virtue distinction by not recognizing the importance of the researcher's character and "doings," while others collapse the theory virtues into personal virtues, attaching no importance to impersonally-framed theory virtues.

Some accounts, though not present one, lean upon a strong interpretation of underdetermination and/or holism, and this is one primary way to collapse the distinction. Perhaps the clearest example of this is Lynn Holt's book, *Apprehension: Reason in the Absence of Rules* (2002). Holt develops an "Apprehensionist" virtue epistemology, and one that takes "Methodism" as its opposite and as its stalking horse. He contrasts the apprehensive virtues of understanding (*nous*) and of practical wisdom (*phronesis*) with "the non-apprehensive elements of expertise—calculative reasoning, technical skills" (44). But theory virtues don't fit well with this dichotomy and seem almost entirely left out of his account of theory choice. Holt's view of theory assessment and the epistemic status of theories is basically that it is whatever reflects the judgments of the experts, those who possess the *phronesis* relevant to their field.<sup>14</sup> This might then be the form of virtue epistemology most appealing if

<sup>14</sup>One of the most common objections is that it is circular; another is that it is simply intuitionism in new garb—apprehension or *bon sens* as 'the Emperor's new intuitions.' Holt acknowledges keen

one took a strong stance on holism or on *ampliative underdetermination* and saw it as motivating an either-or choice between Apprehensionism and Methodism. It results in the very strong claims that “the way to adjudicate between rival traditions is to ask the wise” (72) and that “a genuine epistemology ought properly to be regarded as *virtuoso* epistemology: an account of who is best able to judge truth from falsity in virtue of his or her possession of wisdom” (73).

While he neglects to examine Duhem’s account of theory choice, I take it that Holt’s central distinction between apprehensive and non-apprehensive expertise and the personal traits that constitute the former strongly overlaps with Duhem’s distinction between “intuitive” and “mathematical” reasoning. Not surprisingly, some neo-Aristotelian virtue epistemologists find substantial interest in Duhem’s account of *bon sens*. David Stump’s paper “Pierre Duhem’s Virtue Epistemology” offers a virtue-theoretic account of the role of *bon sens* in Duhem’s philosophy of science. Stump argues that despite the fact that Duhem is sometimes read as a conventionalist arguing that there is simply no cognitive way to decide between empirically equivalent theories, closer examination reveals that through good sense of practitioners consensus typically does emerge, and not for purely conventional or epistemically irrelevant reasons.<sup>15</sup>

Whether rightly or not, Milena Ivanova associates Stump’s reading of Duhem with the apprehensionist variety of virtue epistemology. She associates it with a very strong “change in the direction of analysis” thesis she takes all form of virtue epistemology to be committed to, where the merits of the agent’s character are determinate of the epistemic standing of particular beliefs.<sup>16</sup> Although I don’t think that thesis is held by reliabilist or ‘mixed’ forms of virtue epistemology, and don’t see Stump himself as making all the strong claims she seeks to refute, given what we have said above I do agree with much of her criticisms of Apprehensionist virtue epistemologies. She objects that it seems to negate the need for scientists to look for future evidence to evidentially distinguish the theory chosen by good sense. If this were correct it is easy to see why calling Duhemian good sense a virtue theoretic solution to underdetermination will arouse suspicions. As Abrol Fairweather puts it, “it will be controversial to locate some share of the epistemic value of our currently accepted scientific theories in properties of the scientist, rather in properties of the

critics of his view, including Hintikka (2002), who argues that intuitionism is a failed view in the philosophy of science, and “apprehension” is nothing but a re-working of intuitionism. Holt’s Chapter 3, “Apprehension and the Apprehensive Virtues”, offers a direct reply.

<sup>15</sup> Stump (2007), 149–159, 149–150. The personal habits that comprise the scientist’s methodological cognitive character describe real or ideal excellences of inquirers, not of theories or hypotheses *per se*. Perhaps for this very reason, they are less purely intellectual, and indeed those who emphasize their role in inquiry, from Pierre Duhem to Daston and Galison, often want to *insist* that they are or include character traits in the full Aristotelian sense, engaging motivations and sometimes crossing boundaries between the epistemic and the ethical. See Stump (2011) for a further development of this view.

<sup>16</sup> In fact it represents only one strong form of virtue epistemology that would define justified true belief in terms of what an intellectually virtuous person would believe. Stump (2011) does come close to endorsing such a view.

science itself” (2012, 140). Even independently of whether Stump is right to see Duhem’s account as proto-virtue epistemological, Ivanova finds Stump’s own views about the epistemic value of good sense unsatisfactory. She also argues for a different reading of Duhem’s account of good sense. I will not have space to go very far into Ivanova’s or Fairweather’s interesting responses to Stump, but would like to draw out a few general points and to try to straighten out what I see as misconceptions that hinder progress in this debate over the role of impersonal and personal virtues in scientific reasoning.

Ivanova writes that “[Duhemian] good sense does not determine the construction of a theory and it is not what justifies a belief in the truth of a theory. It determines the scientist’s choice, but not uniquely. It restricts the scientist’s choice by excluding some of the possibilities with which he is faced in theory choice. It does not lead to justified true belief, but simply to a temporary acceptance of a theory” (2010, 62). While I can largely agree, I have certain quibbles. Developing a distinction Fairweather suggests, I would say that a virtue epistemology helps us recognize the contribution to epistemic *value* of the scientist’s methodological cognitive character, but that it leaves open a range of views about the epistemic *status* of the theory that good sense selects. But there is also something important in Fairweather’s point that “Method and evidence reign when they can, but epistemic normativity becomes aretaic in UD inquiry with the express purpose of resolving underdetermination” (141). In my own terms, the question of how to parse the differences between empirical testing, theory virtues, and the personal *bon sens* of researchers themselves admits of no *general* answer, but depends crucially upon *local* issues about the relative normality of inquiry being pursued under conditions of underdetermination.

The success condition of Duhemian good sense seems merely to be its breaking of the empirical stalemate in an appropriate way, not in a way that confers uniqueness (across competent scientists or the epistemic community as a whole) on the choice made. But while respecting the distinction between theory pursuit and acceptance or belief, I also think that distinction should not be made too rigid, like the psychology/logic or discovery/justification dichotomies on which logicist metascience depended. What Fairweather argues is missed when such dichotomies are assumed, is the “continuity” a virtue theoretic account provides to and from the movements between contexts of UD (underdetermination) and non-UD inquiry: “In *UD inquiry* we are trying to resolve the problem of theory choice, whereas in *non-UD inquiry* we either have not yet faced the problem, or have resolved it for the time being. The virtues of good sense do not have a constitutive role in generating the epistemic standing of theories in non-UD inquiry... The virtue theoretic reading exhibits axiological continuity between the two contexts of inquiry and thus provides a constraint on admissible resolutions to underdetermination by precluding the introduction of radically new epistemic values” (141).

Another problem I find with Ivanova’s account is with her own claim about non-uniqueness. In her discussions of theory virtues, she jumps too quickly from the *possibility* of ampliative underdetermination or non-uniqueness to the generalization that a unique choice is *never* indicted by the criteria of ampliative adequacy.

“Even though criteria to describe theory choice can be found, they cannot determine the choice uniquely” (60); “These criteria can help us to describe, explain and justify the scientist’s decision, but they do not do so uniquely” (63). Cannot? Do not? This seems to me too great a generalization, which our own account of the *local* nature of ampliative underdetermination worries should serve to undo. The *possibility* of differential weighting applied to the theory virtues, etc., does not mean they *must* differ so significantly; it would be wrong to presuppose that every time two theories are empirically equivalent (i.e., logical underdetermination prevails) there must also be underdetermination of theory *choice* by methodological standards (i.e., ampliative underdetermination) (Laudan 1990). If we are not implicitly identifying a “unique” choice with a logically or evidentially *forced* choice, the claims Ivanova repeatedly makes that virtue epistemological treatments must be “unsatisfactory” because they fail of “solve” the underdetermination problem but only move it to a new level, are simply un-motivated. “Solving” underdetermination problems was never in the cards, and one who bases satisfactoriness of *resolutions* on that measure will always be disappointed. The sense of uniqueness that should be in play is that of consensus within a scientific community, not that of “conclusiveness” as she uses it. And just as serious underdetermination worries are local, it is quite possible that a consensus emerges that one of a pair of competing empirically equivalent theories is uniquely selected on the basis of ampliative adequacy. Nor ought we to take forced beliefs as paradigmatic of rationality or objectivity.

This leads to my final point of criticism.<sup>17</sup> Ivanova arguably goes too far in the direction of reading Duhem as holding definitively like van Fraassen that only a later experiment that gives advantage in empirical adequacy provides grounds for justified belief. McMullin held that debate over the deductive and ampliative reasoning in the sciences “usually masks a deeper difference about the epistemic function of theory itself” (507). This is certainly evident in the present discussion over how to interpret Duhem’s notion of good sense, which by all accounts wasn’t very well developed by Duhem anyway. Duhem was always walking a thin blue line between empiricist conventionalism and realism, and my reading of him has him walking a similar line here. It is true that Duhem did in one passage characterize ampliative criteria as “essentially subjective, contingent, and variable with time, with schools, and with persons” (1954, 288). But this seems to be an obvious over-generalization on his part and anyway, by way of counterpoint, he also claimed that “Pure logic is not the only rule for our judgments: certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable”

<sup>17</sup>I tend to agree with Laudan that we should distinguish the (true but weak) general claim that theories are underdetermined by data from the (false because over-generalized) claim that theory-choice is always underdetermined by methodological standards. On my view Ivanova presents a straw-man version of McMullin in numbering him among those who supposedly “believe that we can always choose a unique theory from a set of empirically equivalent rivals by simply pointing to the amount of virtues the chosen theory possesses”. Nor is it true that “both views [realism and empiricism] assume that theory virtues lead to a conclusive choice between underdetermined theories” (this volume).

(1954, 217). Arguably also, identifying Duhem with van Fraassen’s constructive empiricist account of theory confirmation would challenge rather than support the axiology of “natural classification” Ivanova (2011) highlights in Duhem’s philosophy of science. McMullin’s emphasis on the substantial role of theory virtues and ampliative reasoning in theory confirmation arguably *better* serves the axiology that Duhem’s “natural classification” evokes than does saddling him with the stance that only differences in empirical adequacy contribute to epistemic value or provide rational grounds for belief. For as McMullin writes, “Those who deny the ability of theory to reveal underlying structure will also tend to see empirical fit as the only feature of theory worth worrying about, with possible pragmatic concession for such features as lend themselves to convenience of use or utility of application...[whereas] those who saw in theory the way to discover real underlying causes of macroscopic regularities are likely to stress a variety of epistemic virtues and to insist that saving the phenomena is not enough.”<sup>18</sup> It is important that a virtue epistemology in philosophy of science refute the view that reduces the epistemic to the purely synchronic notion of evidential fit. That view militates against recognition of diachronic and “explanatory” virtues that scientific realists have drawn attention to. But establishing the relevance of a whole series of confirmatory theory virtues should not be thought to entail any grand conclusions about realism or anti-realism. The claim McMullin makes about how ontologically expansive the recognition of these theory virtues in natural science is, reflects back upon the ‘very different visions’ of scientific reasoning he finds in philosophy of science. But our point has been that a ‘tiered’ account of the role of theory virtues and expert judgment allows us to largely set these axiological differences between the realists and empiricists aside in favor of recognizing the richness of the ampliative desiderata available to scientists to resolve problems of local underdetermination. Theory virtues of both the synchronic and diachronic sort are enablers of rational preference.

## 5 Rational Reconstructionism Meets Normative Naturalism

So what implications for the epistemology and methodology of the sciences might follow from acceptance of the usefulness of virtue theory for philosophy of science? We haven’t the space to develop detailed answers to this question, but I make three suggestions here from the perspective of “inquiry focused” VE, recognizing that proponents of other versions might draw somewhat different implications. The first

<sup>18</sup>McMullin 1996, 17. Indeed McMullin sees this as extending to Duhem himself: “Theory assessment involves the faculty of good judgment (*bon sens*) which permits disagreement between competent scientists...What tends to decide the issue between competing theories is how they develop over time, to what extent their response to anomaly appears ad hoc, and so forth” (17; see Duhem 1954, 216–218, and compare Duhem 1991).

is the one we began with, the idea of a more varied or “dappled” conception of the relationship between the sciences, which it can be argued is an implication of our thesis of the *localized* nature of the most worrisome kinds of underdetermination. I purposely alluded to a term from Nancy Cartwright’s *A Dappled World* (1999) to describe this thesis, in part because I suspect there are substantial lines of support that could be developed between virtue epistemology and the thesis of *metascientific pluralism* she argues for. Pluralism as a metascientific level thesis presents an alternative to both the “unity of method” that Hempelian logical empiricists demanded, and to the epistemological relativism of some of the post-positivists.<sup>19</sup> Indeed the connection with a theory of epistemic virtues has already been made from the other direction by the editors of a notable collection, *Scientific Pluralism* (Kellert et al. 2006), when they explain,

Philosophers of science have begun to advance pluralism at the metascientific level, most notably with respect to epistemic virtues. A variety of views regarding the role, status, and identity of scientific or epistemic virtues has been advanced in the philosophical literature.... [Some pluralists claim] that which virtues should hold what degree of regulative status in any given research project is a function of features specific to the problem and of the particular aims of the research (2006, x).

My second implication is a quite different way of approaching questions of demarcation and of the relationship between disciplines or fields of research. This is the view of John Dupre, who more explicitly than other pluralists has suggested that “we try to replace the kind of epistemology that unites pure descriptivism and scientific apologetics with something more like a virtue epistemology.” If “no strong version of scientific unity of the kind advocated by classical reductionists can be sustained” (1993, 242), then “the successor to the quest for demarcation criteria between science and non-science may be an account of theory virtues that characterize scientific reasoning...we are much better off to think in terms of epistemic virtues, features of an investigative practice that confer credibility. No doubt the cardinal empirical virtue is a proper connection with empirical evidence, which is the large grain of truth in the criterion of falsificationism” (2010). The implication is an abandonment of the hierarchy of the sciences ideal, together with its presumption of clear demarcation criteria: “Many plausible epistemic virtues will be exemplified as much by practices not traditionally included within science as by paradigmatic scientific disciplines...No sharp distinction between science and lesser forms of knowledge production can survive this re-conception of epistemic merit. It might fairly be said, if paradoxically, that with the disunity of science comes a kind of unity of knowledge” (1993, 243).

These two purported bridges between philosophy of science and virtue epistemology are not very original, I am afraid, having been drawn by others. But I would

<sup>19</sup>Moderate historicism, according to which the ‘units of selection’ in theoretical enterprises of all types are historical research programs, and a moderate confirmation holism, according to which a test of one theory always depends on other theories and/or auxiliary and background assumptions, are well-suited to provide this kind of pluralism, but more radical versions of historicism and holism are not.

like to end by developing something that I think isn’t already found in the literature, which is a relationship of mutual support between virtue theory, often described as “Janus-faced,” and normative naturalism. This also ties back to the debate over Duhem’s *bon sens*, since both Stump and his critic Ivanova seem to think that if Duhem’s account of good sense is of value, it is due to its usefulness in some kind of project of rational reconstruction, whereas normative naturalism quite explicitly rejects the project of rational reconstructionism as a central task of the epistemology of the sciences.

I suppose there is a sense in which the person of good sense is best judged retrospectively, as the person who chose wisely from the perspective of future science. But this certainly isn’t the Duhemian meaning of good sense, nor is it the import of a study of a scientist’s methodological cognitive character. These by contrast are clearly *prospective* posits, concerned with guidance and best bets for a path of research to pursue. Ivanova seems at odds with herself in attributing to Duhem (and to virtue epistemologists) a project of using good sense to retrospectively “explain” the fact that underdetermination tends to be resolved prior to the availability of new evidence. To do this is to steep him in the assumptions of later rational reconstructionists, and to import along with this certain dichotomies that arguably make that project impossible to fulfill. My skepticism about this is not because retrospective explanations aren’t ever possible or helpful. Surely at times they are. It is because like Laudan and other normative naturalists I would repudiate the ideal of rational reconstructionism as central to the normative tasks of philosophy of science: “The requirement of rational reconstructibility is neither wanted nor needed” (1987, 21).

Like Duhem, Laudan rejects the notion that justification may proceed algorithmically, while also insisting that non-empirical conceptual considerations are crucial to scientific practice and meta-methodology. Positivists and many post-positivists alike misconstrue the underdetermination problem because they either mistakenly assume that theories possessing the same positive instances must be regarded as equally-well confirmed, or because “they assume that the only rational basis for rejecting [dismissing] a theory or hypothesis is if it has been definitely refuted.” Epistemic values and virtues (scientific axiology) may change somewhat as science develops, but we are still able to view rules possessing normative force as grounded in factual means-end relations.<sup>20</sup> Methodological rules are fixed by means-end relations, but our conception of ends—scientific axiology—is neither given nor timeless. We need also an axiology of inquiry whose function is to certify or decertify certain aims

<sup>20</sup>Methodology so conceived is basically “restricted to the study of means and ends.”; they are “best understood as relativized to a particular aim” and judged by whether they guide inquiry to its achievement. But far from the Quinean version of epistemology naturalized qua replacement thesis for normative epistemology, Laudan holds that “methodology gets nowhere without axiology,” and that “We thus need to supplement methodology” with an investigation into an *axiology of inquiry* (1987, 29). Axiology in turn is multi-faceted, and while generally naturalistic also “preserves an important critical and prescriptive role for the philosopher of science” (29).



as legitimate, for “methodology gets nowhere without axiology.” Against rational reconstructionism Laudan proposes normative naturalism:

[E]pistemology can both discharge its traditional normative role and nonetheless claim a sensitivity to empirical evidence ... normative naturalists hold that the best methods for inquiry are those which produce the most impressive results ... the naturalist uses the simple method of induction to ‘bootstrap’ his way to more subtle and demanding rules of evaluation which, in their turn, become the license for subsequent and yet more highly refined rules and standards .... (1987, 44, 58).

For the normative naturalist, as another of its proponents puts it, “there have got to be other criteria, coherence, simplicity, predictive fertility, explanatory power, that an epistemology, like a scientific theory, must meet, and it must meet them, not because they are intrinsic goals of science, but because they are instrumental ones, instrumental to the goal of attaining knowledge.”<sup>21</sup> Thus responsibilist and reliabilist concerns combine in the present view, which fits better the Janus-faced understanding of virtue theory as both a descriptive account and one aiming to provide prescriptions for the improvement of practice.<sup>22</sup>

In this more naturalistic alternative to the overt or ‘closet’ intuitionism of the rational reconstructionists, we need not preoccupy ourselves with the question of whether we can always replicate the choices of past scientists as rational. This is no grand mark of the adequacy of a methodology of science anyway. Instead we simply “inquire about which methods have promoted, or failed to promote, which sorts of cognitive ends.” History of science still plays a key role here, and indeed may be center stage in the evaluation of proposed methodological standards. But history of science is not in the mission Imre Lakatos (and even the early Laudan!) gave it of insulating an “internal” (rational) from an “external” (social) explanation for past successes. Agreeing with Longino and other social and feminist philosophers who call for deconstructing the rational/social divide constitutes my biggest divergence from Laudan normative naturalism. While sounding simple, I think of normative naturalism as actually demanding that we disassemble (along with the myth of universally valid methodological and epistemological standards), the rational-social dichotomy, the logic/psychology dichotomy, and the dichotomy between internal and external history of science—each of which rational reconstructionists have appealed to in a misguided attempt to maintain the independence of scientific ‘reason’ from the vagaries and messiness of scientific ‘practice.’

<sup>21</sup> Rosenberg 1990, 42–43.

<sup>22</sup> “In light of the requirement that the means *reliably* conduce to the desired end, normative naturalism might appear to be a form of reliabilist epistemology. There do, however appear to be a number of salient differences between normative naturalism and reliabilism, at least as it is classically understood (e.g., Goldman (1979)). First, for Goldman a reliable method is one which leads reliably to truth, whereas for Laudan the cognitive ends in question are typically something other than truth. Second, reliabilism is a theory of the justification of an agent’s epistemic states, whereas normative naturalism is a theory of the justification of method. Thus, rather than take a reliabilist view of individual epistemic rationality, Laudan operates with an instrumental account of rationality on which an agent’s belief that an action will lead to their aim is required for the act to be rational” (Sankey).

What would seem to be obviously false dichotomies between the rational and the social have much staying power, however. One main reason for this is that because underdetermination problems leave us with questions about how scientific theories are chosen when empirical evidence fails to determine one theory as uniquely choice worthy, they also *appear* to present us with a referendum on the rationality of science. They do not. Only a philosophy of science in the service of rational reconstructionism, or a radical historicism that uncritically assumes the same dichotomies in order to take the opposite, relativistic side of the issue, pushes us towards any such referendum. So for instance we hear that to ground theory choice in anything else but hard data impugns the objectivity of the theory chosen, and of science itself; we must perforce seek sociological explanations of scientists’ cognitive choices. Or we hear that if assumptions of some sort are required to mediate the relation between data and hypothesis, these assumptions “can be the vehicles on which cultural ideology or social values ride ‘right into’ the rest of science”.<sup>23</sup> Or again we hear that because appeal to theory virtues are a means of “persuasion” between advocates of different empirically equivalent systems, these concepts introduce a rhetorical dimension that should be foreign to science were it really objective.

But the correct response to each of these mistaken claims, it seems to me, is to reject from the outset the notion that scientific reasoning should be characterized by a sort of epistemic purity that social practices of other sorts lack. Again, rhetorical strategies of persuasion, like underdetermination worries, are ever-present in some areas of research yet rare in others, and their absence or presence never did and never will neatly divide scientific from other forms of inquiry. To the extent that we take the advice of Longino and others to “disassemble the rational-social divide” the global referendum notion simply fades away, while local problems where guidance is actually needed come more clearly into focus. So rather than setting the rational and the social, or again epistemology and history, against one another as the project of rational reconstructionism has done, a virtue epistemology tied to normative naturalism would take another path. “We may still be able to construct a philosophy of science that derives *both* from the learning that has gone on in history *and* from a more general logical and epistemological framework” (McMullin 1984, 57). If this is indeed a viable goal, then virtue epistemologies I suggest will need to be disassociated from rational reconstructionism and developed in tandem with a more naturalistic project of scientific meta-methodology, yet one that preserves important “critical roles” for the philosopher of science (Laudan 1987, 29).

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<sup>23</sup> Longino 1990.

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