Virtue and Contingent History: Engineering Science

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> Physics as a whole is always in a state of incomplete coordination between extraordinarily diverse pieces of its culture: work, machines, evidence, and argument. That these messy pieces come together as much as they do reveals the presence, not of a constricted calculus of rationality, but of an expanded sense of reason (Galison 1997, xxii).

INTRODUCTION

In *Bayes or Bust*, John Earman expresses a thought, one that appears to be widely shared,[1] about what an adequate epistemology of science might look like:

> To my mind, the most interesting aspect [of science] is the epistemic one. I insist . . . that this aspect be explained in Bayesian terms. This implies that all valid rules of scientific inference must be derived from the probability axioms and the rule of conditionalization (Earman 1992, 204).

Earman's thought is that a constricted calculus of rationality—the probability calculus and the rule of conditionalization—suffices to characterize "the epistemic aspect of science," including the phenomena of scientific knowledge and empirical justification. But what if a rationality bound by a constricted calculus is a rationality trammeled and inhibited from accomplishing all that it might? Here I'll offer a picture of a relatively untrammeled variety of rationality—an

[1] Joyce (2011) and Weisberg (2011) are two recent surveys of variations on the thought.

expanded sense of reason—and the epistemic aims its operation in the sciences might advance. In developing this picture, I'll rely on a notion of virtue as (some think) Aristotle conceived it: virtue as a second-nature capacity to see reason, a capacity inculcated through having lived a particular concrete history, a capacity not reducible to the application of a formal calculus. The approach to an expanded sense of scientific reason I sketch here has implications for the social structure of science. I'll try to gesture at some of those implications and some questions they raise.[2]

But I have some obligations to discharge first. My mission is to present *virtue* as a way of thinking and talking about an expanded sense of reason operating in science. Natural questions prompted by this mission include: expanded *compared to what*? And *why* should this expanded sense of reason matter to *epistemology*? The next two sections will tackle these questions respectively. Then the final section will address the question of *how* to care about the variety of reason I try to describe here. That is, it will consider epistemic engineering consequences of the idea that at least some sensitivities to reason are importantly analogous to the sort of virtue wrought by having lived an appropriate contingent history.

A CONSTRICTED CALCULUS OF RATIONALITY

[2] Not for the first time. The present essay is a short-form remix of Ruetsche (2004), which uses epistemic second natures to characterize and evaluate a variety of theses in feminist epistemology.

To draw attention to the features of "mainstream" epistemology she takes to be problematic, Sandra Harding cites Francis Bacon's invocation of "a way of discovering sciences [that] goes far to level men's wits, and leaves but little to individual excellence; because it performs everything by surest rules and demonstrations" (Bacon 1960, I.cxxii). Bacon's invocation suggests a mission for the epistemologist: analyze *without residue* the epistemologically central commodity of the *justification* of belief in terms of these rules and demonstrations. I'll use the label "Traditionalist*"* for epistemologies that take their mission to be coextensive with the one Bacon describes. Traditional epistemologies of science seek to characterize a constricted calculus of rationality governing empirical justification.

Earman's aspiration to explain the "epistemic aspect" of science by deriving "all valid rules of scientific inference" from "the probability axioms and the rule of conditionalization," expresses a commitment to Traditionalism. The form of Traditionalism Earman espouses is known as *Bayesian subjectivism*. I'll explicate the approach in more detail, in order to articulate some key features of it. By using Bayesian subjectivism as an example, I don't mean to endorse it, nor do I mean it to imply that all Traditionalists, much less all mainstream epistemologists, are Bayesians. Bayesian subjectivism is a good example to use here because it is meant to be both modest and resourceful in acknowledging the sorts of influences on successful science that escape the confines of a constricted calculus of rationality.

A subjectivist interprets probabilities as subjective degrees of belief; roughly, the probability that an agent assigns a hypothesis reflects her level of confidence in the hypothesis and can be gleaned from the betting quotients she'd accept as fair odds for a wager on the hypothesis. If the agent's degrees of belief violate axioms of the probability calculus, she's prone

to Dutch books, sets of bets at such odds that she's guaranteed to lose money. So, supposing it's rational to avoid Dutch books, her initial assignment of probabilities is, if rational, constrained by the axioms of the probability calculus. But only by those axioms: subject to the constraint, her initial assignment can embody stubborn prejudice and selfish fantasy.

As the agent makes her way through the world, she gathers evidence. If the agent is rational, she will, obeying the rule of conditionalization, respond to this evidence by revising her degrees of belief in such a way that her new probability function is just her old probability function conditionalized on the novel evidence. Bayes's rule, an axiom of the probability calculus, governs the details of the construction of her new probability function from her old one.

According to the Bayesian subjectivist form of Traditionalism, *good epistemic practice consists in updating one's subjective degrees of belief in response to new evidence by means of the rule of conditionalization and Bayes's rule*.

Bayesians emphasize just how modest a constraint this is. Deductive logic, we tell disappointed introductory students, will not tell us what to believe. It will tell us only, given that we believe some things, what other things we ought not—on pain of contradiction—believe. Just so, none of the Bayesian apparatus dictates our initial degrees of belief or our prior probability assignments. It tells us only how those degrees of belief ought—on pain of susceptibility to unscrupulous bookies—change in light of incoming evidence. What about influences other than a commitment to conform to inductive inference? Bayesian subjectivism leaves them plenty of room to maneuver in the epistemic life of a scientific agent. For instance, such influences could have (just about) everything to do with the assignment of prior probabilities to hypotheses. Degree of belief revision, Bayesians say, must nevertheless proceed according to the rule of

conditionalization and Bayes's rule, rules to which agents are beholden regardless of their prior probability assignments, their social stations, their hopes, desires, or plans. Following those rules gives agents epistemic justification, not to hold beliefs outright, but to revise degrees of belief in light of incoming evidence.

One reason to follow valid rules of scientific inference is that we thereby attain "objective" belief, belief conforming to objects (as opposed to subjects) in that it is supported by evidence rather than by parochial or idiosyncratic allegiances. Hence Bayesians' delight with "washing-out" results—results to the effect that (almost[3]) no matter what their initial degrees of belief (prior probabilities) are, gents will, by updating those degrees of belief via Bayes's rule and the rule of conditionalization, eventually converge in their probability assignments. Driven by evidence rather than by suspicion, this convergence promises to be objective in a way the initial assignments of personal probabilities are not.

Earman takes epistemic warrant to be governed by "valid rules of scientific inference," which he seeks to codify by appeal to this Bayesian apparatus. I'll call epistemic warrant characterized by adherence to a constricted calculus of rationality, such as Bayesian confirmation theory, *rarefied warrant*. Successfully articulating the rules of rarefied warrant *exhausts* the task of epistemology, Traditionally construed. Excise from science instances of following rules of rarefied warrant and no epistemic residue remains.

Traditionalists needn't remain silent about aspects of science other than its justificatory practices and the comprehensive rules they follow. They just have to deny *epistemic* status to

[3] Agents who are dogmatic—that is, agents who assign degrees of belief of 0 or 1—can derail washing out results.

these additional aspects—as Wes Salmon does when discussing N. R. Hanson's logic of discovery (Hanson 1961). Salmon takes Hanson to highlight "plausibility considerations" to the effect that some methods of hypothesis formation are more likely to generate true hypotheses than others. Acknowledging that such plausibility considerations do exist and do influence the practice of science, Salmon nevertheless distinguishes them from the justification achieved by "testing or confirmation," which he identifies as the proper concern of philosophers of science (Salmon 1966, 111-14). Denying *epistemic* interest to plausibility considerations eluding characterization via a calculus of confirmation, Salmon expresses a commitment to Traditionalism.

Enough has been said to distill key features of Traditionalism and the constricted calculus of rationality it pursues: Traditionalism distinguishes the contexts of discovery and justification. It locates the epistemic achievements worth tracking in the latter and devises for their capture a net taking the form of an inductive logic or confirmation theory, a codification of the extent to which evidence (wherever it comes from) supports hypotheses (wherever they come from). Every inference conforming to the rules codified is epistemically warranted, regardless of the particularities of its setting; and every attainment of epistemic warrant in the sciences is an instance of an inference conforming to the rules. Every rational agent has what it takes to achieve rarefied warrant—and so *differences* between agents, their contexts, their histories, etc., *hold no interest* for the epistemologist.

Critics of mainstream epistemology of science seek a "less partial and distorted" (Harding 1991, 1) way to understand scientific knowledge. Some critics express a suspicion of the very idea of what I've been calling rarefied warrant, understood as a species of empirical

justification accessible to all and codifiable by a universally applicable calculus of rationality. I am content here to waive that suspicion and agree that Traditionalists set out to characterize an epistemic phenomenon worth our attention. However, Traditionalism can be partial (in the sense of incomplete, if not in the sense of unfair) if responsible scientific practice involves epistemic achievements that do not consist in attaining rarefied warrant. If such epistemic achievements exist, Traditional epistemology is incomplete. It becomes distorted if it appends to its analysis of rarefied warrant the claim that attaining such warrant is the only kind of epistemic achievement there is.

Traditionalism could be missing something. The notion of *virtue* affords a way to characterize what it's missing and how and why it matters that Traditionalism is missing it.

AN EXPANDED SENSE OF REASON

This section takes on three tasks. The first is to offer an account of Aristotelian virtue that articulates virtue as a model of an epistemic capacity that eludes characterization by means of a constricted calculus of rationality, such as Bayeisan subjectivism. The second task is to suggest that virtue-like epistemic capacities do operate in the sciences. If this suggestion is correct, Traditionalism is incomplete. Traditionalism can disarm this charge of incompleteness by contending that the capacities here likened to virtue aren't genuinely *epistemic*. The third task is to make a case that the capacities in question *do* deserve regard as genuinely epistemic—as well as to expose the epistemological presuppositions underlying that case.

I should hasten to emphasize that I don't mean here to take a stand on how to interpret Aristotle's ethics. I mean rather to develop a way of speaking (modeled, it happens, on a way of

speaking suggested to some by Aristotle's ethics) that turns out to be apt for probing possible limits of Traditionalism in the epistemology of science.

For Aristotle, man is by nature rational. But, Aristotle tells us in the *Nicomachean Ethics*, he is not by original nature virtuous. "The virtues are engendered in us neither by nor yet in violation of nature; nature gives us the capacity to receive them, and this capacity is brought to maturity by habit" (II.i).[4] The virtuous man has through careful training developed what we might call a *second nature* to respond in the morally appropriate way to his circumstances. The virtue acquired by cultivating the appropriate second nature is not a placeholder for a set of dispositions to deploy some moral calculus: "Still less is exact precision possible in dealing with particular cases of conduct; for these come under no science or professional tradition, but the agents themselves have to consider what is suited to each circumstance on each occasion" (II.ii). Virtue is not encoded in general rules, transparent to our inherent first nature, but rather a capacity born of contingent histories, inculcated second natures.

Aristotle's virtuous man can act appropriately without following universally transparent principles of right conduct. Does he therefore act without reason? Only if exercises of reason must take the form of following rules transparent to all. In his 1976 paper, "Virtue and Reason," John McDowell attributes to Aristotle an alternative model of rational action. For his Aristotle, the contingently ennatured sensitivity to moral demands that is virtue consists not in following rules but rather in exercising something like "a perceptual capacity," a capacity, as it were, to see reason (McDowell 1976, 332). The exercise of moral reason available to Aristotle's virtuous man is not available to those who, lacking the benefit of a proper upbringing, haven't developed the

[4] All Aristotle quotations are from the 1926 Horace Rackham translation.

appropriate second nature, the appropriate sensitivities to patterns of moral salience that the virtuous man has.

With a view toward transferring these notions to the scientific arena, it is worth noting that the position developed so far is consistent with an unabashed moral realism. Such a realism would hold that not every capacity—not even every socially habituated and celebrated capacity—is a virtue. Only the capacities the exercise of which constitutes getting things morally right—where moral rightness does not reduce to some conglomerate of (social) context-sensitive features—are virtues. Such a moral realist can maintain that our access to virtue is socially conditioned without adopting a relativism according to which *what virtue is* is determined by social context. On this view, moral reality is not a creature of context, but the capacity to track it labeled "virtue" is.

The story thus far: Aristotelian virtue is a sensitivity to reason that is (i) contingently engendered (that is, whether one develops it or not depends on one's social, historical, political, etc. context) and (ii) resistant to subsumption under universal rules. McDowell's stated aim in developing his take on Aristotelian virtue is to undermine the idea that "ethics [is] a branch of philosophy related to moral theory . . . rather as the philosophy of science is related to science" (1976, 331)—the idea, that is, that just as it is the task of the philosopher of science to elucidate scientifically justified belief by articulating the "rules of valid scientific inference" to which it conforms, so too is it the task of the ethicist to elucidate right conduct by articulating the "principles of acceptable behavior" to which it conforms (1976, 331). McDowell protests that the analogy banishes virtue, understood as the exercise of a second-nature sensitivity to moral reasons, from the ethicist's landscape. He invokes Aristotle to inspire us to reinstate it.

Suppose that McDowell has persuaded us about ethics and moral theory. We should not therefore lose interest in the analogy

ethics:moral theory::philosophy of science:science.

For now we can ask: should philosophy of science ever stand to science as McDowell would have ethics stand to moral theory? That is, are there epistemic capacities operating in the sciences analogous to the practical rationality that Aristotle's virtuous man, exercising his contingently ennatured perceptual capacity to see aright, deploys? If so, the epistemic achievements attained by exercising such capacities could not be assimilated to (for example) obedience to an inductive logic or confirmation theory, or to any other model that takes epistemic achievement to consist only in adherence to a template provided by universal rules.

Traditional epistemology offers models of scientific warrant that suppose it to conform to such a template. But if second-nature epistemic capacities matter in the sciences, even an adequate account of rarefied epistemic practices falls short of being an adequate epistemology of science.

Something like the suggestion that scientific practice includes, and includes critically, second-nature epistemic achievement has been made by a host of philosophers. The host includes Nancy Cartwright (1989), Ian Hacking (1983), Peter Galison (1997), and (maybe even) Mark Wilson (2019). Members of the host would shift the locus of critical scrutiny from physical theory, conceived as a global, complete, logico-mathematical entity, to the localized, in some sense fragmented, practices and interactions of theorists, applied mathematicians, experimentalists, and their equipment. Traditional epistemology relegates the question of where hypotheses come from to the capricious context of discovery, and relegates the question of where

evidence comes from to the machinations of laboratory technicians, so that it might focus on the question of the extent to which evidence (wherever it comes from) warrants hypotheses (wherever they come from). The philosophers I've just mentioned draw our attention to activities that Traditional epistemology of science thereby neglects, activities such as seeing through a microscope or a telescope, using instruments to tell phenomena from artifacts, knowing your recalcitrant laboratory apparatus well enough to keep them working, crafting phenomenological laws and other useful approximations, communicating with representatives of scientific subcultures other than the one in which you were raised, and so on.

Hacking, for example, insists:

> Noting and reporting readings of dials—Oxford philosophy's picture of experiment—is nothing. Another kind of observation is what counts: the uncanny ability to pick out what is odd, wrong, instructive or distorted in the antics of one's equipment (1983, 230).

"[Such] observation is a skill," he declares. "Some people are better at it than others. You can often improve this skill by training and practice" (1983, 168). Caroline Herschel is a case in point.

> I think that Caroline discovered more comets than any other person in history. She got eight in a single year. Several things helped her do this. She was indefatigable. . . . She also had a clever astronomer for a brother. . . . When she did find something curious "with the naked eye," she had good telescopes to look more closely. But most important of all, she could recognize a comet at once. Everyone except possibly brother William had to follow the path of the suspected comet before reaching any opinion on its nature (1983, 180).

Donna Haraway describes another example, one to which we'll return, of evidently acquired abilities to see:

> Devore [one of Washburn's male students] *literally saw* a male-centred baboon troupe structure . . . Jay [one of "Washburn's daughters"] *explicitly saw* the infant as a key centre of attention in langur troop structure . . . she *literally, physically saw* what almost could not figure in her major conclusions because another story ordered what counted as ultimate explanation (1991, 95, 96).

What drops out of what Hacking calls "the Oxford picture" is the fact that to focus exclusively on the inferential significance of an observation—its location in a nexus of confirmation relations—is to neglect the epistemic achievement of skilled coping that made that observation possible.

Skills of scientific observation are duly analogous to virtue: both are brought to maturity by habit—in some cases, by habit honed in very particular social contexts; both are accomplishments that, notwithstanding their contextual conditioning, can count as latching on to real features of the moral/empirical world. So let us suppose that Herschel's capacity to recognize comets, and similar capacities of other scientists, are epistemic achievements, best understood as the exercise of second-nature competences. If Traditionalism lacks the resources to understand them as such, Traditionalism is an incomplete epistemology of science.

Against such a charge of incompleteness, the Traditionalist might well protest that, while surely praiseworthy, Herschel should be viewed as a tank of evidence fueling a confirmation engine. Traditional epistemology aims to investigate the mechanics of the engine, not the

composition of the fuel. Even if successful data gathering is an achievement promoted by the exercise of second-nature competence, the *epistemic* aspect of science has to do with what is done with the data and takes the form of obedience to perfectly comprehensive rules of scientific inference. So in the face of the charge that they neglect second-nature recognitional capacities, Traditionalists could well confess both that they do and that they never meant not to.

I'll meet the Traditionalist protest with a pair of quasi-historical examples, meant to suggest that epistemic good conduct requires more of scientists and their communities than diligently updating on evidence. If the suggestion is true, *epistemically* significant features of scientific practice escape the notice of Traditional epistemology of science.

I'll call the first example, which is inspired by Alison Wylie's (1997) discussion of 1983 work by Gero, *the Bison-Mammoth Construct*. It is the 1970s. Edgeware analysis—using techniques including microscopy to study the physical morphology of archeological relics with a view toward identifying their likely uses—is a feminized subdiscipline of archeology. It is also low-status—low enough that the mainstream is slow to take up evidence from edgeware analysis. Instead, the mainstream is busy articulating the Man-the-Hunter paradigm, theorizing about bison and mammoth hunting practices and the social, technological, linguistic, and cognitive adaptations they required, as well as collecting evidence (for instance from kill sites) informing these theories. A felt puzzle arises for the Man-the-Hunter paradigm: When the big game Man Hunted and relied upon (according to the paradigm) for His subsistence went extinct, how did Man avoid following suit? The (ignored) testimony of edgeware analysis: Many of Man-the-Hunter's tools served foraging purposes; ergo Man-the-Hunter wasn't in fact subsisting

solely on bisons and mammoths. The puzzle dissolves, and the paradigm that frames it is revealed to be incomplete.

The Bison-Mammoth Construct is the story of a community that persisted in embracing a theory longer than it should have. It persisted even after evidence challenging the theory surfaced, as it were offstage from where the tastemakers were theorizing. *Even if* all members of the tastemaking community diligently updated their credences by conditionalizing via Bayes's rule on the evidence at hand, they were engaging in *epistemic bad behavior*. Enabling this epistemic bad behavior was the fact that the community was striated by status in such a way that its status hierarchies induced hierarchies of credibility that retarded or prevented relevant evidence from informing belief dynamics.

The foregoing diagnosis of epistemic bad behavior exhibited in the Bison-Mammoth Construct is a normative epistemological claim, and an important one. Yet it is a claim liable to elude the grasp of Traditionalism. The claim that status hierarchies have epistemic costs attributes *epistemic* significance to *contingent social structures* (ones that happen often to be indexed by race, gender, etc.). If status hierarchies carry epistemic costs, then we can't understand everything we ought to understand about empirical justification if we ignore social structures and the contingencies they condition.

> The Bison-Mammoth Construct also illustrates a moral Peter Railton draws lucidly:
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> Comforting Bayesian thoughts about the influence of prior probabilities washing out in the long run require not only a very long run, but also a short run full of *many more alternative hypotheses* and *much more active conditionalization* than anything we actually see in scientific practice (1994, 77).

Taking the moral to heart, we might suspect that Traditionalism is an incomplete epistemology of science. It's incomplete because it lacks the resources to understand as *epistemic* contributions efforts that *in the short run* enrich the collection of alternative hypotheses under examination, more active conditionalization.

Even if Traditionalism is incomplete, do we have reason to expect that second natures modeled on virtue might help to deliver *epistemic* goods that Traditionalism can't? I think we can support for this expectation in my second quasi-historical example, *Swashbuckling Primatologists,* which is inspired by Donna Haraway (1991) and Sarah Hrdy (1986). Swashbuckling post-World War II primatologists are entertaining hypotheses about the crucial role that male-dominance hierarchies play in primate social structures, and evaluating those hypotheses against evidence collected from field studies, during which those primatologists paid most of their attention to manifestations of male aggression. They provisionally accept what, according to rules of rarefied warrant, are, in light of the data collected, the best of these hypotheses. A subsequent generation of primatologists, many female, many working in the wake of the women's movement, articulate alternative hypotheses about social structures, ones according significance to troop members who aren't adult males. These hypotheses are evaluated against observations of a broader range of troop dynamics than the swashbuckling primatologists made, and are, *according to the Traditionalist's evidential norms*, better supported by those observations than the swashbuckling hypotheses.

The epistemic moral of the Swashbuckling Primatologists example is that interests and habits, second natures, can play a positive role in the juggernaut of conjecture and refutation. The moral so stated concedes to the Traditionalist that valid scientific inference obeys rules of

rarefied warrant. It nevertheless resurrects the epistemic significance in the context of justification of factors usually buried in the context of discovery and corrects for the exclusivity of the Traditional focus on rarefied warrant by suggesting that contingent histories can constructively interact with inferential engines.

In the Swashbuckling Primatologists example, what recommends the later over the earlier hypotheses is that they are better supported, not that they are un- (or less- or differently) biased. But not even religious adherence to norms of valid epistemic warrant will justify a community in choosing a hypothesis it hasn't thought of, supported by evidence it hasn't collected, over a recognized hypothesis supported by available evidence. The beliefs to which rational Bayesian engines drive convergence are only as well-validated as our evidence and hypothesis sets are rich. Thus, the capacity to enrich those sets (vouchsafe evidence, imagine alternate hypotheses) contributes as significantly to the justification of hypotheses as does following rules of valid scientific inference. Insofar as exercising competences consigned to the context of discovery endows us with such capacities, those competences qualify as "epistemic resources" in much the same way as does the capacity to implement an inductive logic. Not specific to Bayesian subjectivism, the point can be adapted to apply to any Traditional epistemology that withholds epistemic significance from "context of discovery" factors that can promote the reliability of scientific inquiry.

If it is the aim of inquiry to adopt the best possible hypotheses for the best possible reasons, diligent adherence to evidential norms does not accomplish the aim on its own, but only in conjunction with the cognitive and technical innovations that bring hypotheses and evidence alike within the purview of those norms. Insofar as second-nature capacities for such innovations

promote the aim of inquiry, they are as genuinely epistemic capacities as is the capacity to follow rules. And examples already given make it plausible that there are social/scientific circumstances under which second-nature capacities can and do promote the aim of inquiry.

I'll close this section by briefly making explicit the connection between epistemic second natures and the possibility of a *feminist* epistemology of science (see Ruetsche 2004 for elaboration). I say "make explicit" because the suggestion is implicit in the quotation from Haraway about what "Washburn's daughters" *literally physically saw,* as well as in the Swashbuckling Primatologists example. To forge the connection, start with the view that second-nature capacities for cognitive and technical innovations promote the aims of inquiry and thereby deserve to be hailed as epistemic capacities. Append to that view the claim that there have been and might be domains of inquiry enveloped in such social situations that the gender and social situations of inquirers and potential inquirers—their second natures—affect (but in less dramatic versions of a feminist epistemology, fall short of determining) the capacity for cognitive and technical innovations. The result is a picture of how gender can matter to the epistemic dimension of inquiry and matter in a way an epistemologist concerned only to articulate evidential norms might miss. The picture affords an understanding of how gender (and other contingencies) can matter substantially to inquiry. Because nothing about the picture cancels the broadly empiricist requirement that *evidence* is essential to empirical validation, the connection to gender is forged without reducing inquiry to wish-fulfillment.

ENGINEERING SCIENCE

The epistemology of science I'm calling Traditionalism aims to articulate the contours of *rarefied warrant*, an epistemic achievement attained by conforming to a calculus of rationality transparent to our rational first natures, and so an epistemic achievement to which differences between aspiring knowers make no difference. In the previous section, I've used the model of Aristotelian virtue, understood as a second-nature capacity, to propose that we recognize other sorts of epistemic achievement: epistemic achievements to which *differences* between knowers, their *contingent histories*, and their *contexts* are relevant.

The epistemology lying in the background of this proposal is broadly *externalist*. What qualifies second-nature capacities *as* epistemic is their efficacy in promoting aims of inquiry, such as sorting empirically adequate beliefs from empirically inadequate ones and enhancing the variety and the force of the evidence we have for the hypotheses we embrace. The epistemology lying in the background of the proposal is also *social* in at least two senses.

First, it recognizes epistemic achievements conditioned by social context. An example of an epistemic achievement conditioned by social context is the capacity to imagine hypotheses about primate social structure which aren't framed in terms of male-dominance hierarchies. The externalist grounds for counting the capacity as *epistemic* are that a community where the capacity is exercised collects more and better evidence than a community from which the capacity is absent. The community enhanced by the capacity entertains a wider variety of alternative hypotheses suggesting experimental approaches generating a richer supply of evidence. A reason to count the capacity as conditioned by *social* context is that, as a matter of historical fact, scientists who had had their consciousnesses raised by the women's movement were able to imagine alternatives to male dominance as a central explanatory trope, alternatives

that did not occur to scientists to whom the centrality of that trope seemed so natural as to be beyond question. It's plausible that the social locations of scientists made a difference to their capacities to contribute alternative hypotheses that challenged underlying assumptions of received models.

The proposal that second-nature epistemic capacities operate in the sciences is supported by a background epistemology that is social in a second sense. The background epistemology treats scientific communities as relevant epistemic units. For instance, the framing epistemology faults the Bison-Mammoth community for being so striated by status that it was slow to take up salient evidence recognized by outlier subcommunities. For another instance, the framing epistemology lauds as *epistemic* the second natures whose presence *in a scientific community* better enables *that community* to discharge its epistemic duties.

The social dimension of the epistemology of science framing the proposal developed here invites a question of epistemic design. *How would a scientific community be organized, so that it might make highly effective use of epistemically relevant second natures?* This is a question of which Aristotle would approve. He considers "politics . . . pre-eminent among sciences, because it is responsible for organizing the state in such a way that its citizens become virtuous and that it itself attains its Good" (II.i). A well-designed *polis* fosters virtue among its citizens, and the virtue of the citizens constituting a *polis* enables that polis to thrive. Our epistemic design question attributes to the epistemic engineer a pre-eminence analogous to that of Aristotle's politician. The epistemic engineer is responsible for organizing science in such a way that its practitioners bring to bear second natures that enable science to attain its Good, which we are

supposing to consist in developing empirically cogent understandings of the natural and social world.

A possible tension lurks in the design task facing Aristotle's politician. Exercising virtue, a creature is supposed to attain the kind of thriving appropriate to it. But what guarantees Aristotle's politician that by designing a polis that delivers its *individual* citizens to thriving on their, human, terms, one thereby delivers the *community* consisting of those individuals to *its* overall Good? The balance of this section will discuss similar tensions lurking in the design task facing our epistemic engineer. A strategy for relieving them, suggested by the work of Helen Longino and congruent with the story told here, is to reconceptualize the Good of science in design terms.

The examples presented in the last section indicate that a well-designed scientific community requires a wide variety of (at present often uninstantiated) broader social conditions to obtain—a point to which I'll return. The Swashbuckling Primatologist example suggests that in a well-designed scientific community, a relevant heterogeneity of second natures will participate.[5] And the Bison-Mammoth Construct example alerts us that the mere presence in a scientific community of a relevant heterogeneity of second natures may not be enough. The relevant heterogeneity of second natures must also be *enfranchised*, in the sense that their

[5] For some recent empirical work on what sorts of heterogeneity are relevant to what sorts of inquiries, see Page (2017).

deliverances get sufficient uptake to affect community-wide processes of hypothesis development and evaluation. A community striated by prestige may frustrate such uptake.[6]

The design desiderata of *representation* and *enfranchisement* recall Helen Longino's epistemic ideal of a "cognitive democracy" (1990, 396), a community characterized by equality of intellectual authority. And Longino's approach to the epistemology of science helps to illuminate connections, raised by the design question, between individual second natures and the Good of science. Longino urges us to understand epistemic accomplishment (knowledge, objectivity, and the like) in the sciences *not* as the accomplishment of individual unconditioned subjects *but* as an accomplishment of *communities* of *conditioned* subjects.

> Scientific knowledge is constructed not by individuals applying a method to material to be known but by individuals in interaction with one another in a way that modifies their observations, theories, and hypotheses, and patterns of reasoning (1992, 395).

Translated into the language of the present essay, Longino's proposal is to understand scientific knowledge as resulting from the right sorts of interplay of second natures.

But what is the right sort? It might seem that, having abandoned a constricted calculus of rationality as the sole arbiter of epistemic achievement, we have no way to draw the sorts of distinctions that would enable us to answer this question. Longino denies this appearance. Holding that "community level criteria can be invoked to discriminate among the products of scientific communities, even though context-independent standards of justification are not attainable" (1992, 396), Longino proposes to compare, not hypotheses with respect to their

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[6] For formal results supporting this contention, see Hegelsmann and Krause (2002), which Ruetsche (2019) discusses in this connection.

objectivity, but communities, with respect to their capacity to produce objective beliefs. A distinctive account of scientific objectivity emerges from the comparison:

> The greater the number of different points of view included in a given community, the more likely it is that its scientific practice will be objective, that is, that it will result in descriptions and explanations of natural processes that are more reliable in the sense of less characterized by idiosyncratic subjective preferences of community members than would otherwise be the case (1990, 80).

What Longino terms *transformative criticism* is an important means of exposing idiosyncrasies and promoting objectivity as she understands it. Transformative criticism is criticism that exposes and targets widely shared background assumptions (1990, 76). It "requires the presence and expression of alternative points of view" (1992, 396). And "a method of inquiry is objective to the degree that it permits transformative criticism" (76). Translating again into this essay's vernacular, transformative criticism, and so objectivity as Longino understands it, is sustained in a scientific community designed to represent and enfranchise heterogeneous second natures.

Naming the Good of science "objectivity," Longino identifies it procedurally, in terms of a community structure permitting and promoting transformative criticism. Following Longino, we should be reassured that our epistemic engineer's design task won't come to ruin on tensions between individual scientific thriving and community-wide scientific thriving. On Longino's account, a scientific community organized to enable its participants to realize, represent, and enfranchise their epistemic second natures is *ipso facto* a scientific community that realizes its own Good.

This is hardly to say that the approach to the epistemology of science here presented is without tension. A working assumption that does the critical work of qualifying second natures as *epistemic* is that whatever promotes uncontroversial epistemic values (such as empirical adequacy or convergence to the truth) is thereby an epistemic capacity, indeed an epistemic virtue. Considering the working assumption from the perspective of the design question has implications, some of which are surprising, for what virtues are epistemic.

What virtues are epistemic? On this telling, those second-nature capacities whose full realization in a well-designed scientific community promotes that community's pursuit of transformative criticism and hence of objectivity procedurally construed. The pursuit by a community of transformative criticism arguably requires of its members a suite of individual-level capacities—the humility to take the testimony of others into account, the patience to try to understand evidence arising from approaches imperfectly aligned with one's own, the trust to expend time and energy updating on evidence contributed by others. All of these individual-level capacities could well promote community-level pursuit of transformative criticism. And that would make humility, patience, trust—*prima facie* moral virtues—epistemic virtues as well.

Peter Railton has suggested a less uplifting variation on the story just told. Like Longino, Railton reconceives objectivity in terms of appropriately designed scientific communities. He acknowledges that science as we know it is commodified, intent on prediction and control, intensely competitive, conducted by groups "led by dominant principal investigators—usually male—who are overcommitted to their research program" (1994, 79). Then he observes that *just such a design* is one you might adopt for a community "whose forms of interaction . . . might

selectively reward reliability in belief formation" (81). You might adopt it because reliability fosters prediction and control, and hypercompetitive, overinvested groups competing for the monetary awards subsequent upon identifying effective strategies of prediction and control "might better fit the bill of producing novel forms of feedback that can frustrate subjective projection" than disinterested inquiry (82). In the context of the design strategy Railton explores, the individual-level capacities that promote the collective pursuit of objectivity include arrogance and overconfidence. Moral vices[7] emerge as epistemic virtues!

Noticing *that* epistemic virtues matter affords a way into—without settling!—the question of *which* virtues matter. The conflicting derivations just produced, one of arrogance and the other of humility as an epistemic virtue, suggest that background social conditions, assumed as given, could make a difference to how to answer what I've been calling the design question. But—like Aristotle's politician—we needn't take background social conditions to be given. We can also ask: What background social conditions must obtain, in order to permit a scientific community to be organized that makes efficient use of epistemic second natures? Supposing we could determine an answer to this question, it's not obvious that we would like it. A social organization that fully promotes epistemic values could well sacrifice others' values, such as fairness or equity (think of Plato's *Republic*). And even if the social conditions that, say, fully promoted transformative criticism *also* measured up as just in other respects, there could well remain moral and political qualms about the permissibility of steps that might bring those social conditions about.

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[7] I'm not sure that Aristotle would count arrogance as a vice—see his discussion of the moral virtue *megalopsychia* in *NE IV.3-4*. But I would.

I'm rooting for social justice, scientific objectivity, and individual thriving (both moral and epistemic) to turn out to be mutually reinforcing goods, and moreover goods that are both practically and ethically attainable. I don't know that they are. I have tried to suggest that one way to think about whether they might be is to expand our notion of scientific reason to incorporate epistemic capacities modeled on Aristotelian virtue.

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REFERENCES

Aristotle. 1926. *Nicomachean ethics*. Trans. H. Rackham. New York: G.P. Putnam's Sons.

Bacon, Francis. 1960. *The New Organon and related writings*, edited by F.H. Anderson. Indianapolis: Bobbs-Merrill.

Cartwright, Nancy. 1997. *Nature's capacities and their measurement*. Oxford: Clarendon Press.

Earman, John. 1992. *Bayes or bust*. Boston: MIT Press.

Gabbay, D., S. Hartmann, and J. Woods (eds.). 2011. *Handbook of the History of Logic*, Vol. 10: *Inductive Logic*. Amsterdam: Elsevier.

Galison, Peter. 1997. *Image and logic*. Chicago: University of Chicago Press.

Hacking, Ian. 1983. *Representing and intervening*. Cambridge: Cambridge University Press.

Hanson, Norwood Russell. 1961. “Is there a logic of discovery?” In *Current Issues in the Philosophy of Science*, edited by Herbert Feigl and Grover Maxwell. New York: Holt, Rhinehart, and Winston.

Haraway, Donna. 1991. *Simians, cyborgs, and women*. New York: Routledge.

Harding, Sandra. 1986. *The science question in feminism*. Ithaca: Cornell University Press.

——. 1991. *Whose science? Whose knowledge?* Ithaca: Cornell University Press.

Hegselmann, Rainier, and Ulrich Krause. 2006. “Truth and cognitive division of labor: First steps towards a computer aided social epistemology.” *Journal of Artificial Societies and Social Simulation* 9 (3): 10.

Hrdy, Sarah. 1986. “Empathy, polyandry and the myth of the coy female.” In *Feminist approaches to science,* edited by Ruth Bleier. New York: Pergamon.

Joyce, James. 2011. “The Development of Subjective Bayesianism”, in Gabbay, Hartmann, and Woods, 2011, pp. 415–476.

Kukla, Rebecca, and Laura Ruetsche. 2002. “Contingent natures and virtuous knowers: Could epistemology be ‘gendered’?” *Canadian Journal of Philosophy* 32 (3): 389-418.

Longino, Helen. 1990. *Science as social knowledge*. Princeton: Princeton University Press.

——. 1992. “Subjects, power, knowledge: Prescriptivism and descriptivism in feminist philosophy of science.” In *Feminist epistemologies,* edited by Linda Alcoff and Elizabeth Potter. *New York: Routledge* (1992).

——. 1996. “Can there be feminist science?” In *Women, knowledge and reality*, edited by Ann Garry and Marilyn Pearsall. New York: Routledge.

McDowell, John. 1976. “Virtue and reason.” *Monist* 62 (2): 331-50.

Page, Scott E. (2019) *The diversity bonus: How great teams pay off in the knowledge economy*. Princeton: Princeton University Press.

Railton, Peter. 1994. “Truth, reason, and the regulation of belief.” *Philosophical Issues* 5 (1): 71-93.

Ruetsche, Laura. 2004. “Virtue and contingent history: Possibilities for feminist epistemology.” *Hypatia* 19 (1): 73-101.

——. 2019. “What is it like to be a woman in philosophy of physics?” in *Routledge Handbook of Feminist Philosophy of Science,* edited by Sharon Crasnow and Kristen Intemann.

Salmon, Wesley. 1966. *The foundations of scientific inference*. Pittsburgh: University of Pittsburgh Press.

Weisberg, J., 2011. “Varieties of Bayesianism”, in Gabbay, Hartmann, and Woods, 2011, pp. 477–552.

Wilson, M., 2017. *Physics Avoidance: and other essays in conceptual strategy*. Oxford: Oxford University Press.

Wylie, Alison, 1997. "The engendering of archaeology refiguring feminist science studies." *Osiris* 12(1): 80-99.