In Defense of "Rationalist" Science

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Mainstream philosophy of science has embraced an "empiricist" approach.[1] To be slightly more precise, I venture that most philosophers of science today would endorse the view that experience is the source of most if not all scientific knowledge. The aim of this essay will be to challenge the consensus, by showing how we cannot and should not abandon all elements of the "rationalist" tradition, a tradition often identified with philosophers such as Descartes. The very idea that science could be founded on "reason," or in some sense *a priori* seems absurd, at least at first pass. Surely, today, it would be difficult to anyone who regards intuition or the "natural light" as a source of information about the world. Even logical and mathematical knowledge – once regarded as a paradigmatic examples of knowledge founded in "reason" – are now more often than not viewed as a product of experience, or, at very least, a result of combining experience and, perhaps, our psychological dispositions.[2] Further, many argue that explanations in the sciences must be empirical; thought experiments and mathematical truths may not "explain" – or, at least, they do not tell us anything interesting or new about "the world."[3] Appeal to 'pure reason' will get us nowhere; there is no longer any plausible role for "first philosophy." According to one popular conception of "naturalism," this is what it means, after all, to be a "naturalist"

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[1] This sense of empiricism is rather different from use of the term "empiricist" understood in contrast with "scientific realism," which concerns the ends of science, rather than the methods. While I have not conducted a survey, a search of "PhilPapers Survey" for "Target Faculty" in General Philosophy of Science shows that over half (56.3%) would categorize themselves as "empiricist". (http://philpapers.org/surveys/)

[2] For a much more detailed discussion of the source(s) of mathematical and logical knowledge, see Maddy, 2007.

[3] See, e.g., Norton and Brown for an exchange, in Hitchcock, 2004.

– and, we are all naturalists, now.[4]

How then might one defend a view so far afield from the consensus? My aim here will be to carve off some of the less palatable rationalist commitments from the elements of "rationalist science" I endorse. Although I am not the first to challenge the traditional characterization of the history of modern philosophy as a stalemate between continental rationalists and empiricists, this will be part of my aim, as well. There are several elements frequently identified with "rationalist" science (Stump, 2005)[5]: questioning of sense experience, and particularly, accepted, or "commonsense" observation, the attempt to rethink the "metaphysical" foundations of one's science (in the broadest possible sense), using either thought experiment, or appealing to demonstrative arguments purporting to establish 'necessary' truths (under certain assumptions), often (but not always) using either mathematics or geometry, appeal to the assumption that nature (or some part thereof) is itself "systematically" organized, and the associated assumption that unified theories (in some sense) are preferable, and more generally, appeal to "virtues" not usually considered "strictly empirical," such as simplicity. To some extent, labeling such methods "rationalist" is a bit artificial; both "empiricist" and "rationalist" thinkers questioned sense experience, used thought experiment, and appealed to simplicity. Indeed, this is (in part) my point; by using such methods, one does not depart from an ideal. Such "rationalist" tools are good ones for moving science forward; Descartes' ladder (or, perhaps, van Fraassen's "inferential wand") should not be thrown to one side. More detail about why will become clear as we progress; it's best explained by looking to examples.

The structure of the argument will be as follows. First, I will begin by discussing

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[4] Or, at least 49% of all respondents endorsed naturalism, but 64% of all specialists in general philosophy of science (see, www.philpapers/surveys/)

[5] Some, but not all of these are mentioned by Stump, (2005).

how Descartes exemplified some of the above virtues. Descartes stood at center stage in the history of science, and not simply in the history of philosophy (Shea, 1991; Garber, 1992; Ariew, 1999; Gaukroger, et. al., 2000, Gaukroker, 2002; Hatfield, 1979, 2002; Slowik, 2002)). His scientific work transformed physics; even if he was wrong about many of the conclusions he arrived at, he served as the interlocutor in absentia for much of subsequent physics. Second, I will discuss how and why a rationalist vision for science is not so counterintuitive as one might think; I illustrate several examples of scientists deploying "rationalist" methods to advance science.

Part I: Rationalist Virtues:

1.1 Skepticism about the senses.

Of course, what it means to be a "skeptic" about sense experience has varied over time, and has served different purposes in the history of philosophy and of science. Doubt of sense experience is a long-standing tool used by philosophers (or, if you like, "natural philosophers" – i.e., scientists) going back at least to the Pre-Socratics, for raising problems of knowledge. Skepticism, whether mild or radical, is a way of illustrating the underdetermination of belief by sensory experience. Whether the world is, in fact, exactly as it appears, was a question raised by "empiricists" and rationalists alike, from Aristotle to Hume. Galileo uses many vivid examples to demonstrate how our "common sense" perception can fail us. For example, Galileo addresses "common sense" Aristotelian objections to the idea that the earth moves by pointing to a series of theoretical scenarios where compound motion would be perceived as a single directional motion for a person who shared the motion of the object. (Tossing a ball in the hull of a moving ship, or on the back of a galloping horse, one would only observe the ball rise and fall, not the arc of forward motion combined with vertical motion.) Descartes, just like Galileo, argues that relying upon what one is "taught by nature" to discover natural kinds or true explanations is dubious. While he argues that "there is no doubt that everything I am taught by nature contains some truth," that truth must wait upon the "intellect" to "examine the matter." (CSM II : 82-83)

One of Descartes' most vivid examples, from the *Meditations*, concerns the size of the sun relative to the earth; relying upon direct visual inspection to determine the sun's actual size may be misleading. We must (ultimately) resort to geometrical reasoning, to conclude that, in fact, the sun is many times larger than the earth. For our purposes, it will be best to show how Descartes viewed skepticism, or perhaps better, doubt, as a tool. Doubting the senses was, for Descartes, the first step along the way toward a discovery of foundational truths. Descartes believed that a new physics required a new metaphysics. Descartes' skepticism about the senses is thus not merely a first step along the way towards discovery of the "clear and distinct" metaphysical truths he takes to ground his new science; it is also a "rearguard" action, if you like, against his opponents. His goal is not simply to motivate questions about the nature and possibility of knowledge, but also, to question a worldview. In order to do this, Descartes needs to question what are regarded as "commonsense" observations about the kinds of things there are in the world (men, trees, stars, etc.), and how we are to explain their behavior (form, matter, "natures").

At the end of the *Meditations*, Descartes notes that our ideas of external things are produced without our consent and cooperation (a cancer diagnosis, or a burned hand). Since we know (by *Meditation* 3, at least), that God is not a deceiver, our perceptions of these external things must be (when properly disciplined by reason) about the world, for

> [God] has given me a great propensity to believe that [ideas of corporeal things] are produced by corporeal things. So I do not see how God could be understood to be anything but a deceiver if the ideas were transmitted from a source other than corporeal things. It follows that corporeal things exist. They may not exist in a way

> that exactly corresponds with my sensory grasp of them, for in many cases the grasp of the senses if very obscure and confused. But at least they possess all the properties which I clearly and distinctly understand, that is, all those which, viewed in general terms, are comprised of the subject matter of pure mathematics. (CSM II: 80)

Descartes makes a number of points here – our sense experience is "about" the world, but our senses' grasp of the world is, in many cases, "obscure and confused." Here Descartes appeals to some important distinctions: "obscure and confused" versus "clear and distinct" ideas, things grasped via "sensory" experience versus "viewed in general terms." These distinctions are supposed to provide readers with clues as to which kinds of knowledge are trustworthy, at least at this stage in his analysis. His point, put simply, is that the senses may often deceive us. In contrast, the "subject matter of pure mathematics" – a field that includes truths of geometry, for Descartes, as well – is known with certainty. What is the relationship between these two? The clue comes with Descartes use of the expression, "viewed in general terms"; it is only when we abstract away from the appearances that we gain general, and more importantly, accurate understanding of the "real" properties of objects - i.e., properties of objects that truly belong to the object, and are not simply a byproduct of our senses – such properties will include size, shape and motion. A few paragraphs after this, Descartes makes an important distinction between things "taught by nature" and things "taught by reason." The former are aspects of corporeal things that are particular – such as sensations of sound, light, pain, or flavor. "Habitual" judgments of the sense "inform the mind of what is beneficial or harmful for the composite of which the mind is a part." (CSM II: 83) The trick is not to assume that the perceptual categories that we take for granted (sweetness, brightness, etc.) are what "ultimately" explains the phenomena. This is the fault that Descartes (fairly or not[6]) attributes to the

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[6] Thanks to E. Schleisser for making this point about Descartes rather uncharitable reading of his predecessors. See also, Grant, *God and Reason in the Middle Ages*, (2001), and Grant

“School metaphysicians,” that there is “something in the fire which resembles heat”:

> If you find it strange that I make no use of the qualities one calls heat, cold, moistness, and dryness, as the philosophers [of the schools] do, I tell you that these qualities appear to me to be in need of explanation, and if I am not mistaken, not only these four qualities, but also all the others, and even all of the forms of inanimate bodies can be explained without having to assume anything else for this in their matter but motion, size, shape, and the arrangement of their parts… (CSM I: 25-26)

Descartes’ argument is that perceived properties of bodies may “correspond” to “real” properties but not “resemble” them (CSM II: 81)– for instance, sweetness may “correspond” with geometrical features of sweet objects’ – e.g., the size, shape, and motion of their parts may correspond to their ability to cause sweet sensations. In other words, Descartes’ project in questioning the senses is not, ultimately, to show that *all* sense experience is fundamentally flawed. It is, at least in part, to challenge (what he took to be) Scholastic conceptual categories. That is, his argument is that qualities of heat, cold, moistness, are not “natural kinds.” His claim is thus not that our sense impressions of color, flavor, etc., are altogether useless, but that our psychological judgments (what we are “taught by Nature”) about the sweetness of a piece of fruit are exactly spot on, at least, for the purposes of our survival. They are, as it were, coded instructions by God to continue to enjoy this tasty mango. The mango is not a delusion, or caused by an evil demon. It is real, and it tastes good because God wants us to eat it. Of course, ultimately, he will argue that the true nature of the mango is that it is an extended, changeable thing, made of parts with definite sizes, shapes, and motions; knowledge of these mechanistic physical properties of the mango will be necessary for the future physics of mangoes (as well as of the future of flavor technology). However, we need not know this in order to know that it's sweet and

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and Murdoch, *Mathematics and its Applications to Science and Natural Philosophy in the Middle Ages*, (1987).

juicy, and just the thing for a nice afternoon at the beach.

The function of the senses, in short, is to convey information for preservation of the body. What is flawed is not sense experience per se, but habitual judgments about the senses - e.g., that the world is exactly as it appears to us, uncorrected by reason, and prompted, in part, by a good dose of skeptical reflection. The idea that secondary properties, (the sweetness or orange color of the mango) are really "in" the thing itself, is the view that Descartes aims to displace. In other words, Descartes' aim is not to reject experience altogether, but to show that the way we ought to do science is to replace our "commonsense" observations with a more "objective" understanding, i.e., to use reason to correct "habitual" empirical judgments.

Thus, the senses provide us with knowledge of particular ways in which we can and should come to approach and avoid pain and pleasure, but the "intellect" discerns general metaphysical properties of those particulars - their substance and modes - e.g., size, shape and motion. But how does the intellect discern these particulars?

1.2: Reason as a Source of Information about the World

Descartes viewed reason as a source of knowledge. This much is uncontroversial; what is far more controversial is what it means to say that "reason is a source of knowledge." A narrow sense of this would have it that knowledge founded on "reason" simply means all and only "a priori" propositions and claims demonstratively following from these. Is this what Descartes intended by use of "reason" as a way of knowing about the world?

No. One traditional reading of Descartes had it that he hoped that all of science should exactly follow the example of Euclid's *Elements* – a deductive demonstration of all of science from first principles that were known a priori. Anything less than perfect certainty was unsatisfactory; "scientia" is all and only "certain and evident knowledge." While Descartes may, early in his career, have hoped that the sciences might follow Euclid's *Elements in achieving the same kind of certainty*, it's clear that he did not believe – even then – that all of science could be deduced (in the narrow sense associated with the "syllogistic art") from a priori first principles.[7]

We must be very careful about the use of the term "demonstration." Descartes made a distinction between "proof" and "explanation" – or, what he called "a priori" justification, versus, "a posteriori" justification. The former moves from causes to effects, and is somewhat closer to our notion of demonstration as deduction, (but not the same); the latter moves from effects to causes. The word, "demonstrate," Descartes insists, can be used both for proof and explanation, "according to common usage and not in the technical philosophical sense."(CSM III: 198). So, when Descartes claims that he has used his reason to "demonstrate" certain truths, he is not (at least not for the most part) saying that he has proven truths by syllogistic reasoning from a priori first principles. Even in the a priori case, Descartes appeals to a somewhat broader sense of "demonstration" than what is called to mind by 20th or 21st century philosophers.

Descartes was aware that deductive demonstrations (in the sense in which syllogistic arguments are "deductive") prove no more than what is contained in the premises. Thus, early on in his career, he is at pains to distinguish his sense of "deduction" from that of the "logicians": "The syllogistic art is of practically no assistance in the search for truth… logicians can form no syllogism which reaches a true conclusion unless the heart of the matter is given, unless they previously recognized the very truth which is thus deduced."(CSM I: 406). Having been schooled by Jesuits, Descartes was certainly aware of

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[7] For a discussion, see Garber, 1978.

this classical objection to the possibility of knowledge via demonstration, discussed in the *Posterior Analytics,* (Book 1, Chapter 3, 72b). Purely "syllogistic" reasoning as a form of knowledge does not bequeath anything *new*. As Descartes hopes to reform the sciences, indeed, to offer a new, systematic account of the world as a whole, his (intended) sense of "demonstration" is not merely "syllogistic" reasoning.

As Descartes developed his scientific program, his early ideals about the nature and possibility of certainty in the sciences were jettisoned at least in practice, for what we would today regard as a much more probabilistic mode of argumentation.[8] In his empirical work, he makes appeal to the latter sense of "demonstration" – i.e., a posteriori demonstrations from effects to causes. Along the way, he appeals to parsimony, coherence, and unifying power of his theoretical framework; in short, a sense of "demonstration" that falls very far short of Euclidian proofs. Descartes argued with critics that this did not constitute a "failure"; he wrote to his colleague Mersenne:

> You ask if I regard what I have written about refraction as a demonstration. I think that it is, in so far as one can be given in this field without a previous demonstration of the principles of physics by metaphysics, and so far as it has ever been possible to demonstrate the solution to any problem of mechanics, or optics, or astronomy, or anything else which is not pure geometry or arithmetic. But to require me to give geometrical demonstration on a topic that depends on physics is to ask the impossible. And if you will not call anything demonstrations except geometers' proofs, then you must say that Archimedes never demonstrated anything in mechanics, or Vitellio in optics, or Ptolemy in astronomy. But of course nobody says this. In such matters people are satisfied if the author's assumptions are not obviously contrary to experience and if their discussion is coherent and free from logical error, even though their assumptions may not be strictly true. (CSM III:141)

This passage provides a useful way of explicating how Descartes (at least at this point in his career) viewed "geometrical" demonstrations as distinct from those to be found in the sciences. The former inherit their certainty from the certainty of their first principles.

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[8] For further discussion, see Garber, 1992; Hatfield, 1988; Raftpoulos, 1995.

The latter arguments, Descartes seems to suggest, are "demonstrative," to the extent that they rely upon assumptions not "obviously contrary to experience," and, when they are "coherent," and free from "logical error," they are even better.

While it may sound odd to our 21st century ears to suggest that demonstration could be a matter of degree, for Descartes, who is attempting to found an altogether new science, appeal to such criteria as "coherence" was an important first step to jettisoning Aristotle's metaphysics and physics. He believed, moreover, that a new physics required a new metaphysics; since, for him, a metaphysical foundation was prior to, and foundational for, the new physics. Descartes wished for his new theory to be persuasive – what does persuasion require? Descartes explains in a letter to Vatier:

> … indeed it is not always necessary to have *a priori* reasons to convince people of a truth. Thales, or who ever it was who first said that the moon receives its light form the sun, probably gave no proof of it except that the different phases of its light can be easily explained on that assumption. That was enough to ensure that from that time to this his view has been generally accepted without demur. My thoughts are so interconnected that I dare to hope that people will find my principles, once they have become familiar by frequent study and are considered all together, are as well proved by the consequences I derive from them as the borrowed nature of the moon's light is proved by its waxing and waning. (CSM III: 564).

Descartes' appeal to the example of the moon is illuminating, and not only because the pun is so tempting. Descartes is appealing to two " rationalist" virtues here; first, he speaks of his thoughts being "interconnected" and of his principles being "considered all together"; second, he speaks of these very principles being "proved by the consequences I derive from them." In other words, he seems to suggest here that the "proving" his principles is not a matter of simple deduction of the phenomena from them, more geometrico, but (at least in part), showing how they are all systematically connected and that they have enormous explanatory power. Or, proof (in a far broader sense than the syllogistic one) may run in the opposite direction of a Euclidian one – the effects may prove the causes. The borrowed nature of the moon's light is "proven" by its characteristic patterns of waxing and waning.

In short, Descartes has stumbled upon the problem of underdetermination. "Descartes' ladder" is this: he scaled the heights of generality, arriving at the simplest possible first principles of physics. Then, he descended to the physical world, and found that he often had to resort of hypothetical explanations. When challenged by his contemporaries, he found that he needed to bridge the gap between the explanations he wished to offer at the level of the "visible world" and the first principles of his physics. How did he know that this particular explanation was preferable to others? How might we bridge such a gap? Descartes offered a variety of criteria – coherence, consistency, simplicity, and explanatory power. As in the case of Thales, Descartes asks his readers to consider what "further observations" one's hypothesis entails – i.e., imagine other plausible consequences of one's hypothesis, and check whether these consequences are borne out by experience. If they 'explain' systematically, so much the better.

Further, he claims that no alternative hypothesis could explain the same phenomena as parsimoniously:

> Finally you say that nothing is easier than to fit a cause to an effect. It is true that there are many effects to which it is easy to fit many separate causes; but it is not always so easy to fit a single cause to so many different effects, unless it is the true cause which produces them. There are often cases in which in order to prove what is the true cause of a number of effects, it is sufficient to give a single one from which they can all clearly be deduced. I claim that all the causes of which I spoke belong to this class… Compare my assumptions with the assumptions of others… Compare all their real qualities, their substantial forms, their elements and countless other such things with my single assumption that all bodies are composed of parts. This is something which is visible to the naked eye in many cases and can be proved by countless reasons in others. All that I add to this is that the parts of certain kinds of body are one shape rather than another… Compare the deductions I have made from my assumption - about vision, salt, winds, clouds, snow, thunder, the rainbow, and so on – with what others have derived from their assumptions on the same topics. I hope this will be enough to convince anyone unbiased that the effects which I explain have no other causes than the ones from which I have deduced them. None the less, I intend to give a demonstration of it in another place. (CSM III: 199-200)

Though one may whimsically assign causes to effects, if there is one cause that explains many effects, Descartes claims, it is more likely the true cause. In this exchange, Descartes invites Morin to compare his assumptions with those of others, proceeding to list the scholastic notions of real qualities and substantial forms. He indicates that his assumptions are more parsimonious – in effect, only that all bodies are composed of parts. Finally, he gives a promise (unfulfilled) that he will be able to give a "demonstration" elsewhere, in his *Principles of Philosophy*.

Yet, here too, there is a gap between the project of the first two parts of the *Principles*, which cover the most basic metaphysical and epistemological questions, and the latter, incomplete parts, which are concerned with particular effects – the visual world. These latter parts often involved "hypothetical" demonstrations. At the end of the *Principles*, Descartes defended the use of idealized and hypothetical suppositions. Descartes remarks, again, in the *Principles*, "In fact, it makes very little difference what initial suppositions are made…" (CSM I: 103) And, "We are free to make any assumption on these matters with the sole proviso that all the consequences of our assumptions must agree with experience." (CSM I: 101) The problem, which Descartes acknowledged, is that many general theories were possible; "experience" can be made to agree with any number of suppositions. There was, in short a gap between his first principles and the concrete applications. Here, Descartes attempts to offer a reply to this problem:

> … if people look at all the many properties relating to magnetism, fire, and the fabric of the entire world, which I have deduced in this book from just a few principles, then, even if they think that my assumption of these principles was arbitrary and groundless, they will still perhaps acknowledge that it would hardly have been possible for so many items to fit into a coherent pattern if the original principles had been false. (CSM I: 205)

Descartes confesses that the best argument available to him is an inference to the best explanation. It is implausible, he claims, that his assumptions were groundless. For, consider all the phenomena that he was able to explain on these assumptions! His explanation was comprehensive, systematic, and unifying. Not only is his view, in contrast to Aristotle's and the Aristotelians, simpler, it is, he claims: a better explanation of the phenomena. Why? He makes a few simple assumptions, such assumptions are consistent with observation. Moreover, these assumptions are proved by "countless reasons," not least of all, they fit the observations; they fit a "coherent pattern." His theory has the virtues of parsimony, unifying power, AND empirical adequacy.

Part II: Principles of Rationalist Science

The aim of this essay has been two fold. First, I've reviewed (if briefly) some of Descartes' "rationalist" methods. Second, I wish to show how Descartes anticipates modern science in important ways. I'd like to expand on this second goal further here, by giving examples of what I would call a "rationalist" dimension to the sciences – one all too frequently overlooked. How is a devout rationalist in any way like a "modern" scientist?

Modern scientists do not consult their intuition to discover a priori truths about the natural world. Moreover, unlike Descartes, who seemed to have deep reservoirs of faith in his abilities to get things exactly right, most modern scientists are fallibilists.[9] Nonetheless, I believe a good case can be made that modern scientists do deploy "Descartes' ladder" to good effect in attempting to bridge the gap between what are often highly general and idealized models and claims about particular effects in nature. It's well known that a good part of modern science consists in the development of mathematical models, and the generation of arguments founded on these models. In many of these cases, arguments have the form of, "If we assume X, Y, and Z," where these are idealized assumptions about the world, "then, this will follow." These arguments involve treating the world as an idealized

[9] Thank you to David Stump for pointing this out.

system with properties, which we (often) know not to hold (at least not exactly). And, many of the arguments in these cases are demonstrations of what 'must' be the case (at least if you accept such assumptions). Such arguments are, I would argue, often necessary for new theories in the sciences to even begin to lift off the ground. They serve, both, as "tests" of our "commonsense" assumptions, suggesting alternatives that we may not have considered, perhaps because we are entrenched in a theoretical framework. Moreover, such model building and associated thought experiment often functions as a way of rethinking the "metaphysical foundations" of our science. Such models *frame the space of possibilities*; without knowing what is "possible", even in admittedly "idealized" systems that do not directly describe the world as it is, we cannot begin to frame some of questions we might seek to test. The use of "what if" here is not "merely" precursor or heuristic; it is, I would part of the process of scientific argumentation, and scientific innovation – if you like, "first philosophy" in the sense of challenging our most basic assumptions. Some of the most important theoretical innovations in the biological and physical sciences occurred through the process of generating such idealized models or thought experiments.

It's what happens next that most philosophers of science seem to focus their exclusive attention upon; novel empirical discoveries (which can, by the way, occur only once we know what we "should" be looking for, once we have the theoretical framework to make sense of them) confirm our theories. Yet, this process of confirmation is rarely so spontaneous as the newspaper headlines or textbooks would have us believe. As most "post-Kuhnian" philosophers of science know, choice among competing hypotheses is often (if not always) a process of negotiation. The new theory or hypothesis needs to be shown to be superior.

Here again, I would suggest, methods of argumentation analogous to those of Descartes play a significant role in modern science. Rationalist science, I would argue, involves several features. First, doubt of what are, at the time, taken to be "common sense" observations – accepted, empirically informed views of the way the world is – can be a useful first step toward a new theoretical paradigm. Second, offering up a "theoretical demonstration" or thought experiment – i.e., what "must" be true – though, only under certain assumptions. Third, and finally, a demonstration that such a theory better explains a wider range of phenomena, is simpler, and/or is more coherent with other well-confirmed theories or empirical observations. An example is Mendel – at the time Mendel did his famous experiments, most assumed that heredity was blending – i.e., that the hereditary material was not "discrete," but was, instead, rather like the combining of ingredients in a soup, or dye in a vat. The offspring of a tall parent and a short parent would thus be of average height. This was simply "common sense"; hundreds of observations confirmed it. Indeed, most hereditary traits are quantitative – height and weight vary not in kind, but by degree, and offspring generally do tend to regress to the mean of their parents'. So, Mendel, in idealizing heredity as a matter of discrete particulates, was countering common sense observation. This was necessary to generate his laws of segregation and independent assortment. Though we now know that heredity is far more complex than Mendel's simple picture suggested, Mendel's step was essential to subsequent developments of both genetics, and evolutionary theory. His use of examples of "rare" cases to prove the rule was an ingenious move; his use of qualitative traits – traits that vary discretely – made possible a quantitative theory of heredity. Both his challenge to "common sense" observation, and his deployment of an entirely new metaphysics of heredity are common "rationalist" moves.

"Demonstration" in the broader sense of showing what one might explain both new and old phenomena, or unify what are viewed as competing paradigms, under a new theoretical framework is a common rationalist strategy. Consider, for instance, Fisher's demonstration of the consistency of Biometric patterns of inheritance with Mendelian theory of heredity. In order to show that these two theories were consistent, he had to idealize away from features of each theory – he assumed, for instance, that there is no dominance, and that single phenotypic traits are caused by many hereditary particles of equal effect. In Fisher's case, as in Mendel's, what was important was the demonstration of what is "possible," even on assumptions known (or believed) to be false. The (idealized) Mendelian assumption turned out to be explanatory (in Descartes' sense of being capable of deriving the phenomena from it – the light of the observations reflected back on the theory), and this explanatory power made possible the adoption of a theoretical framework, which (however limited) unified and advanced genetics and evolutionary biology for the next fifty years. Whatever we now think of the limitations of the "Fisherian" model of inheritance and evolution, the theoretical framework needed a "foothold." I.e., what was important in Fisher's demonstration was that he proved that, at least on some assumptions, Mendelism and Darwinism were (at least) consistent with one another.

Some have argued that appeal to "rationalist" considerations – e.g., unifying power, and simplicity – runs counter to good science. Since false theories may be unifying, and simple theories may be false, some argue that such virtues are either at variance with good science (van Fraassen, 1980), or, alternatively, reduce to or are supported by empiricist warrant – e.g., predictive accuracy. For instance, in a recent paper (2003), Elliot Sober explains how unifying power may be relevant to predictive accuracy, in some contexts. We ought to prefer models that are more unifying (or, more precisely, models with fewer adjustable parameters), Sober argues, because they are more likely to be predictively accurate. Sober deploys Akaike's theorem as a useful way of solving what he calls "Peirce's problem." Others contend that "rationalist" virtues like unification might be recaptured in

Bayesian terms (McGrew, 2003; Myrvold, 2003; Schupbach, 2007). The problem is a very old one; how do we move from the observations to an abstract theory? Peirce was concerned with the rules that govern inference to the best explanation. Pierce's question was, in a way, not unlike Descartes' question viz. Scholastic metaphysics: Which of two competing theories would lead to a total system of beliefs with the most explanatory power? Why ought we to choose, of two competing explanations, the one that better unifies the phenomena?

For Sober, the Peircian problem is just the problem of model-selection in a different guise. The problem of model-selection is familiar from the case of curve fitting. Scientists prefer a model that is simpler, or one that is described by an equation with fewer parameters rather than more. Why? According to Sober and Forster (1994), the model with fewer parameters is more likely to be predictively accurate. Sober and Forster deployed Akaike's theorem in the context of the curve-fitting problem, and they argue that the same theorem explains why we ought to prefer a theory that is more unified.[10]

Sober asks us to imagine that we seek to explain the effects of smoking on rates of cancer. He describes two models, one "unified" and one "disunified". The first model, "U," simply ascribes the rates of cancer to tobacco use, regardless of the brand of cigarette. The second model, "D," treats the effects of smoking two different brands of cigarettes as distinct. Sober argues that it's inevitable that the second model will fit the data better, but contends that "U" is nonetheless preferable. According to Akaike's theorem, an unbiased estimate of a model M's predictive accuracy is approximately equal to the log-likelihood [L(M)]-K. Or, the log of the likelihood of the fitted model (a model whose parameters are

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[10] Though, see Kukla, 1995, for a reply.

estimated by maximum likelihood from some data set), minus the number of adjustable parameters, is a good estimate of the predictive accuracy of the model. In other words, a model with fewer adjustable parameters – the "unified" model on Sober's reading – will be more predictively accurate, and so preferable, in the longer run. So, according to Sober, "If you want to find out which of several models will be predictively accurate, then it is an objective fact that unification is relevant." (p. 214)

Likewise, Myrvold and McGrew both give a Bayesian re-formulation of unification as a virtue; essentially, their argument is that appeals to "unifying power" can be reduced to empiricist virtues. By deploying Bayes theorem, they show that the greater positive relevance of facts to one another, or, the higher probability of all the facts combined on a unifying as opposed to the disunified hypothesis, ceteris paribus, lends greater posterior probability to the unifying hypothesis.

Sober essentially assimilates the problem of choosing between more or less unified theories to the problem of model selection. Likewise, Myrvold and McGrew reduce the value of unification to Bayes rule and empiricist virtues. Both of these strategies are certainly brilliant solutions to the problem of valuing unification for the empiricist. However, I wish to suggest that the search for empiricist justification of unifying power is rather post hoc, at least from the perspective of the history of science. When we have two theories, and the evidence does not (at least not yet) allow us to choose, what ought we to do? Does "unifying power" give us warrant to prefer one to the other? Sober, McGrew and Myrvold answer yes, but only BECAUSE the unified theory will be (under certain conditions) more empirically accurate, ceteris paribus. But, unifying power and predictive accuracy surely do occasionally pull apart, and it's not always clear that when they do, we should prefer the latter to the former (see., e.g., Cartwright, 1983). Sometimes, I would suggest, having a general explanatory theory, especially in early stages of a science, is preferable to one that is predictively accurate. That is, having a theory that either unifies theoretical frameworks otherwise viewed as inconsistent, or a theory that unifies diverse phenomena under a new metaphysical framework moves science forward in important, novel ways. While I cannot claim this as a general rule, it seems that there are enough cases of this in the history of science to bear consideration.

For instance, classical population genetic theory unified a diversity of phenomena under a common set of mathematical models. But, classical population genetics is infamously bad at prediction. The kinds of generalizations that it generates are useful. They describe what's possible and what's necessary in evolving populations, given certain assumptions. They are, in short, rather like a very abstract calculation device, for imagining what evolution would be like in populations where genes determine traits, among other (idealized) assumptions. One may derive, for instance, broad claims about the relative significance of drift v. selection, say, in populations with x population size and y selection coefficients. But, such predictions and models are highly idealized and general; it's quite difficult to "apply" them – yet, the theory served a central explanatory role in the history of evolutionary biology. They provided a new way of "seeing" populations – as changing over time due to a set of "forces" much like Newton's physics. The role of population genetics is to provide a dynamic way of representing populations over time, at a time when very little was understood about the relationship between genes and traits, much less what genes were. Given all the complexity and difficulty of representing the many features potentially relevant to evolutionary change – heredity, ecology, chance, phenotypic adaptation, etc. – a simplified model provided a way unify complex, diverse populations under a single explanatory framework. Even if today, the "extended" evolutionary synthesis will require a broader set of tools to explain the evolution and diversity of life, this framework was, I would suggest, a simple, but unifying and necessary first step toward seeing evolution as a process that could be explained using quantitative models and general causes (causes that operate in all populations – selection, mutation, etc.).

Arguably, Descartes made a similar move in his physics of size, shape and motion. Though his physics failed, it failed spectacularly; seeing all of the physical world as explainable in terms of matter in motion made way for Newton's physics.

Finally, contra Sober, it's not clear that every case of what we consider a more unified theory involves choice of a model with fewer rather than greater parameters. For instance, at the turn of the century, biologists were seeking single factor explanations of evolution (e.g. neo-Lamarckism, mutationism, etc.). What the early synthesis of Biometry and Mendelism and the development of theoretical population genetics made possible, however, was the realization that no single factor need predominate in evolution. Biometers believed that evolution was governed primarily by gradual selection on minor variations; Mendelians believed that evolution was a product of major mutations. The models of theoretical population genetics take no single factor to be primary in generating evolutionary change. Rather, the models integrate the different factors of selection, mutation, migration, and drift in a common mathematical framework. Population genetics is thus in a sense unifying, but not because it requires fewer parameters than the alternatives then available. Rather than take one factor (mutation, selection) to be the exclusive cause of evolution, several were taken to work in concert (though some were considered of greater or lesser significance). The virtue that could be claimed by theoretical population genetics is that it "unified" a variety of processes under a single theoretical framework, however idealized, and was more general as a quantitative description of evolution in populations than any "single factor" alternative.

This dialectical process of reasoning – moving between one's own model, the going alternatives, and the world – and showing that one's own preferred hypothesis explains "more", and "more simply" – is often what takes place exactly during those times of "negotiation" when it is not clear what the "evidence shows." Such negotiation is, I would argue, not "bad science"; all too often, the data is consistent with a variety of hypotheses, and thus the goal is to show how a framework both is consistent with observations, but also (a) coherent, or logically consistent, (b) adopts simpler assumptions – i.e., assumptions that while not strictly true, are at least, in Descartes' words, not "obviously contrary to experience," (b) is more comprehensive. The process of exploring this, and the "rationalist" arguments appealed to in this context, often help "unpack" as it were, the assumptions of alternative theoretical frameworks, and show how they may be displaced, if not once and for all, at least in theory. This unpacking is part of the process of the advance of science.

Recall the three elements of Descartes' view outlined above. First, he argues that direct observation should be questioned and corrected by rational reflection. Moreover, he wished to replace Scholastic metaphysics and provide a new metaphysical foundation for physics. Second, Descartes offered an simple, "streamlined" alternative to his competitor; and, he often asked his readers to imagine scenarios which were false. In the end of the *Principles*, Descartes requests of his readers to consider the possibility that his results, "will be allowed into the class of absolute certainties,"(CSM I: 327-328), though he makes a much better case for a weaker standard, "moral certainty" on the basis of arguments from simplicity and explanatory power. For, all he can say is that his mechanistic assumptions seem to explain a great deal.

Descartes' *Meditations* is frequently taught as the statement of a theory of knowledge fundamentally at odds with Hume's *Enquiry* or Locke's *Essay*. Alas, Descartes is often made out to be the (rather naïve) loser in this conversation. However, Descartes was far from naïve. It is true that Descartes believed that there was a rational faculty by which we could intuit clear and distinct ideas about the nature of God, the truths of mathematics, and the truth of a substantial distinction between mind and body. The Cartesian circle is, indeed circular. My aim is not to challenge this. Rather, I seek to identify what, of Descartes' actual scientific argumentation is retained in much of science today, and might reasonably be called "rationalist" science. Descartes did anticipate modern science, but in ways that are not usually acknowledged by us moderns; he was modern in several ways, not only in rejecting Scholastic physics and metaphysics, and not only in his "mechanistic" physics, but also in advancing a set of tools for rationalist science.

What is rationalist science? It is not (or, not exclusively) to be committed to a set of foundational, metaphysical truths, said to be known via reason alone with absolute certainty. Descartes went very far beyond simply asserting that bodies are made up of parts with size, shape and motion. What is much more interesting is how he deployed these assumptions in offering explanations of motion of bodies. Rationalist science, as Descartes actually practiced it (instead of what he avowed) involves questioning appearances, rethinking metaphysical foundations, using either thought experiment or idealized assumptions, deriving "necessary" conclusions under such idealized assumptions, and comparing one's model with alternatives, to see whether it can explain more, with fewer (suspect) assumptions. Of course, today, such a set of strategies will be familiar to empiricists and rationalists alike. My suggestion here has been that attempting to reduce such a process to seeking empirical adequacy loses a great deal of the texture of the history of science, in a way that philosophers should be wary of. Reducing such strategies to empiricist virtues, while perhaps a promising solution to the "problem" of the value of unification, is post hoc. While we may, in some cases, reduce or explain away the value of “unified” theories, unification is a many splendored thing.[11] “Rationalist” methods such as those discussed above have played an important role in the history of science, to the extent that it’s difficult, today, to say which count as “rationalist” and what as “empiricist.” Rationalists like Descartes were often wrong, but fruitfully so; there is value to “systematic” and “foundational” rethinking in science, so perhaps naturalists need not give up “first philosophy” in the broadest possible sense.

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[11] Margaret Morrison in *Unifying Scientific Theories*, (2000) devotes chapter 1 to discussing “the many faces of unity.”

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