Synthese (2017) 194:3327–3346
DOI 10.1007/s11229-016-1015-2

S.I.: NEW THINKING ABOUT SCIENTIFIC REALISM

A pragmatic, existentialist approach to the scientific realism debate

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Received: 31 July 2015 / Accepted: 5 January 2016 / Published online: 12 February 2016

Abstract It has become apparent that the debate between scientific realists and constructive empiricists has come to a stalemate. Neither view can reasonably claim to be the most rational philosophy of science, exclusively capable of making sense of all scientific activities. On one prominent analysis of the situation, whether we accept a realist or an anti-realist account of science actually seems to depend on which values we antecedently accept, rather than our commitment to "rationality" per se. Accordingly, several philosophers have attempted to argue in favour of scientific realism or constructive empiricism by showing that one set of values is exclusively best, for anyone and everyone, and that the downstream choice of the philosophy of science which best serves those values is therefore best, for anyone and everyone. These efforts, however, seem to have failed. In response, I suggest that philosophers of science should suspend the effort to determine which philosophy of science is best for everyone, and instead begin investigating which philosophy of science is best for specific (groups of) people, with specific values, in specific contexts. I illustrate how this might be done by briefly sketching a single case study from the history of science, which seems to show that different philosophies of science are better at motivating different forms of scientific practice.

Keywords Scientific realism · Constructive empiricism · Scientific practice · Philosophy of science · Voluntarism · Epistemology · Epistemic stances

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1 The scientific realism debate as a stalemated debate

After 35 years of debate between scientific realists and constructive empiricists, philosophers of science seem to have reached an impasse. Scientific realists, characteristically, understand science as an effort to discover truths about the unobservable nature of reality that are able to explain various phenomena of interest, e.g. by developing theories that are (approximately) true descriptions of both the observable and unobservable features of reality. Constructive empiricists, by contrast and in opposition, understand science merely as an effort to "save the phenomena" by constructing empirically adequate theories, i.e. theories that are true with respect to the observable features of reality, but not necessarily the unobservable features. From a constructive empiricist perspective, understanding scientists as searching for unobservable truth is supererogatory in the effort to develop a viable philosophy of science—we can make sense of scientific practice, as a whole, without understanding the quest for unobservable truth as part of its aim, its telos, or its end-in-view. And while scientific realists have tried, in a wide variety of ways, to demonstrate the objective superiority of their position in some manner, it seems increasingly certain that they will be unable to achieve that goal. After decades of debate it would seem that the most we can say is that there are versions of scientific realism that are coherent, plausible, and reasonable philosophies of science; but at the same time, it would seem we can say the same for constructive empiricism, the most prominent brand of anti-realist empiricism currently on offer.

Thus, when introducing a volume on the debate over scientific realism and Bas van Fraassen's constructive empiricism, Bradley Monton noted that "there is a growing consensus that van Fraassen has argued to a stalemate against the scientific realists. Scientific realists cannot conclusively show that belief in the literal truth of scientific theories is epistemically warranted, but constructive empiricists cannot conclusively show that the aim of science is limited in the way they describe" (2007, p. 3). A stalemate of this sort tends to sit uncomfortably within philosophical circles, as philosophers are generally in the business of determining the superiority of one position over another by way of argument. But the generally accepted analysis of the situation suggests that no *non-circular* argument will ever be able to demonstrate the unqualified superiority or rational preferability of either scientific realism or constructive empiricism as a philosophy of science.[1] As a result, the developing consensus is that this stalemate is more or less unavoidable, and that the dispute between scientific realists and anti-realist empiricists is accordingly "irreconcilable" according to the usual standards of philosophical debate.

One way of understanding this stalemate is through an analysis of the conflict developed by van Fraassen (1991, 2000, 2002; see Chakravartty 2004 for the most systematic account). On this understanding, varieties of scientific realism and anti-realist empiricism are seen as the outgrowth of opposed "epistemic stances". An epistemic stance sits, as it were, at a "meta-epistemological" level in this analysis, above the level of epistemological theories and philosophies of science, which in turn

[1] For more detail on this point, see (Chakravartty 2007a, 2011, as well as Fine 1986a and Kukla 1998, pp. 27–42).

sit above the level of more concrete, ground level facts (about our world's actual ontology and laws of nature, for example). Each level serves as a guide for developing beliefs and claims about things at the lower level, such that epistemic stances inform our development of theories and epistemologies of science, and these in turn inform our ontological beliefs on the basis of contemporary science. Importantly, adopting an epistemic stance is not understood as making a factual claim; rather, it is akin to adopting, on the basis of one's own system of values, a set of policies regarding the generation of epistemological theses. Taking an epistemic stance that encourages the search for explanations of the observable in terms of an underlying, unobservable reality leads one to develop some form of scientific realism as their epistemological theory of science; it is for people who value explanations, even if the search for them increases our chance of forming false beliefs. Taking an epistemic stance that proscribes the search for such explanations as too epistemically risky, by contrast, leads one to develop some form of anti-realist empiricism as their epistemological theory of science; it is for people who value the minimization of epistemic risk over the satisfaction of any desire to explain the various phenomena we observe around us.[2] Following existing convention (e.g. in Chakravartty 2004, 2007a; van Fraassen 2002), let us call the pro-explanation stance, which leads one to develop and adopt some form of scientific realism (such as Chakravartty's "semirealism") "speculative metaphysics", and the anti-explanation/pro-minimization of epistemic risk stance, which leads one to develop and adopt some form of anti-realist empiricism (such as constructive empiricism) "empiricism".[3]

This analysis of the scientific realism debate is understood as a form of *voluntarism* at the meta-epistemological level because our commitment to being rational does not, ultimately, determine whether we accept and work within the framework of empiricism or speculative metaphysics. Because epistemic stances themselves inform our more fine-grained conceptions of rationality—for instance, whether it is rational to make abductive or inductive inferences—there is very little common ground at the meta-epistemological level over what is required to be "rational". Accordingly, only a very minimalistic conception of rationality can constrain our choice of stance, where "nothing is needed above and beyond coherence" (van Fraassen 2000, p. 277) for a stance to be deemed rational. Here coherence is understood as not only logical consistency and conceptual agreement, but also pragmatically, as not leading to "self-sabotage by one's own lights" (van Fraassen 2005, p. 184; cf. Chakravartty 2007a, pp. 21–25). But so long as an epistemic stance meets that minimal constraint—as both speculative metaphysics and empiricism seem to do—what determines our choice between an array of options

[2] "Epistemic risk" is here understood as the chance of developing false beliefs without any potential practical benefit, in line with the discussion in van Fraassen (2002), Ch. 3.

[3] Chakravartty (2007b, 2011) understands the relation between theories of science and epistemic stances a bit differently than I have here, seeing scientific realism and constructive empiricism as themselves epistemic stances (2011, p. 39). I think it's important to recognize this as a simplification of the analysis, and potentially a pernicious one, for reasons that are not especially relevant to the topic at hand. In either case, his views in (2004, 2007a) are closer to the above analysis.

is not a commitment to "rationality", but our commitment to certain values such as "explanatory power" or "minimal epistemic risk". Thus, rationality underdetermines our choice at the meta-epistemological level of stance selection, leaving those of us committed to being rational with "epistemic options", as Peter Lipton put it (2004, p. 149).

In this way, the stalemate referred to above can be understood to result from the fact that scientific realism and anti-realist empiricism issue from opposed but *equally rational* epistemic stances. van Fraassen (2002) suggests that many other philosophical disagreements might be understood as resulting from opposed epistemic stances—for example, between a pragmatic stance and a skeptical stance—but he and other commentators have narrowly focused their discussion on the opposition between empiricism and speculative metaphysics so as to better understand the contemporary scientific realism debate between various forms of anti-realist empiricism and scientific realism. Within such a meta-epistemologically voluntaristic analysis, the debate between scientific realists and anti-realist empiricists reduces to a debate over which epistemic stance one should adopt, which in turn seems to reduce to a disagreement over which values should inform one's choice of stance.

The scientific realism debate can be understood to have reached a stalemate, then, for two reasons. First, philosophers will characteristically, for the most part, try to resolve conflicts between two opposed positions by arguing that one of the views is the most "warranted", uniquely "rational", or likely to be "true".[4] But if rationality is not determinate with respect to the epistemic stances informing our choice of scientific realism and anti-realist empiricism—as the developing consensus seems to indicate—it's unclear what philosophers can do, if anything, to resolve the dispute. For secondly, value selection—which is the determinate factor in downstream stance selection—seems to be, on this voluntaristic analysis, an unanalyzable act of will. This means that if both positions are deemed equally (i.e. entirely) rational, then everyone simply chooses whichever epistemic stance best suits their idiosyncratic, agent-relative set of freely chosen *values*. If one values having explanations, one adopts speculative metaphysics, and goes on to develop some form of scientific realism as a philosophy of science; if one instead values the minimization of epistemic risk, one adopts empiricism, and goes on to develop some form of anti-realism as a philosophy of science. Van Fraassen chooses the latter, and his opponents choose the former, but neither can condemn the other as irrational so long as they maintain success "by their own lights", i.e. their choice of epistemic stance serves their values. Personal choice about which values to adopt, on this understanding, is a central and ineliminable aspect of our epistemic lives, and as a result the philosophical dispute over scientific realism seems irresolvable.

[4] Consider, for example, the opening question of Bertrand Russell's *Problems of Philosophy* (1912), one of the most classic and basic works of modern philosophy: "Is there any knowledge in the world which is so certain that no reasonable man could doubt it?" (p. 2) Russell goes on to discuss our ordinary beliefs about the external world, our usual probabilistic inferences, and several other traditional philosophical issues, aiming to determine what position the "reasonable man" he refers to at the outset should take on these issues. The implicitly central task of philosophy, in this work as in so many others, is to determine what position is mandated by a commitment to rationality alone.

2 Breaking the stalemate: choosing for everyone versus choosing for individuals

Some philosophers have attempted to support scientific realism or anti-realist empiricism as the universally preferable philosophy of science by attacking the bedrock, arguing that some specific values should always be informing anyone and everyone's stance choice (e.g. Cartwright 2007; Chakravartty 2011). The idea here is that we can say something edifying—that is, helpful and action-guiding—about which epistemic stances we should choose by showing that everyone, if they think clearly enough about it, would reasonably choose one set of values over the other, and that those values would in turn determine one stance over the other, meaning that all properly rational individuals should develop one philosophy of science and not the other.

It is certainly true that we can change someone's choice of stance—whether that person is ourselves, an interlocutor, or a reader—by showing that they have good reasons to accept a specific set of values. Unfortunately, to date there has been no successful argument showing that one specific set of values is universally better than its alternatives. Chakravartty (2011), for instance, tries to say something edifying about how we might decide between equally rational epistemic stances by looking at value selection in a very general way: we might ground value choice in facts about the human condition, in our shared emotional phenomenology, or in our common social circumstance, for instance. Ultimately, however, all these attempts are (rightly) seen by him to fail: it would seem that most people are relatively free to choose whether to value explanation more than the minimization of epistemic risk, or vice versa. Furthermore, their choice seems to be a philosophically unanalyzable act of will, and as a result there is little that argument, inquiry, or philosophical analysis can establish regarding universal constraints placed on every rational person's choice of such values. Accordingly, there seems to be little that argument, inquiry, or philosophical analysis can do to change people's downstream choice of rational epistemic stance, and thereby their downstream adoption of scientific realism or anti-realist empiricism. Hence the stalemate. By placing the human will at the centre of our meta-epistemological analysis, it would seem, we are left with the conclusion that whether we end up as some kind of scientific realist or some kind of anti-realist empiricist comes down to a matter of value selection, a process that philosophical inquiry has (so far) nothing especially informative to say.

This is not to say, Chakravartty writes, that "one cannot change one's mind, for values can change" (ibid., p. 42); it is simply to say that, ultimately, no edifying account of when, how, or why one might change their mind (or change someone else's mind) about which values to adopt has been given. He concludes:

> Regarding rational epistemic stances, when it comes to understanding the ultimate wellsprings of voluntaristic choice, is there nothing more we can say, or do? That question, and the inspiring, infuriating, enticing yet stultifying mystery of the will, is the puzzle at the heart of stance voluntarism (ibid., p. 47).

I argue that we can say more. Specifically, I argue that with some effort we can uncover information that will help guide certain people's choice of epistemic stance on the basis of their idiosyncratic values. Furthermore, I argue, we can do so without solving all

the mysteries of the will, or even engaging with the issue of value selection at all. That is to say, I think there are ways we can help people make more well-informed choices about epistemic stances, and that the best way to do this is by resisting the urge to determine some specific epistemic stance as *universally* best for everyone. Instead, we should strive to look at agent- and context-relative questions about which epistemic stance is best for some individual, whether actual or hypothetical, while taking their specific set of values as given.

In the next two sections I sketch out a general methodology for investigating such questions, constituted by two assumptions and a model of choice. These assumptions and this model can be justified methodologically, i.e. by the success of the studies they aim to support. If successful, however, such studies will not resolve the traditional debate over scientific realism. That is to say, they will not identify one epistemic stance, philosophy of science, or set of values as rationally preferable for everyone, in all contexts. Rather, they will identify one epistemic stance as rationally preferable for specific types of people, given their specific values and contexts, by uncovering non-obvious associations between taking a certain stance and achieving success in a certain kind of practical activity. In many practical contexts, I argue, it is not obvious which epistemic stance will best support our aims, and determining which stance is in fact best will require some investigation. That is to say, not only does rationality underdetermine our choice of stance, but in many cases our commitment to rationality and our choice values together underdetermine our choice of stance, for it is not always immediately obvious which stance best serves our values. Specifically, I argue that there are many empirical facts that, when properly uncovered and considered, can *pragmatically* determine the preferability of one epistemic stance over its alternatives, relative to a specific set of values (both epistemic and non-epistemic) and a specific context. For philosophers of science interested in the scientific realism debate, determining those empirical facts is likely a worthwhile endeavour, for determining which stance best serves a given set of values in a given context can help each of us determine which stance is best for each of us, given our individual values and contexts. And that, I think, will prove to be not only valuable information, but ipso facto justification for the methodological assumptions I suggest below.

There are a wide range of contexts and value sets, at varying levels of generality, against which we might attempt to evaluate the differential preferability of opposed epistemic stances. I might try to evaluate the respective instrumentality of speculative metaphysics and empiricism for myself, in a given moment, according to my own values, or perhaps for a roommate, spouse, or colleague, according to their values and context. Those would be amongst the lowest level of generality, that of the truly particular. At higher levels of generality we might begin to precisely specify an ordered set of values, and a specific context, and see if we can come to a determinate answer. But the most promising approach, I submit, would be to address *role-specific* sets of values, relative to role-specific contexts and agendas. We can ask and attempt to answer, for instance, questions about which epistemic stance best serves the role-specific values of the working scientist, the science policy-maker, the historian of science, the science educator, or the science critic.

As an example of the kind of study I'm suggesting, I conclude with a brief historical investigation of the way different philosophical conceptions of science influenced

electrodynamics research in late 19th century Europe. This case study suggests, as a kind of working hypothesis to be confirmed or disconfirmed by further historical investigations, that different forms of scientific practice are better motivated and supported by different epistemic stances. If further study demonstrates the historical robustness of these motivational and supportive linkages between epistemic stances and certain forms of scientific practice, at different periods but in similar contexts, I suggest we will have achieved our goal: giving a pragmatic justification, on empirical grounds, for adopting some epistemic stance in certain practical contexts, given certain values. That is to say, we'll have shown some scientists which epistemic stance is most likely to support the success of their specific type of research. Surely that would be valuable information to have.

To be clear, it is beyond the scope of this paper to conclusively establish the historical robustness of the connections identified in the final section. Rather, I aim only to motivate such investigations, provide a methodological framework for conducting them, and then provide a compelling example of one way we begin to determine a single epistemic stance as best for some practical context and set of values. Having sufficiently established the motivation for such studies, I now turn to matters of general methodology.

3 Making informed choices on the basis of groundless values: a pragmatic, existentialist approach

As a first step towards determining which epistemic stance is best for a specific (type of) person, given their specific context and set of values, I recommend, on methodological grounds, that we simply abandon the idea that values can be grounded or motivated in anything other than human choice and action. This accords with an understanding of values that I generally associate with existentialism, so I will refer to this as an "existentialist approach to the question of value selection", or more briefly "the existentialist assumption". I recognize that the existentialist label will be judged inappropriate by many, for reasons that need not be gone into here, but I can think of nothing better to signify the assumption.[5] For as I take it, the central contention of existentialist thought is that the values we hold and the meanings we supposedly "find in the world" arise out of our own choices and practices, rather than being determined for us by some external source such as God, Rationality, or an immutable human condition.

Philosophically speaking, an existentialist approach to the question of value selection means accepting that we are, ourselves, ultimately responsible for the values we adopt, and that we cannot defer the choice to some other agent. But this existentialist assumption is here understood only methodologically, and methodologically speak-

[5] It should be noted that the idea of working through the scientific realism debate on pragmatic and existentialist considerations was originally put forward, in those terms, by van Fraassen (2000, p. 273, 2002). He notes that, by taking a meta-epistemologically voluntarist perspective on the debate, "[t]he element of personal decision, values, and volition has entered and received a legitimate place in our epistemic life" while noting that "[by] itself, however, this element is no cure-all" (2002, p. 91). His account is more suggestive than the version I present here in terms of explicit, methodologically grounded assumptions for further inquiry into the connections between epistemic stances and success in certain practical activities.

ing, making this assumption simply entails never trying to argue anyone out of their values, or to argue that everyone must (for whatever reason) accept a single set of values. To be clear, I only suggest an existentialist assumption with respect to the question of how values are selected, which does not entail that choosing an epistemic stance on the basis of those values is similarly a matter of pure choice. For even if values are ungrounded and ungroundable in evidence or rationality, our downstream adoption of speculative metaphysics (and thereafter some form of scientific realism) or empiricism (and thereafter some form of anti-realist empiricism) on the basis of those values need not be similarly ungrounded and ungroundable. According to the meta-epistemologically voluntaristic framework we are working within, it is rational to choose the stance which best serves one's values, so as not to be self-defeating by one's own lights; however, I contend, it is not always obvious which stance best serves one's values. Determining which epistemic stance (and resultant philosophy of science) is best for a given set of values is precisely what the existentialist assumption will help us do. It prevents us from arguing over the values themselves and helps us instead focus on more pragmatic questions of the form: "which epistemic stance is preferable for a specific individual, given their specific set of values and a specific context?" The idea is that if we simply accept that the values informing any individual's stance choice cannot be given any ground other than that individual's free choice, and that values will therefore likely and legitimately differ between rational epistemic agents, we can then move on to see whether there is anything, *in addition to values and rationality*, that can ground an individual's epistemic stance choice.[6]

The second methodological step will be to admit as legitimate the influence of non-epistemic values on one's choice of epistemic stance. Most voluntaristic analyses of the scientific realism debate to date (e.g. Chakravartty 2004, 2007a; van Fraassen 2002) have only investigated the ways that different *epistemic* values will impact epistemic stance choice: explanatory power, minimal epistemic risk, etc. But, of course, real people hold many non-epistemic sorts of values as well: wealth, liberty, equality, community, good governance, joy and happiness, technological progress, etc. Cartwright (2007) suggests explicitly that non-epistemic values are important considerations for determining our choice of epistemic stance, but she too focuses on grounding or moti-

[6] Even as a matter of methodology this may be an uncomfortable assumption for any philosopher used to universalizing argument, which strives to determine which position amongst an array of options should be accepted by anyone and everyone, regardless of their individual circumstances. It might even be objected that this assumption ultimately leads to a pernicious and unpalatable form of relativism, for combining an existentialist approach to the question of value selection with a voluntarist minimal epistemic stance selection means nothing less than accepting that different epistemic stances will be rational for different people, or even for the same person in different places and at different times. Such objections are misplaced. Universalizing arguments about the values informing epistemic stance choice have, to date, failed. An existentialist approach to the question of value selection is specifically meant to help us avoid that apparently fruitless mode of argument. Furthermore, justifying certain philosophical assumptions on the basis of their methodological fruitfulness is not without precedence. It is quite common in the history and sociological study of science (see Latour and Woolgar 1979), for example, where it has produced some truly fascinating results (Shapin and Schaffer 1985; Arabatzis 2006, etc.). If the existentialist assumption nevertheless proves too uncomfortable, I invite those interested in the issue of epistemic stance choice to either continue attempts to universally ground values, or to find some suitably weaker assumptions to work with. But I think it is clear that the existentialist assumption is sufficient for the purpose here intended: preventing people from arguing over the acceptability of certain values.

vating stance and value choice in *universally held values*, attempting to motivate an empiricist outlook by appealing to highly general facts about the pragmatic values underwriting everyday life. Chakravartty (2011) has show that her argument does not go through at this level of generality, however, but only holds for anyone who prioritizes a life of practicality over a life of wonder and curiosity. That is to say, appealing to non-epistemic values does not work for Cartwright's argument specifically because she tries to justify certain values universally, yet as a matter of fact people often legitimately choose otherwise, e.g. by valuing a life of wonder and inquiry over the demands of practicality. But Cartwright is right, I think, in allowing that non-epistemic values can properly impinge upon our epistemic stance choice, even if she is wrong in attempting to universally justify a single stance on the basis of the supposedly universal demands of human existence. As long as we don't get caught up trying to argue about which values are "correct" for everyone, the assumption that non-epistemic (i.e. pragmatic) values can properly influence our choice of epistemic stance ensures that we are able to address the circumstances of real individuals, rather than abstracted, de-contextualized "knowers" who only have epistemic considerations in mind at all times.

In short, these two methodological assumptions will bar us from any attempt to determine which epistemic stance or set of values is "correct", "right for everyone", or rationally preferable *per se*; instead, we can focus on investigating which epistemic stance is rationally preferable for some specific (type of) individual, whether hypothetical or actual, without assuming that the same answer will hold for other (types of) individuals. Before illustrating with a specific example the sorts of studies these assumptions are meant to underwrite, I next suggest a simple model for thinking about epistemic stance choice on the basis of personal values, within a meta-epistemologically voluntaristic framework. This model is useful because it illustrates how, even within this framework, rationally choosing an epistemic stance does not entirely reduce to our choice of values, but depends importantly on empirical information about which stance best serves those values.

4 The menu model of stance selection

Overall, what I am suggesting is this: if we seek a firm grounding for our discussion of the conflict between scientific realists and anti-realist empiricists, in the sense of some universal account of the human condition, it should be something like Korsgaard's Sartrean dictum that "human beings are *condemned* to choice and action" (2009, p. 1, emphasis in original). Speaking at the very least from personal experience, this seems to be the best available description of the contemporary situation surrounding the scientific realism debate. When I think about the realism debate in its current stalemated state, I for one—and I'm certain there are others out there—sometimes feel similarly to how I feel ordering off a menu in a restaurant. Do I choose the lobster, or the salad? Speculative metaphysics or empiricism? Whether it's easy to make the choice or not, I generally feel the need to make it; it often appears to me as a "forced" choice, in William James's sense of the term. I cannot simply defer to "rationality" or "my values" when debating realism versus empiricism any more than I can say to the

waiter asking me for my order, "I'm a determinist, I'll just wait and see what happens". My personal context will insist that I make a choice, eventually, whether through the waiter, my hunger, or even the police if I sit there long enough waiting for a choice to be made for me. And as when a waiter comes to take my order in a restaurant, there are practical contexts in which I feel the need to make a choice between realism and empiricism, e.g. in political debates or in philosophical deliberations on other topics. If we accept that whether we end up as a scientific realist or an anti-realist empiricist is in large part a free choice at the level of values, as much as the choice between lobster or salad is, we can get on with the pragmatic issue of making sure our choices are *well-informed*, i.e. that they will actually serve our specific set of values.

Call this the "menu model" of epistemic stance selection. What it shows us is that it is not always clear which selection best serves our values. When it comes to making a well-informed choice of meal, we need to answer a variety of questions to determine which choice best accords with our system of values and priorities at that moment. First we need to determine what our values are, and how they rank against each other: Do I value nutrition over taste? How should I balance cost against my desire for deliciousness, or nutritiousness? Is my craving for cheese more important than my desire to avoid gastrointestinal distress? Are there social values—such as not being seen as "cheap" for skipping the tip, or "poor" for ordering only soup—that are more important to me, in this moment, than the cost? And furthermore, should I eat light and try to get some work done after I eat, or can I gorge myself and then safely slip into a post-prandial digestive slumber? Some of these values will be partially grounded in unalterable but agent-relative facts such as allergies and intolerances, while others may result from religious or social commitments, or similarly free choices. Either way, the purpose of taking an existentialist approach to value selection is to take such values, once determined, as given. For once we have our values determined and prioritized we can focus on answering those empirical questions relevant to determining which menu option best serves those values: What is the macronutrient profile of the various options? Do any of the ingredients in any of these dishes give me indigestion, or allergic reactions, or contravene any dietary restrictions I've put on myself for moral, political, or religious reasons? Which meal is likely to be the most filling? What are the relative costs, how much can I afford to spend, and does anyone want to split an appetizer? These are empirical questions, relevant to my practical context, and even if we already have an ordered and unquestioned set of values answering them will require us to collect non-obvious factual information through additional inquiry (looking at the nutritional information, checking the price, learning our fellow diners' social habits, etc.). And as we will see, there is information that can play a similar role in the process of epistemic stance selection, though it will likely take more effort to uncover than figuring out whether a restaurant's soup is made with vegetable or beef broth.

So let us treat the question of which epistemic stance to adopt similarly to how we treat a dining quandary, and see what comes of it. That's my proposal. Let us look to see which "epistemic option" best satisfies our specific wants, needs, and values, so that we can make the most well-informed choice amongst a given set of rivals. Responsible Diners should seek out information about which meal is most likely to give them the energy they need without giving them indigestion, so they can make better-informed menu choices on that basis. Likewise, responsible scientists,

policy makers, and educators, I argue, should seek out similar information about which epistemic stance is most likely to support their practical activities, so they can make better-informed choices on that basis. And while there is, of course, reason and room to debate the values informing stance choice, for now let's avoid that conversation, just as a polite friend would avoid interjecting to debate the reality of "gluten sensitivity" while a friend is asking the server about gluten free options. While such issues are certainly connected, keeping the discussions separate should help us more clearly and easily determine which epistemic stance is likely to work best for different (types of) people, given their values and contexts.

In the final section I show how we might begin to make such determinations for one group—working scientists—through historical inquiry. Let me stress, however, that the approach to the scientific realism debate I'm prescribing should not limit itself to historical methods. Information regarding which epistemic stance best serves a given set of values can be edifying information for a wide variety of people, and I think it should be pursued using a variety of methods—historical, conceptual, statistical, experimental, or whatever else seems promising. An epistemic stance may be rational so long as it is not self-defeating by the lights of the values informing it, but it is not always clear which stances are likely to be self-defeating by which lights. And while it seems plausible that, with some effort, clarity on these matters can be achieved, it also seems likely that a variety of methods will need to be used.

5 The pragmatic, existentialist approach to the scientific realism debate versus the traditional approach

Attempting to determine which epistemic stance is best for a specific (type of) person, on the basis of the above model of choice and pair of methodological assumptions, is what I call the "pragmatic, existentialist approach" to the scientific realism debate. It is pragmatic because it is not concerned with determining which position is true or most rational per se, but rather with determining which position is best given some antecedently, idiosyncratically, and unquestioningly held set of values, in a given practical context. It is existentialist because it resists any attempt to question those values by denying, as a provisional assumption, that there is any ultimate ground for them, other than our own choices and actions. To be clear, these assumptions are not justified by the promise that we might finally resolve the traditional scientific realism debate, once and for all, according to its universalizing standards, i.e. by somehow showing that one stance, philosophy of science, or set of values is correct, true, uniquely rational, or otherwise preferable for anyone and everyone, forever and everywhere. The pragmatic, existentialist approach to the debate will be justified when we uncover edifying information, relevant to some practical circumstances that certain people might find themselves in, that can accordingly inform such people's choice of epistemic stance in such circumstances.

Many philosophers writing on the scientific realism debate have made it quite clear that they are not trying to determine which position is best for specific (types of) people, or most effective in a given practical context. Rather, they are explicitly trying to determine which position is "correct" in that universal sense. When developing his

version of anti-realist empiricism, years before articulating his meta-epistemologically voluntaristic framework for understanding the scientific realism debate, van Fraassen readily admitted that it might make a great deal of difference to the working scientist which philosophy of science she adopts. He attributes to Paul Feyerabend the idea that "Realism is a philosophy that stimulates scientific inquiry; anti-realism hampers it", noting that "[w]e might even suggest a loyalty oath [to realism] for scientists, if realism is so efficacious" (1980, p. 93). He immediately clarifies, however, that determining the practical efficacy of a realist or an anti-realist attitude for the working scientist is simply not his aim. From a philosophical perspective, he argues, any criticism of an anti-realist empiricist account of science on the basis of realism's methodological fruitfulness in the practice of science raises "a totally false issue … for the interpretation of science, and the correct view of its methodology, are two separate topics" (ibid.). Van Fraassen is interested in interpreting science to reveal its governing aim, telos, or end-in-view, which as an anti-realist empiricist he deems to be the construction of empirically adequate theories. His anti-realist interpretation of science can account for the fact that many working scientists expressly search for explanations thusly: "the search for explanation is valued in science because it consists *for the most part* in the search for theories which are simpler, more unified, and more likely to be empirically adequate" (ibid., emphasis in original).[7] Thus, it makes sense on his account that many working scientists are realists, but not because realism gives a correct account of what counts as success in science; rather, scientists are often realists because searching for explanation often gets us what we truly want, namely empirically adequate theories. So to argue that realism is correct and anti-realist empiricism is not because realism is more methodologically fruitful for scientists, or because scientists as a matter of fact often search for explanations, would be fallacious, drawing a philosophical conclusion about the adequacy criteria operant in scientific theorizing from a social or psychological fact about which assumptions and attitudes work best for its practitioners.[8]

Van Fraassen is entirely correct here. Determining whether scientific realism and anti-realism are differentially beneficial to the individual working scientist cannot help us determine which of those philosophies of science gives the correct account of what ultimately counts as success in science. But this does not, of course, imply that such pragmatic questions about the role of philosophy of science in scientific practice are unimportant or unanswerable; nor does it imply that answers to such pragmatic questions cannot be found, or are immediately obvious. The practical efficacy of different philosophies of science for working scientists remains a matter of empirical, social, psychological, or otherwise practical fact, even though such facts are irrelevant to deciding the traditional (and, as noted at the outset, apparently stalemated) philosophical debate over scientific realism and anti-realist empiricism. Such under-investigated

[7] These issues are more extensively dealt with in Fine (1984a, b, 1986a, b) as well as Kukla (1998). Both authors echo van Fraassen's contention that anti-realism, like realism, can account for all relevant aspects of scientific practice, and that as a result the scientific realism debate cannot be decided on the basis of some scientific activities being "laden" with realist or anti-realist assumptions. Psillos (2000) gives what I consider a failed attempt to show that some scientific activities are, in fact, "realism-laden" enough to warrant our acceptance of scientific realism as the correct philosophical account of science.

[8] See van Fraassen (2004) for more detail on what it means for a philosopher to determine "adequacy criteria" operant in scientific theorizing.

facts should, once determined, prove quite relevant to an individual working scientist's decision about which epistemic stance to adopt on the basis of their idiosyncratic values and context, even if they are not at all relevant to deciding van Fraassen's questions about science's "end-in-view".

With motivations and methodology for the pragmatic, existentialist approach to the scientific realism debate now both well established, a brief example of this approach in action is prudent. What follows is a sketch of an historical episode that I find especially illustrative of what Robin Hendry calls an "historiographical intuition" that realism and anti-realism do not equally well motivate different forms of scientific practice, from the perspective of a working scientist, even if realism and anti-realism can both account for all relevant aspects of scientific practice (Hendry 2001, p. S27; cf. Leplin 1986). This case study shows how adopting different philosophical outlooks on science can, in fact, differentially motivate and support different kinds of scientific activities. What is nice about this example is not just that we find several very different approaches to a single field of research existing contemporaneously, neatly divided along philosophical lines, but also that in retrospect all of these approaches proved instrumental to the advancement of that field, in characteristically different ways. If further investigations show this case to be representative of general trends in the history of science, as I suspect they will, we will have shown that certain philosophical outlooks are best for those involved in certain kinds of scientific activities, even though no single outlook is best for scientific practice in general.

6 Philosophies of science at work in late nineteenth century electrodynamics

In the late nineteenth century there were (at least) three distinct approaches to electrodynamics research in Europe, all operating contemporaneously: the Weberean action-at-a-distance approach; the Maxwellian field-theoretic approach; and the Helmholtzian action-potential approach. It was an interesting time for electrodynamics research, as ontological questions about the nature of electricity and magnetism, as well as methodological questions about how best to study and represent them, were by no means as settled as they are now. Accordingly, these three programs of electrodynamics research varied widely in terms of their ontological and methodological assumptions, as well as the theoretical frameworks they used to represent the phenomena. But they also, we will see, varied in terms of the philosophical conceptions of the nature of science that informed the development of those theories, ontologies, and methodologies. Paying attention to these philosophical divisions, the way they affected scientific practice, and the contributions that each school made to the advancement of electrodynamics suggests some very specific hypotheses about which epistemic stances prove most fruitful during specific types of scientific research.

By the late nineteenth century, Weber's theoretical framework was based around a fundamental ontological assumption about the physical structure of charges and currents: all electrodynamic effects are the result of the movements and interactions of two types of charged particles, acting instantaneously on each other at a distance (Buchwald 1994, p. 8). Within his framework, all electrodynamic phenomena were

modelled in terms of the exchange and movement of these particles, even when it was mathematically difficult to account for a given phenomenon (such as magnetism) in this way. Weber was certainly a committed realist about these electrical particles, so much so that in 1870 he (somewhat) anticipated Rutherford by developing a planetary model of the atom that had positive particles being orbited by negative ones (though, to be sure, his particles should not be confused with the modern electron and proton). These electrical particles were so foundational to his physical thinking that he expected this "electrical theory of matter" to explain not just electricity and magnetism, but also atomic structure (and therein the whole of contemporary chemistry) and even gravity (and therein the whole of contemporary physics).[9]

Weber's realism about electrical particles also expressed itself in the laboratory, where it proved especially effective at motivating efforts to experimentally determine various electrical magnitudes, parameters, and constants. Electrically charged macroscopic objects were understood to function as containers for unobservable electrical particles, whose presence was supposed to account for the observable, macroscopic charges and currents; as a result, Weberans could only conduct laboratory experiments with apparatuses whose parts were well understood from the point view of their theory, e.g. parts made of materials whose capacities for "storing electric particles" were well known. Because of this, the laboratory activities overseen by Weber in his facility in Göttingen were almost exclusively oriented towards the fine measurement of the properties of these particles and various relevant parameters (such as the capacity of various materials to "store" electrical particles).

From Weber's perspective his laboratory studies provided information about the true nature of these unobservable particles, along with the capacities of different materials for storing or conducting them. To use a phrase of van Fraassen's meant to capture a realist's understanding of instrumentation and experimentation (2008, Ch. 4), Weber's vision of laboratory experiments and scientific instruments was that they gave him a "window into an invisible world". On a realist's understanding of science, scientific instruments such as microscopes, particle accelerators, and the double-slit apparatus allow scientists to detect and measure the properties of unobservable things like electrons and microbes, effectively extending their senses into the unobservable realm.[10] While Weber's electrodynamics has largely been discarded by history in favour of a field-theoretic approach, the incredibly refined and precise measurements he made in an effort to uncover the true nature of his electrical particles could be stated as absolute magnitudes, and this allowed them to be directly incorporated into other electrodynamic theories, such as Maxwell's field-theory.

We generally count the completion and wide adoption of Maxwell's theory as a watershed moment for electrodynamics, a clear moment of scientific progress that lead to the development of special relativity along with all the attendant theoretical

[9] See Wise (1981) or Woodruff (1962) for brief but excellently detailed discussions of Weber's program, in the context of contemporary approaches. For a shorter account, see (Buchwald 1994, Ch. 1). For an extensive account of his model of the atom, including a discussion of its relation to gravitational effects, see Assis et al. (2011).

[10] For example, see the discussion of "detection properties" in Chakravartty (2007a) for a realist understanding of scientific instruments along these lines.

insights (time dilation, gravitational lensing, etc.) and technological applications (wifi, GPS, etc.). But at Cambridge where the Maxwellians were developing this theory, the Cavendish labs were, under Maxwell's direction as first Cavendish professor, not at all concerned with the kind of fine measurement activities that Weber was undertaking. Rather, the Maxwellians for the most part used experiment to test and demonstrate broad theoretical principles.[11] Without the eminently realist approach of the Webereans, focused on (as they saw it) investigating the properties of the unobservable world, the Maxwellians would have been missing a lot of data that, in the eyes of Maxwell himself, eventually proved instrumental in the formulation of a field-theoretic electrodynamics (see Maxwell 1873, Ch. XXIII). That is to say, without the data provided by Weber's distinctly realist efforts, Maxwell's theory would not have enjoyed the successes it did, and scientific progress in electrodynamics would have suffered as a result.

Unlike Weber, Maxwell was no realist. While he often thought through different mechanical models of the ether—sometimes imagining invisible cogs, elastic solids, flowing waterways, or "displacement currents"—the Maxwellians saw as their end goal the formulation of analytic principles capable of describing and predicting physical phenomena. In what is now seen as a characteristically "British" approach to theory building (see Hesse 1966) the Maxwellians were not scared of making realist assumptions for heuristic reasons, modelling electromagnetism through analogy to other physical systems whose dynamics were already well known, then cobbling together into principles the various bits of mathematical analysis derived from their models before discharging those models. As long as the theoretical principles they derived from these models helped them analytically predict the behaviour of more concrete electrodynamic arrangements, their job was done. In this way, their various models were always considered hypothetical in nature, and were generally treated (quite explicitly) as mere heuristics.[12]

Philosophically speaking, the Maxwellians themselves seem to sit outside the dichotomy of empiricism and metaphysics. They were, by and large, pragmatic theory builders, and as such they might best be seen as taking a third kind of epistemic stance, not discussed in extant meta-epistemologically voluntaristic treatments of the scientific realism debate. Further study of alternatives to the dichotomy between empiricism and speculative metaphysics is certainly warranted, but for now we might simply call this third stance "instrumentalism", and characterize it as a stance that prioritizes not

[11] Sir David Gill once reminisced of Maxwell that "his experiments always failed" (Forbes 1916, p. 17). A "failed" experiment of this sort was possible mainly because Maxwell used physical experiments not to discover new phenomena, or to make precise measurements of relevant parameters, but rather only to illustrate established physical principles. His invention of the colour top to illustrate the trichromatic theory of colour, his use of a zoetrope to illustrate the motion of smoke rings traveling in the same direction, and his plaster model of Gibb's thermodynamic surface for water all speak to the way Maxwell treated laboratory work: as a means of illustration of broader theoretical principles. Thought experiments, by contrast, were often used to a different purpose, specifically the extraction of analytic principles that could be matched to the known phenomena and, eventually, redemonstrated in the lab. But physical experiment, in the Maxwellians' day-to-day practice, had little to no role in the process of theory-building.

[12] For more on Maxwell's characteristic scientific methods, with emphasis on the philosophical commitments behind them, see Morrison (1992).

the search for explanations, nor the minimization of epistemic risk, but the use of "whatever works" above all else.[13] Here the aim of science would be understood as the formulation of widely predictive, unified theories, and the attendant conception of laboratory work would be the demonstration that those theoretical principles make accurate predictions.

Neither the activities that appeared most natural from Weber's realist approach nor those that appeared most natural from Maxwell's instrumentalist approach were likely to advance the field of electrodynamics all on their own. Weber's theory struggled to model many electrodynamic phenomena, especially light, and the Maxwellian's laboratory work did not provide them with a lot of important data. The results of both Weber's realist approach and Maxwell's instrumentalist approach proved integral to the development and advancement of our scientific understanding of electrodynamics. Their philosophical commitments motivated these activities in the sense that, as Hendry put it, it is unlikely they would have as fervently undertaken such activities without those specific commitments (2001, p. S27; cf. Leplin 1986). But even taken together, Weber and Maxwell's work did not do everything needed to advance our understanding of electrodynamics. Somewhat surprisingly, Helmholtz's eminently and explicitly empiricist approach to scientific research also played a key role (with some irony) in demonstrating the superiority of a field-theoretic approach to electrodynamics.

In the words of one biographer, "[a]ll his life, Helmholtz proclaimed himself a confirmed empiricist" (Meulders 2010, p. 197). He was committed to the idea that ultimately experiment *could not* uncover truth about unobservable reality, precisely because "[i]n immediate experience we find only extended diversely configured and composite bodies; only on these can we make our observations and perform experiments" (Helmholtz 1871, p. 17).[14] Because of this understanding of science and experiment, Helmholtz understood the laboratory as a place not for uncovering facts about unobservables, nor for demonstrating theoretical principles, but as an empiricist most naturally would, as a place for producing and exploring new and different scientific effects. For an empiricist characteristically understands the epistemic function of experimental apparatuses and scientific instruments quite differently than either a realist or an instrumentalist. On an empiricist's understanding scientific instruments are devices that simply "create new observable phenomena [… that then] become part of what science is meant to 'save"' (van Fraassen 2008, p. 96). Scientific experiment, for the empiricist, constitutes "a literal enlargement of the observable world, by the creation of new observable phenomena, rather than a metaphorical extension of our senses" (ibid, pp. 98–99). On this understanding, experiments and instruments never take us beyond the level of the observable, they merely expand and enrich it. Creating

[13] There are other ways we might characterize the Maxwellian's chosen epistemic stance, e.g. as a form of fictionalism. Nevertheless, since our concern in this sketch is primarily with determining the way that adopting speculative metaphysics or empiricism motivates certain forms of scientific practice, we need not dwell on determining how best to characterize the Maxwellian philosophy of science or epistemic stance. Future characterization and investigations of epistemic stances beyond the empiricism/speculative metaphysics dichotomy are certainly warranted, though outside the scope of this particular paper.

[14] For more on Helmholtz's empiricist vision and how it affected his scientific activities, see Moulines (1981) and Turner (1977).

a novel experimental effect forces any prospective theory in that domain to account for a new phenomenon, discrediting any current theory that cannot account for it and raising the standards to which all future theories will be held. Accordingly, Helmholtz's entire electrodynamics research program, much like his previous work in acoustics and physiology, was organized around trying to produce such novel phenomena.

In his laboratory, Helmholtz's main activities involved placing charged or current carrying objects in various novel arrangements and then recording the energetic transfers that were observed. The aim was simple: to discover something new and exciting, hopefully something that could not be accommodated by either Weber or Maxwell's schemes, thereby validating Helmholtz's approach over theirs. To do this he purposefully developed a representational framework that was ontologically pluralistic, open-ended, and macroscopic. The idea was that this framework could serve as a kind of book-keeping device for recording the results of his experiments without committing to any particular ontology regarding the ultimate nature of electrodynamic action (Buchwald 1993, p. 327, 1994, p. 9).

Helmholtz himself made no major contributions to the advancement of electrodynamics. It took one of his students—Heinrich Hertz—to demonstrate the utility of Helmholtz's approach to the study of electrodynamics by creating an important and novel scientific effect: the artificial production of "electric waves" through a spark-gap transmitter/receiver system. This proved significant because, in Hertz's own words, Maxwell's theory "affirms the possibility of the class of phenomena here discovered just as positively as the remaining electrical theories [specifically Weber's and Helmholtz's] are compelled to deny it" (Hertz 1962, p. 19). Hertz had looked through the existing theories of electrodynamics to find some prediction of novel phenomena, found that field-theory predicted the existence of self-propagating electromagnetic radiation, and then deployed the explicitly empiricist laboratory methods that he learned from Helmholtz in an attempt to bring about this predicted scientific effect and study it exhaustively. And as it turned out, his efforts were successful: he artificially produced a wide variety of self-propagating waves, an important and novel class of electrodynamic phenomena. This is exactly the type of thing that Helmholtz had tried (in vain) to do for his entire career in electrodynamics, and had trained Hertz to do as his protege in Berlin (Buchwald 1994, Ch. 2). Hertz's primary intention had been simply to explore a novel class of phenomena, rather than to support Maxwell's theory. Nevertheless, his work quickly drew attention away from competing theories and focused the scientific community's attention on field-theory, for while Helmholtz had *hoped* his theory would be general enough to accommodate any novel phenomenon, like Weber's system it ultimately proved incapable of accommodating Hertz's "electric waves".

The effectiveness of an empiricist approach in this context is perhaps unsurprising. As van Fraassen notes, the empiricist view of scientific experiments as "engines for creating novel phenomena" is modelled mainly on those situations where "different theoretical leanings compete" (2008, p. 97). In those situations, the most significant experiments are often those that involve "the production of new phenomena that all theories in [this] domain must account for if they are to compete successfully there at all" (ibid., p. 98). Van Fraassen rightly identifies Hertz's production of electric waves as a perfect example of how anti-realist empiricists understand laboratory work,

along with Arago's experimental production of Poisson's predicted bright spot. For our purposes, the lesson of empiricism's effectiveness in scientific practice should be clear enough: when trying to advance science by discovering novel phenomena, empiricism seems to be the most fruitful epistemic stance. For without the deeply empiricist laboratory methods that Hertz learned from Helmholtz, committed as they were to the ontologically noncommittal exploration of electrodynamic phenomena, Maxwell's theory would not have enjoyed such sudden (and warranted) attention, and scientific progress would have suffered as a result.

7 Conclusion

As the preceding case study illustrates so nicely, there seem to be specific circumstances (e.g. when making refined measurements of natural constants, property magnitudes, and theoretical parameters) in which adopting a metaphysical stance supports progressive scientific practice more readily than adopting one of its rivals. At the same time, something similar can be said for its empiricist rival in other circumstances (e.g. when attempting to produce and explore novel phenomena in the laboratory), as well as for its under-considered instrumentalist rival in other circumstances (e.g. when constructing powerful, unified theories).

Let me be clear, however, that one case study does not prove a stable historical association. I have aimed primarily to give an example of what a pragmatic, existentialist approach to the scientific realism question can look like: using historical methods to show that certain epistemic stances facilitate success in certain forms of scientific inquiry more readily than other stances. While I would suggest the associations seen in this case study as a kind of working hypothesis for further investigations of the motivational effects that philosophies of science generally have on different forms of scientific practice, I do not mean to suggest that this case study alone establishes the stability of these associations. Brief surveys of the kinds of work done by other notable realists (e.g. Dalton's fine-grained experimental work on relative atomic weights) and empiricists (e.g. Mach's novel discovery of supersonic shock waves) seems to support the associations observed in the case of late 19th century electrodynamics. Nevertheless, further research must be done to determine whether a metaphysical stance really does better support fine-grained measurement activities, empiricism really does better support the discovery of novel phenomena, and instrumentalism really does better support the construction of powerful, unified theories. My hope is that we will find they do, or that we will find other important such regularities, and that by making such information explicit (and thereby available to working scientists who might be sitting on the fence viz empiricism, speculative metaphysics, and possible alternatives) we may actually provide such fence-sitters with edifying information: given the state of their field, and what they feel is necessary to make advancements (more refined measurements, more exploration of novel phenomena, or more free-flowing theorizing), they can see which epistemic stance will likely best serve their purposes, and make a well-informed choice accordingly.

Similar projects could be undertaken—and I would argue, should be undertaken—to determine which epistemic stance is most appropriate for science policy makers,

historians of science, or science educators, in a variety of contexts. I would also argue that we should expand our methods beyond historical research, and attempt to discover associations between certain philosophical commitments and success in different practical activities by using the methods of statistics, science and technology studies, and experimental philosophy, e.g. by conducting surveys of contemporary scientists, policy makers, and science educators to determine their philosophical commitments, then correlating them with different kinds of success in those fields. Such projects, however, sit outside the scope of this paper, which has only aimed (and I hope succeeded) to make a pragmatic, existentialist approach to the scientific realism debate seem appealing for philosophers of science who accept that the traditional scientific realism debate has, in fact, reached an intractable stalemate.

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