12/18/20

On the question of realism

In a number of recent, and some perhaps now not so recent, writings, I've been describing a straightforward but highly idealized inquirer called the Second Philosopher.[1] She begins with simple perceptual beliefs, progresses to generalizations, then experimentation, then theory formation and testing, always circling back to examine and correct her beliefs and methods as she goes. Along the way, I've confronted her with a series of traditionally 'philosophical' questions to explore how they look from her perspective: the challenge of radical skepticism, the nature of truth and reference, the ground of logical truth, and the ontology, epistemology, and methodology of mathematics, from arithmetic to higher set theory. None of this amounts to a direct argument for approaching philosophy as the Second Philosopher does, but my hope has been that at least some readers will feel the draw.

Though I never set out to address the status of the theoretical posits of natural science, the issue came up indirectly in a discussion of Quinean holism, the linchpin of his indispensability

[1] See [2001], [2007], [2011c], [2014c], [2017a].

argument for the existence of mathematical entities.[2] I'd felt free to help myself to indispensability considerations – then widely accepted – in my early realistic philosophy of mathematics,[3] but nagging conscience soon led me to doubt that the role of mathematics in 'our best scientific theory' could bear this kind of ontological weight. In fact, the assumptions of science seem a remarkably varied lot, from explicit idealizations (like continuity in fluid dynamics), to requirements that may or may not be accurate (like the continuity of space-time), to items currently posited but whose relation to the world is still largely mysterious (like the wave function of quantum mechanics).

This line of thought brought me to the case of atoms, before and after Perrin's famous experiments at the turn of the last century.[4] I argued that they were part of 'our best scientific theory' before Perrin, but weren't fully confirmed until after, and thus, that holistic indispensability alone is not enough to support ontological conclusions. (The same would presumably go for other general conformational rubrics like inference to the best explanation.) And so, incidentally, the presence of mathematical entities in 'our best scientific theory' wouldn't be enough to establish their existence.

In this way, pursuit of a question in the philosophy of mathematics landed me – and the Second Philosopher with me – at least

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[2] See [1997], II.6.

[3] See [1990].

[4] See [1997], pp. 135-143, [2007], pp. 404-407.

in the vicinity of the realism/instrumentalism debate in the philosophy of science: I'd claimed, on her behalf, that we have rationally compelling evidence, now, for the existence of atoms – better evidence even than Perrin, given cloud chambers, electron microscopes, and so on – which would apparently make me a realist.[5] I say 'apparently' because, paradoxically, claiming rational belief in atoms doesn't seem to be enough to qualify one as a 'realist' as the term is currently used, or at least as its used by two influential instrumentalists: Bas van Fraassen and Kyle Stanford. My goal here is to trace how this paradox plays out in the views of these two thinkers. The point is comparatively straightforward for van Fraassen, so I start with him, then use his constructive empiricism as a foil for the more complex case of Stanford's epistemological instrumentalism. In the end, I hope to show that the distance between Stanford's position and the Second Philosopher's is considerably shorter than labels and rhetoric would suggest.

I. van Fraassen

In van Fraassen's terms, both Second Philosophy (what the Second Philosopher does) and Empiricism are stances:

[5] van Fraassen [2015], p. 7, footnote 5, remarks that 'The debate between empiricists and scientific realists is not over the reality of unobservable entities, but over the *telos* of scientific activity', but he admits that 'What is true is that the debated questions would be moot given an epistemology that would entail that the existence of unobservable entities … could be established on the basis of the evidence'. Unpacking this precisely will have to wait, but for now, the Second Philosopher and I are claiming that the existence of atoms, and thus the existence of unobservable entities, has been established (at least within the bounds of scientific fallibility) by experimental evidence.

> By a stance I mean a position that consists in attitudes, commitments, and a characteristic approach to philosophical problems, possibly including or presupposing some beliefs as well, but not defined by a thesis or doctrine. (p. 65)[6]

> Maddy's Naturalism [is] characteriz[ed] quite explicitly as a *philosophical stance*. (p. 66)

Even the quick sketch of the Second Philosopher given above should demonstrate that this attribution is entirely accurate.[7] This raises the question whether rational disagreement between two stances is possible; van Fraassen's example is the analytic metaphysician:

> Can we not say: the metaphysician disagrees, has a different and incompatible stance, but disagreement does not make one unscientific [or] irrational. (van Fraassen [2002], p. 48)

Arguing that the answer is no, van Fraassen switches to the example of the crank:

> a disregard for evidence, a refusal to submit one's ideas to natural selection by relevant experiment or to engage in vigorous testing when nature itself does not put one to the test (these are some examples, none of them factual beliefs!) can certainly take one beyond the scientific pale. So the paradigm of science does not suggest that disagreement in the sphere of attitudes, commitments, values, and goals is invulnerable to empiricist critique. On the contrary, it suggests that it is. (van Fraassen [2002], p. 48)

So perhaps there is room for rational adjudication of any disagreement between the Empiricist and the Second Philosopher.

Since science, in some sense, is to act as judge, and since both parties tout their fundamental respect for that practice,[8] the

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[6] Unaccompanied page numbers in the text refer to van Fraassen [2015].

[7] I chose to introduce Second Philosophy by describing the behavior of the Second Philosopher because I took it to be what van Fraassen would call a stance (see, e.g., [2007], p. 1).

[8] Cf. van Fraassen [2015], p. 64: 'Naturalism and Empiricism place the admiration of empirical science, as a paradigm of rational inquiry, at center stage'.

Empiricist is here faced with a more delicate case than those of the analytic metaphysician and the crank. Under the circumstances, it's obviously important to get the fine points of the Second Philosophical stance right. I don't think van Fraassen quite does this, so I'll try to set the record straight here and there. Some of these corrections, especially at the beginning, may seem nit-picky, but my hope is to escalate in small steps and display along the way what I take to be the particular virtues of the Second Philosopher's approach. Whether this amounts in the end to a comparative advantage for her stance over the Empiricist's is obviously a separate question.

But first a note on terminology. Van Fraassen discusses the Second Philosopher under an alias: the Naturalistic Native, or Native for short. His aim is to distinguish the inquirer I describe from me, the person doing the describing – that person he calls the Naturalistic philosopher.[9] This distinction seems less important to me than it does to him because I take his Naturalistic philosopher – me – to be using the same methods, to be taking the same stance, as his Naturalistic Native – my inquirer.[10] The sources of our disagreement on this point should be clarified below (see (2)).

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[9] See van Fraassen [2015], p. 67.

[10] In an APA book session on [2007], Barry Stroud raised the question whether I, the author of the book, was a Second Philosopher. When I'd asked myself this question in the past, I'd felt confident that the answer was yes, because I wasn't using any methods that aren't available to the Second Philosopher, but it turned out that Stroud had something else in mind: were the questions I was addressing ones the Second Philosopher would naturally be led to ask? Here again, I think the answer is yes. I take this as obvious for cases like the nature of truth and reference, the ground of logical truth, the proper methods of mathematics and their rational defense, and even for some of the issues surrounding radical skepticism, but a few others, like the analysis of the enduring attractions of the dream argument and the

1. Scope

I have in mind here both questions about where the Native starts and about where she ends up. Van Fraassen takes her to begin with 'the *currently accepted scientific view*' as 'the rock to build on', as her 'personal foundation … as the received knowledge, or securely founded opinion, about what is the case'.[11] He sets this in contrast with the Empiricist, who takes the same body of beliefs 'as our natural first target for analysis and reflection, for critique and interpretation, as precisely what you need to subject to critical reflection'. I have used the phrase 'native to the contemporary scientific worldview' – a nod to Quine's 'naturalistic philosopher' who 'begins his reasoning within the inherited world theory as a going concern' (Quine [1975], p. 72) – but the Second Philosopher is first introduced along the lines given above:[12] she begins with ordinary perception (not some pre-certified conclusions to be taken for granted) and gradually extends her methods, always circling back to reassess and correct both those methods and her beliefs.[13] Notably, in

argument from illusion in [2017a] or of contemporary worries about the prospects for metaphysics naturalized in [2007], V.5, only arise when the Second Philosopher inquires into the nature of the human practice of philosophizing. But she will get to that eventually!

[11] All the quotations from van Fraassen in this paragraph come from his [2015], p. 76.

[12] Though van Fraassen [2015] makes heavy use of [2001], the Second Philosopher doesn't make her appearance until [2007].

[13] See [2007], p. 14, [2011c], II.1, [2014c], p. 2. Though I consider the possibility of characterizing the Second Philosopher top-down instead of bottom-up (in [2011c], IV.5), even in that case, her starting point isn't 'the currently accepted scientific worldview', but the entirety of our beliefs, reasonable and not. (Van Fraassen [2015], p. 75, may have this in mind when he says 'if we ask where we are just now, we are confronted by a plethora of answers from our neighbors and peers, as well as by uncertainties

none of this does she employ a distinction between scientific beliefs and the rest, which she would need in order to identify the starting point van Fraassen assigns to her. (I assume we'd all count this as a virtue, given that there's no way of corralling the ever-evolving 'methods of science' into a fixed criterion.[14]) This means, for example, that she meets Descartes's Method of Doubt on its own terms: she doesn't denounce the search for a priori certainties as 'unscientific' – if there's a way to get them, as Descartes promised, she'd like very much to know about it! – it's just that as she follows his line of thought, she doesn't see that his various claims are justified.[15]

Though he doesn't attribute this explicitly to the Native, van Fraassen also includes a strain of ontological parsimony in his general characterization of Naturalism:

> the ontological view that everything is physical, material, or within the domain of the natural sciences. … it is typically understood to classify, as needing to be 'reduced' or explained away, such items as consciousness, reference, goodness, beauty, purpose, functions, etc. (p. 63)

Another corollary to the Native's starting point is that she begins with no such restrictions in place. As to where she ends up, there's also no pre-existing restriction to what's typically regarded as the disciplinary hard sciences. She observes particular details that fall

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and ambiguities in what we find ourselves tending to answer'.) The Second Philosopher's job is then to figure out how to whittle this sprawling mass down to its justified portions.

[14] See [2007], p. 1.

[15] See [2007], I.1, [2017a], pp.7-18.

beneath the notice of physics, botany, or metallurgy – the pattern of bruising in the grass where the cannonball falls or the remnants that cling to the ball itself as it rolls to a stop – and her passion for generalizing and understanding would reach into disciplines like history, sociology, anthropology, and linguistics. Interdisciplinary thinkers behaving second-philosophically are currently engaged in vision science and consciousness studies, and there's no predicting what revamping of our ontological expectations might be needed as these inquiries develop. For that matter, my own second-philosophical work in the foundations of higher set theory makes room for mathematical abstracta.[16] I don't know what might emerge when the Second Philosopher turns her attention to the human practice of moral judgment, but it obviously falls within her purview, and the discovery of objective moral facts, for example, can't be ruled out in advance.

2. The force of 'native'

At one point, van Fraassen describes the 'Naturalistic Native' as someone who 'lives [in the scientific worldview] as unselfconsciously as a fish in water or a bird in air' (p. 92). Modulo the caveat above about 'the scientific worldview', this sounds about right for the second-philosophical Native, but elsewhere we find descriptions that seem slightly off: van Fraassen's Native embodies 'the common sense of the educated layperson in our culture today' (p. 66); she's 'the "model reader" of the texts that the sciences offer the public' (p.

[16] See the Thin Realism of [2011c].

67, see also p. 68). Here the Native is pictured as standing apart, being guided by what she regards as 'scientific'; we hear of 'the relevant scientific community to which [the Native] defers' (p. 84). An odd consequence of what might seem a trivial deviation is that the Native is now compared to a scriptural fundamentalist; the utterances of scientists are to be taken as gospel, at face value, with no room for further investigation.[17] The distance between this figure and the second-philosophical inquirer I've described should be obvious: the Second Philosopher doesn't defer, she simply speaks as a scientist, citing evidence and subjecting her beliefs to scrutiny. When asked why she believes in atoms, the second-philosophical Native doesn't say 'because the scientists say so'; she says 'because of this evidence', citing results from Brownian motion, cloud chambers, electron microscopes, and so on. Let's reserve the term 'Native' in what follows for the occupant of this stance.

So what distinguishes van Fraassen's other figure, the 'Naturalistic Philosopher'? She's the one who describes the Native, but, of course, this isn't enough to set her apart – a person can describe herself. Her defining characteristic, as far as I can tell, is that she rejects all forms of transcendentalism,[18] that is, views

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[17] See van Fraassen [2015], pp. 81-82. For example, van Fraassen suggests that this leads the Native to misunderstand Dumas's remark that 'never in chemistry must we go beyond the realm of experiment' as expressing a 'positivist philosophical prejudice'. I don't see that I myself attributed such a prejudice to Dumas (see [1997], pp. 136-137), but the current point is that the Native is perfectly capable of assessing the historical context of a past scientist's remark and understanding it accordingly.

[18] See van Fraassen [2015], pp. 66-67.

that posit a second level of inquiry standing over and above, separate from, that of the Native. (Kant's critical philosophy is the prime exemplar.) I can see why van Fraassen might think this rejection can't be the work of the Native – she has no tools to address positions that explicitly set her entire framework aside – but this is only a problem if the 'rejection' of these views is understood to involve a direct counterargument. In fact, all that's intended is what van Fraassen calls the Native's 'disturbingly uncomprehending stare' (p. 82), though there's more to it than that. The template for the Native's interaction with the transcendental philosopher goes roughly like this: the transcendentalist explains that his inquiry is different from, disjoint from, the Native's empirical study; the Native asks what this special inquiry is designed to find out and what methods it uses to do so; when the answers to these questions are unsatisfying and/or unconvincing, the Native sees no reason to sign on to the transcendentalist project. That's what the 'uncomprehending stare' of the 'rejection of transcendentalism' comes to - and it falls well within the Native's capabilities. If van Fraassen's Naturalistic Philosopher is to attempt something stronger, a direct refutation of transcendentalism, then she goes beyond the powers of my Second Philosopher.

One reason, then, that van Fraassen feels the need to distinguish the Naturalistic Philosopher from the Native is an overly strong understanding of what the Naturalistic Philosopher does, but the Native is also being understood as too weak: the 'Native is no philosopher, and cannot be confronted with philosophical questions'

(p. 68). On the contrary, as noted above, I've argued at length elsewhere that the Native will eventually face, and even answer, many traditionally philosophical questions. So, in the end, I remain unconvinced of the need to distinguish the Native from the Naturalistic Philosopher, unconvinced of any 'studied ambiguity' or unstable 'vacillation' between the two (pp. 68, 84). Still, there does remain one stark difference between me, the author, and the Second Philosopher I describe: she's highly idealized; she knows so much more and is so much smarter than I am!

3. Interpretation

In van Fraassen's eyes, the central shortcoming of the Native is that she is incapable of 'interpretation'. He freely admits that she doesn't take 'a completely uncritical attitude' (p. 84) toward her current beliefs and methods, that she assesses and corrects them as she goes, but interpretation is somehow more than, or different from, just that. What she's incapable of doing, according to van Fraassen, is 'bracketing' something that she currently accepts. This sounds at first like the claim that the Native can't call any of her well-confirmed beliefs into question, which is obviously too strong,[19] but van Fraassen's intentions come clear when he describes the new challenges that arise when we bracket:

> questions as to how to understand … scientific activity, of the criteria of success apparently applied in intra-community assessment of the work in that area, and therefore of its aim. (p. 85)

[19] This would seem to be the upshot of the lifejacket example (van Fraassen [2015], p. 85).

Unsurprisingly, the two salient options for that aim are truth and empirical adequacy. This is where the realism/instrumentalism debate is joined.

At this point, van Fraassen is careful to distinguish the Native from other exemplars of what he terms 'naturalism'. He bemoans her inability to see that the Empiricist – in claiming the aim of science is empirical adequacy, not truth – isn't raising a scientific question about, say, the evidence for the existence of atoms based on Brownian motion. In fact, this way of (mis)understanding Empiricism is just the Native's attempt to distill an intelligible question out of a transcendental debate.[20] As we'll see in more detail in connection with Stanford, there were those in Perrin's day who doubted we could ever know unobservables and who regarded atomic theory as a useful fiction. When those doubts are removed by Perrin's experimental successes, the Native takes it that a realism/instrumentalism debate integral and essential to scientific activity has been resolved – science itself would be hamstrung if this sort of doubt and resolution were relegated to some extra-scientific realm of evaluation. So, the Native understandably imagines that when van Fraassen denies we know there are atoms, he's raising a similar doubt, but that he's doing so now, when there's abundant rationally compelling evidence for the existence of molecular, atomic, and even sub-atomic structures.

[20] As van Fraassen [2015], p. 87, recognizes: 'the only question … that makes sense to the Naturalist Native is the factual question whether unobservable entities such as atoms or molecules exist. That is a question she takes to have been addressed, and answered, by scientists themselves'.

Understandably, she sees him as challenging the epistemic force of that evidence, and sets out to defend it.

Van Fraassen contrasts this slow-witted reaction on the Native's part with that of the 'naturalist' Stephen Leeds, who addresses the Empiricist directly, on his own terms. As understood by van Fraassen, Leeds argues that since inference to the best explanation is a principle of scientific inquiry, it should be applicable, too, in the interpretive mode to establish the correctness of Realism as opposed to Empiricism (p. 79). Obviously, this sort of thing falls nowhere near the Native's repertoire; she doesn't even regard inference to the best explanation as conclusive in the first place![21]

Still, some features of other naturalisms do seem to bleed over into van Fraassen's treatment of the Native. For example, he emphasizes Leeds's insistence that there's no need to justify or validate scientific inferences (p. 79), then goes on to claim that despite the Native's reliance on observation, 'she sees no need to investigate the relation between *observation* and the final answer as to what is so or not' (p. 82). I'm not sure what to make of 'final' here, but the Native certainly does engage in serious investigation of how the senses register information about the world, of when they tend to be reliable and when not – for the case of vision, this is one of the foci of contemporary vision science. More dramatically, van

[21] Section II.6 of [1997] makes a case that inference to the best explanation, along with Quinean holism, is not a principle of scientific inquiry. Perhaps it's also worth noting that it's possible to think the aim of science is truth without thinking our best theories aim to be 'true throughout' (van Fraassen, p. 86).

Fraassen takes Arthur Fine's Natural Ontological Attitude to entail that it makes no sense to consider alternative understandings of quantum mechanics, then remarks that this is 'Undoubtedly the Naturalist Native's reaction as well!' (p. 83). But it isn't. Offering an account of how quantum mechanics relates to the world is only a more elaborate cousin of the earlier question about whether atomic theory describes actual physical entities. This is the sort of criticizing and assessing that the Native does all the time, the sort of thing that she offered up as a possible understanding of van Fraassen's 'interpretation' and that van Fraassen rejected.

And this is the point, really. 'Interpretation', for van Fraassen, isn't part of the Native's ordinary empirical inquiry. It's a reflection on that inquiry, not from within, but from a point outside.

4. Transcendentalism

This brings us to van Fraassen's transcendentalism, a label he appears to embrace without protest in this passage:

> Maddy goes on to display her Naturalism as a recurring revolt against transcendentalism, in a broad sense – against 'two-level' philosophical views, citing Kant, Carnap, and me as the main examples. (p. 66)

What makes a view 'two-level' for me is its methodological independence from ordinary science. Van Fraassen isn't offering a direct critique of Perrin's experiments[22] or of the scientific

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[22] A possible exception is his suggestion that understanding Perrin's results 'as providing evidence for the reality of molecules … is a rather strange reading' partly because his 'research was entirely in the framework of the classical kinetic theory in which atoms and molecules were mainly represented

community's subsequent embrace of atomic theory. His objection, rather, is to Perrin's prose and that of those around him, which would include Einstein, Poincare, Ostwald, and the Nobel Prize committee:

> Such pronouncements are important for the historian, to indicate the terms in which such episodes were discussed, but we must always keep in mind that these words do not come in the context of a philosophy seminar, where our distinctions are made, or the conceptual problems are disentangled in the way we do. (van Fraassen [2009], pp. 22-23, footnote 20)

Viewing the matter in this special philosophical way, one comes to appreciate that what was established wasn't 'the reality of molecules' (ibid., p. 22), but the bankruptcy of

> the idea that it might be good for physics to opt for a different way of modeling nature, one that rivaled atomic theories of matter. (Ibid., p. 23)

But this rejection of the prose makes no difference in practice: the Native, inclined to speak, and dare we say believe, with Perrin and company, is left to carry on as before.[23]

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as hard but elastic spheres of definite diameter, position, and velocity' (van Fraassen [2015], pp. 22-23, see also van Fraassen [2009], p. 8). If the scientific use of idealizations were enough to make the Empiricist's case, the debate would be over before it started! What Perrin and company claim isn't that every detail of atomic theory is correct – they're well aware that it involves these false assumptions – but that the model it describes resembles the world in important ways, most importantly, in the particulate nature of matter. (See previous footnote.)

[23] Imagine this debate between Perrin, standing in for the second-philosophical Native, and van Fraassen. Perrin claims to have strong evidence for the existence of molecules. Van Fraassen disagrees: what Perrin has done is establish the 'empirical grounding' of atomic theory. Perrin responds that, yes, before we only had evidence from large collections of molecules; I figured out how to get direct empirical access to the individual behavior of molecules, and along the way verified the most unlikely aspect of that behavior, namely, the random walk. Again van Fraassen disagrees: what you and your coworkers did was to develop both theory and measurement procedures so as to enhance the empirical adequacy of atomic theory. Perrin wonders why van Fraassen is so keen on this strange way of describing the situation – and here van Fraassen's reasons will have to do, not with the specifics of Perrin's work, but with his Empiricist stance and his seminar-room debate with the Realist. From that point of view, van Fraassen's instrumentalist would say that Perrin is just wrong about what he's

This returns us, at last, to the question whether there's room for rational adjudication of the case between the Native and the Empiricist. Van Fraassen admits that is 'it is still possible [to] read [Perrin's] results as providing evidence for the reality of molecules' (van Fraassen [2009], p. 22), which might suggest that this is a dispute between stances with no definitive resolution, but I suspect that in fact this possible reading of Perrin's experiments isn't being given by the Native, but by those who share the seminar room with van Fraassen – Realists like Leeds, for example, who directly oppose the Empiricist there. This comes out more clearly in his earlier extended analysis of Perrin's work: there he debates Salmon, Glymour, and Achinstein, but remarks that 'Maddy takes for granted … Perrin's reasoning … and does not offer a competing account to these' (van Fraassen [2009], p. 6, footnote 4). ('Taking for granted' here recalls the earlier 'deferring'; we've already noted that the second-philosophical Native doesn't defer.) Of course, the Native doesn't simply offer Perrin's evidence uncritically any more than Perrin himself did – it's rightly submitted to all appropriate scientific scrutiny – but it is true that whatever assessing gets done is simply part of scientific activity. Though van Fraassen likens the Native to the Kantian 'transcendental realist', I think the label applies more properly to those Realists who undertake to rebut the Empiricist on his own terms – these are the thinkers van Fraassen

accomplished; the realist would say that he's right, but that he doesn't fully understand the reasons why. However, they would both heartily agree that his science is fine, even exemplary, quite unaffected by his errors in the seminar room.

takes himself to have wrestled to something like a draw (though 'in retrospect', theirs is 'rather a strange reading' (ibid, p. 22)). Despite the Native's claim to have good evidence for the existence of atoms, the term 'realist' as it's used in this literature doesn't appear to apply; Realists, properly so-called, reside in the seminar room, engaging directly with Empiricists about matters that transcend ordinary science. The Native and van Fraassen's Empiricist aren't in direct conflict after all, no adjudication is required.

Notice, by the way, that from the Native's point of view, this Realist is almost as mystifying as van Fraassen's instrumentalist. Faced with evidence from Perrin and his successors, both seem to think that it isn't enough, that it requires supplementation (the sort of thing Leeds' use of inference to the best explanation in the seminar room is intended to provide); they just disagree on whether or not that supplementation can be provided. The Native naturally wonders what's wrong with her evidence as it stands. This isn't to say that she doesn't subject that evidence to critical scrutiny – we've seen that she does, just as Perrin and his contemporaries did, just as scientists do all the time – but she doesn't insist, as the seminar-room Realists do, on some peculiarly philosophical line of assessment. She attends, rather, to the specifics of her particular case: were the experimental protocols sufficiently stringent? Were the calculations correct? Were all relevant variables accounted for? In the end, both sides of this transcendental debate escape her.

One last note. I've claimed that despite her inability to cotton on to the discussion in the seminar room, the Native – or Second

Philosopher – still manages to address many traditional questions about skepticism, logic, language and the world, arithmetic and higher mathematics. *Second Philosophy* and subsequent writings aim to demonstrate just how much she can do. It's hard to know what to call these inquiries if not 'philosophy' – maybe 'Natural Philosophy' in something like the sense of the early moderns?[24] In any case, if there remains any doubt that I, the author of these works, function as a (sadly limited) Second Philosopher, I can report that my own reaction to transcendentalism in general, and van Fraassen's transcendentalism in particular, is the same as hers: I can't see what the point of the project is supposed to be or what methods it might properly employ.

II. Stanford's epistemic instrumentalism

In sharp contrast to van Fraassen, Stanford is no transcendentalist. His views have evolved over the years – I hope to trace the broad outlines of that development – but his naturalism has been constant, beginning in his influential book, *Exceeding Our Grasp*:

> I am not reaching beyond or outside of science itself for evidence of some supposedly higher or purer kind with which to sit in global judgment on the scientific enterprise as a whole. With those philosophical naturalists who emphasize the essential continuity of philosophical and scientific efforts to acquire knowledge, I hold that there is only good and bad evidence, not higher and lower evidence or scientific evidence and some other kind. Indeed, I expect my argument to be congenial to … naturalists of this sort. (Stanford [2006], p. 37)

Later he proposes, more specifically, what he calls 'integrative naturalism':

[24] See Essay #1.

> For … integrative naturalists, understanding what our best scientific theories are telling us about the world and understanding how we go about entheorizing that world in the first place are not distinct challenges: both are part of the overarching and more fundamental challenge of trying to *simultaneously* understand both the world and our own place within it. … That project has plenty of moving parts, from developing new statistical methods and quantitative measures of confirmation to building better microscopes, and from proposing and testing novel empirical hypotheses to interpreting the historical record of scientific inquiry, but these are all parts of a single interconnected inquiry. (Stanford [2016], p. 93)

So Stanford begins from a stance very like that of the Second Philosopher.[25]

From this perspective, Stanford mounts an original historicist critique of scientific realism and replaces it with his own 'epistemic instrumentalism'. Crucially, his characterization of the line between those parts of our theory that qualify as literal and those merely instrumental makes no appeal to a notion of observation. Typically, the instrumental parts will concern domains where

> it is difficult to acquire information … because the entities inhabiting them are too small or too large or too amorphous for us to readily perceive; because the causal interactions between those entities are too fast or too slow or too rare or take place on too grand a scale for us to engage with in ordinary ways; these entities and interactions occur in times and places either far removed from our own or otherwise inconveniently located (e.g., at the dawn of life on Earth, in remote regions of the universe, at the center of the Sun), and so on. (Stanford [2006], p. 3)

But membership in such a domain, by itself, isn't enough;[26] for example, though dinosaurs admittedly 'roamed the earth long ago …

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[25] As he acknowledges (see Stanford [2016], p. 93, footnote 1).

[26] See, e.g, Stanford [2006], p. 35: 'we neither invariably reason eliminatively about unobservables, nor invariably reason otherwise about observables, and it is the application of eliminative inference outside its domain of reliable operation, not observability as such, which represents a legitimate source of concern'.

> fossilization is not a hypothetical postulated mechanism but a process we can study in action and we simply have no specific reason to doubt that its products in the remote past were any different from its present ones. (Ibid., p. 33)

The determining feature, then, is the kind of evidence we have for our account of a given theoretical posit – the topic of the next section. For now, it's enough to note that the Second Philosopher's theory of atoms, based on the evidence of Perrin and his successors, concerns a domain 'too small … for us to readily perceive' and clearly qualifies as a potential target of Stanford's critique.[27]

I think it will help clarify the contours of Stanford's historicist critique and his epistemic instrumentalism if we consider a historically located version of the Second Philosopher, an embellished version of Perrin, newly in possession of the evidence described in his *Atoms* (Perrin [1913]). At the turn of the 20th century, atomic theory was fundamental to chemistry, and kinetic theory had flowered in physics.[28] Still, there were doubts about the reality of atoms, as attested by this remark in a leading textbook by Wilhelm Ostwald, one of the founders of physical chemistry and an eventual Nobel prize winner:

> the atomic hypothesis has proved to be an exceedingly useful aid to instruction and investigation … One must not, however, be led

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27 See Stanford [2006], p. 210, where 'rocks are made up of atoms with a specific internal composition' is offered as an example of a belief that separates the realist from his instrumentalist. I worry a little that 'with a specific internal composition' might commit the realist to every detail of contemporary atomic theory (something Perrin* avoids in the description below), but let's set this aside for now.

28 I discuss this case in more detail in [1997], pp. 135-143, and [2007], pp. 404-407.

> astray by this agreement between picture and reality and combine the two. (Ostwald [1904], p. 151)[29]

The copious evidence for atomic theory at that point was all abductive: the theory as a whole was explanatorily and predictably powerful in dramatic ways, but the tiny molecules in endless random motion that it posited were entirely theoretical, entirely inaccessible.

At the time, Perrin agreed that 'the skeptical position … was … legitimate and no doubt useful' (Perrin [1913], p. 216), and so did Einstein, who set out to correct the situation:

> My major aim in [developing 'the statistical mechanics and the molecular-kinetic theory of thermodynamics'] was to find facts which would guarantee as much as possible the existence of atoms. (Einstein [1949], p. 47)

What Perrin accomplished was to design and carry out experiments so precise that this restless motion was revealed for the first time in the movements of his tiny manufactured particles – all in perfect harmony with Einstein's detailed predictions. Ostwald and other skeptics were convinced.

Thus the historical Perrin, the basis for my imaginary, fully second-philosophical Perrin*. This figure is a fallibilist to begin with – he makes no claim to certainty about anything – and he realizes, of course, that theoretical science – the sort of thing the instrumentalist calls into question – is more epistemically risky than, for example, ordinary perceptual belief. In the particular case of atomic theory, he worried at one time that despite its immense

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[29] Quoted by Richard Miller [1987], p. 473.

success, it was in some sense unverifiable, the domain it describes ultimately inaccessible. But he now believes that he has a new and decisive kind of evidence – a means of access, of direct verification – based on his experiments and other considerations available at the time. This doesn't mean that he believes every part of the theory he embraces – it naturally involves idealizations and simplifications, plus a few somewhat dicey assumptions[30] – but he does at least tentatively believe that he's closed the case for the particulate structure of matter in the small and its accompanying randomness.

Unlike van Fraassen's empiricist stance, Stanford's arguments come from within integrative naturalism, so they ought to engage directly with Perrin*, they ought to present considerations he'd be duty-bound to address. My plan, then, is to confront this figure with Stanford's historicist challenge.

1. Stanford's historicist challenge

Any gloomy appeal to the historical record naturally begins with the classic pessimistic induction: many successful scientific theories of the past have turned out to be false; why should we expect our own apparently well-confirmed theories to be any different? Despite his sympathy for this line of thought, Stanford believes 'there is at least some justice'[31] to the reply that our best current theories have

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[30] See van Fraassen [2009], p. 8: 'Perrin worked throughout with the "billiard ball" version of the kinetic theory. In his models, molecules are perfectly hard, perfectly elastic spheres, taken as relevant approximation.' He points out that Rutherford's work on atomic structure was known at the time.

[31] See Stanford [2006], §2.3. The quotation is from p. 44.

important virtues that distinguish them from the failed theories of the past, so he offers a critique that rests on the failures of theorists rather than the failures of theories. His thought is that even if our current theories are better in significant ways, it's unlikely that our current theorists – mere humans, after all – are significantly better than those of the past.

To isolate the systematic failure of scientists in the problematic domains, Stanford suggests that the evidence we have for many of our theories has a distinctive character:

> the reasons we can offer for believing [their claims] would seem … to be limited to the fact that each of the fundamental hypotheses in question offers the most powerful and convincing systematic account we have for explaining, predicting and intervening with respect to a wide range of empirical phenomena … and we can neither offer nor even imagine any alternative hypothesis whose performance in these respects would be equally impressive. (Stanford [2006], p. 34)

This is precisely the kind of holistic, abductive evidence that Perrin and his peers considered insufficient in the late 19th century. In Stanford's telling, this type of inference to the best explanation, as it's also called, consists of an eliminative induction to the last theory standing.[32] The trouble with this – and here we have Stanford's fundamental point – is that the range of alternatives we've eliminated only includes those we were clever enough to think up, and the historical record suggests that scientists of the past very often didn't think up all the alternatives – indeed alternatives that ended

[32] I use the terms 'abductive inference', 'inference to the best explanation', and 'eliminative induction' interchangeably in what follows.

up being accepted later on in the progress of science.[33] Given it's unlikely that humans are more imaginative now than they were in the past, Stanford presents Perrin* with the kind of ground-level challenge that didn't turn up in van Fraassen's constructive empiricism: in problematic domains, which include the micro-structure of matter, human theorists are prone to misapply eliminative induction – what makes you think you aren't doing the same? In other words, what makes you think your new evidence is of a qualitatively different kind?

One line of defense against Stanford's argument would be to argue that his examples aren't representative, that they're all three drawn from the early history of genetics, before that science had found its footing,[34] but I imagine Perrin* turning instead to his own house, asking whether his inference is badly flawed in the way Stanford has identified. Answering this question properly would require careful examination of the reasoning of Darwin, Galton, and Weismann in Stanford's examples and even-handed comparison with the detailed structure of Perrin*'s evidence and argumentation. Without attempting anything like this here, I imagine Perrin* might at least begin his response by pointing to a few disanalogies.

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[33] For the record, Stanford doesn't impugn all abductive inferences in science, only those for which we lack a clear overview of the space of possibilities (see Stanford [2006], p. 32). This happens most often in the problematic domains, and since our focus will be on one of these, the case of atomic theory, I won't continue to insert this caveat.

[34] E.g., Godfrey-Smith [2008], p. 142, thinks the historicist critique should rest on 'mature' theories, which classification wouldn't include the genetics of Stanford's period. Magnus and Callendar [2004] raise doubts about the comparison class from which the historicist's examples are supposed to be drawn.

The biologists in question were contemplating an ill-understood phenomenon, heredity, and searching for a reasonable theory that would explain it; this is the kind of situation that invites an eliminative induction or an inference to the best explanation and thereby risks the kind of error Stanford identifies. In contrast, Perrin* has a well-developed theory in hand, a theory that has been effectively explaining and predicting chemical and physical phenomena since Dalton's work a century prior. Perrin* isn't pulling a new theory out of the air to explain Brownian motion; he's using the case of Brownian motion to put a highly successful existing theory to the most stringent test available, a test Einstein had developed for precisely that purpose.

Some realists have taken a different line of defense, arguing instead that the scientists in Stanford's examples weren't so badly wrong, after all, that they were right about certain central features of their theories and the rest was irrelevant. One way to execute this strategy would be to argue for a general criterion that allows us to differentiate the confirmed parts from the rest. Stanford points out that such efforts often depend on considerable hindsight: we identify these confirmed parts by looking back at a theory and selecting those that coincide with our more mature understanding of the situation. But this is unhelpful, Stanford argues, because what the realist needs is a way of telling which parts of our current theory are confirmed, and that requires a criterion that can be applied without the benefit of this kind of Monday morning quarterbacking.

Now we've seen that Perrin* doesn't take all details of atomic theory to be confirmed by his experimental results – there are all those idealizations and simplifications and possibly dodgy hypotheses – but he does take them to confirm the particulate nature of matter in the small and its accompanying randomness. He offers no general criterion for identifying the relevant morals of any scientific theory – if I were to put a few more words in his mouth, I'd say he was skeptical that there is such a thing (see section 4, below) – but for now, what's of interest is that there is a participant in Stanford's realism/instrumentalism debate, Stathis Psillos, who represents something that sounds compatible with Perrin*'s stance. Stanford summarizes Psillos's 'selective realism' like this:

> [His] intriguing alternative approach promises to evade these persistent problems by avoiding the need for any *explicit* criterion of selective confirmation at all … he seeks to finesse this problem by arguing that working scientists *themselves* routinely judge different parts, features, or aspects of extant theories to be differentially confirmed by the empirical evidence and that the historical record shows these judgments to be generally reliable. If so, we can … safely rely on *scientists' own* judgments in identifying the selectively confirmed, trustworthy aspects of existing theories. (Stanford [2006], pp. 173-174)

Stanford responds to this with a second historical induction whose goal is to establish that scientists' own judgments in such matters are not in fact 'generally reliable'.

Viewed from Perrin*'s perspective, this presents a fresh challenge: not 'given Stanford's historical examples, am I relying on the same sort of flawed eliminative induction?', but 'given more of Stanford's historical examples, am I making the same sort of mistake about which parts of atomic theory my evidence confirms?' Once again, there's room for a general argument about whether Stanford's examples are representative, but as before, let's suppose Perrin* looks to his own house: can he distinguish his case from those Stanford cites? As before, I won't attempt to answer this question in any detail, but a quick review of the examples – 19th century vitalists, Maxwell's defense of the ether, Lavoisier's insistence on caloric fluid – finds repeated arguments, not only that such-and-such exists or happens, but that an alternative is inconceivable. So, for example, how could something be transmitted from here to there without some 'medium or substance in which [it] exists after it leaves one body and before it reaches the other'?[35] The quasi-conceptual flavor of these discussions contrasts sharply with Perrin*'s circumstances: he's not only open to the conceivability but to the real possibility that matter isn't particulate; that's why he (along with Einstein) is so keen to find a test! So I think there is room for Perrin* to reply to this historicist challenge as well.

The important point in all this is that Stanford's epistemic instrumentalist has issued a direct challenge to Perrin* – show that your evidence is truly different in kind from the previously available purely abductive evidence – and this challenge is one that Perrin* would find both understandable and appropriate. In this way, Stanford, unlike van Fraassen, opens a direct dispute over the epistemic force of Perrin*'s experimental results. I take this ground-level dispute to be the crux of Perrin*'s confrontation with

[35] This comes from Maxwell on the ether, quoted by Stanford [2006], p. 152.

the epistemic instrumentalist. My goal here isn't to mount a case in favor of Perrin*'s side of the argument, though I believe there is one to be made and I often gesture in that direction. Rather - to anticipate - it seems to me that, contrary to what one might expect, this debate, this crux, often isn't Stanford's primary focus, and much of what follows is an effort to trace how and why this is so. For now, let me just note that this isn't the issue Stanford forefronts at this point in *Exceeding Our Grasp.*

Instead, Stanford focuses on the claims of his opponents in the professional literature, in particular, on the realist who appeals to some refinement of the Success Argument:

> the only satisfactory explanation for the success of our scientific theories is that they are (at least approximately) true … any other view of the matter leaves it a complete and utter miracle why our best scientific theories are so successful. (Stanford [2006], p. 6)

Such a realist believes in atoms because they are (the right kind of) posits of (the right kind of) successful scientific theory. Obviously this is not Perrin* – it was his own specific experimental findings, not a claim about scientific theories in general, that resolved the issue for him.

Thus, Stanford's historicist challenge to Perrin* and to the scientific realist. Let's now consider what he takes to follow from his critique.

2. The moral

To orient ourselves on this question, let's first consider what moral Perrin* would draw if he were convinced that his conclusions rested on one of Stanford's bad eliminative inductions, on an open-ended inference to the best explanation, that his evidence for atomic theory consisted exclusively of its explanatory and predictive powers, the interventions it makes possible, and our inability to think of anything else that could do as well. How should he react? From his perspective, all he's accomplished is to add more evidence of the same kind as he had before; accepting Stanford's critique would mean that he and Einstein had failed to achieve their goal of establishing the reality of atoms. Presumably, at that point, he would try again. Nothing in Stanford's position rules out the possibility that he might eventually find the right kind of evidence, evidence as effective as fossils are for our theory of dinosaurs.[36]

Surprisingly, Stanford doesn't see the situation this way:

> This does not mean the moral suggested here is that we must somehow constrain and regulate the inferences we draw in the course of our scientific theorizing by some perfectly abstract and general commitment to the likely existence of completely unspecified but serious unconceived theoretical alternatives. What could this amount to but … a sure recipe for inferential (and therefore conceptual) paralysis? (Stanford [2006], p. 135)

He would deny that Perrin* was 'somehow irresponsible or careless' or 'made reckless inferences' (ibid., p. 134), or that his instrumentalism is a 'narrow and defensive creed', as Popper once claimed (ibid., p. 209):

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[36] What would Perrin* do if all these efforts failed? What if it appeared that atomic theory was wildly successful, but that there were no atoms? I submit that Perrin* would then seek some other explanation, some other, more subtle relation between the theory and the world, to explain the theory's success. See the M/W principle in section 4, below.

> The epistemic instrumentalist will insist no less than the realist that we continue to challenge our best scientific theories by uncovering and testing further empirical implications they have, that we use them to unearth new phenomena and new ways to predict and intervene in the course of events around us, that these theories serve as the appropriate starting point in trying to determine how they themselves can be refined, improved, and developed … She will not … pursue the further implications of our theories less doggedly, or invest those implications with less significance, than the realist … In short, the instrumentalist is in a position to take the claims of our best scientific theories about nature every bit as *seriously* as the realist does. (Ibid., p. 210)

The recommendation seems to be that Perrin* behave just as he would have done if his inference to the reality of atoms had been sound.

Having ruled out Perrin*'s reaction – back to the drawing board! – Stanford considers two other candidate responses to the fundamental error he's uncovered. One is 'a vapid agreement to tack the phrase "but of course there may be something I haven't thought of yet" piously and toothlessly onto every conclusion we draw' (Stanford [2006], p. 135). Oddly enough, this sounds like what Perrin* already does, given his general fallibilism and his awareness of the heightened epistemic risk involved in theoretical science; he says this about his conclusion that atoms are real, despite being unconvinced by Stanford's arguments that his evidence is weaker than he thought.

Whether this 'pious' addendum is toothless is a question we'll get to in a moment, but as the rhetoric makes clear, it isn't Stanford's choice. He prefers, instead, that we adjust our attitude toward our theories, that we recognize that scientists are 'not entitled to believe the conclusions of their eliminative inferences', that we restrict our belief to consequences of the theory that 'can be understood independently of the theory … toward which we are adopting an instrumentalist stance' (Stanford [2006], p. 197). This instrumentalist stance is nothing exotic, he tells us; it's the same as the contemporary realist's attitude toward Newton's theory of gravitation when 'she allows that we can perfectly well make use of [it] to send rockets to the moon without believing … [that] gravity is … a force exerted by massive bodies on one another, [that] there is … absolute space or time, and so on' (ibid., p. 204). The contemporary realist is also an instrumentalist about quantum mechanics, except that in this case we have no more inclusive theory that explains its success. The instrumentalist and the realist both take instrumentalist stances towards some theories and realist stances toward others; they just draw the line in different places.[37]

(Notice, by the way, that here Stanford places the realism/instrumentalism debate within science, as van Fraassen does not. In particular, presumably Stanford would take Perrin and Ostwald to have been instrumentalists about atomic theory before the decisive experiments and realists after; they simply redrew the line in light of new evidence. Here the ground-level debate comes back into view: was Perrin's evidence sufficient to make this move rationally compelling?)

At this point, it's hard not to worry that Stanford's withholding of belief has no more teeth than Perrin*'s fallibilist scruples, that he's simply being asked to add a pious 'but I don't believe it'

[37] See Stanford [2006], pp. 204-205.

instead of 'but I could be wrong'. And Stanford does worry.[38] There are hints of a response in *Exceeding Our Grasp*,[39] but the fully developed version doesn't appear until the aptly named 'Catastrophism, uniformitarianism, and a scientific realism debate that makes a difference' (Stanford [2015a]).

Stanford borrows the terms 'Catastrophism' and 'Uniformitarianism' from geology, but in his usage, Uniformitarians 'hold that the future of the scientific enterprise will be characterized by revolutions and transformations every bit as profound and consequential as those we find throughout its history', while Catastrophists 'believe that truly profound and fundamental revisions to our scientific understanding of the world are now largely confined to the past' (Stanford [2018], pp. 214-215). His thought is that the instrumentalist is a Uniformitarian, while the realist 'thinks that contemporary theories have things sorted out at least roughly right and that our remaining errors are simply errors of detail' (Stanford [2015a], p. 874) and is thus apparently a Catastrophist. Catastrophists, unlike Uniformitarians, he takes to be theoretically conservative by nature - that is, disinclined to pursue 'transformative' or 'revolutionary' science. And there, Stanford concludes, we have a difference that makes a difference.

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[38] Recall that there's no such worry for Perrin*: as we've seen, the success of Stanford's argument would have enough teeth to send him back to the drawing board.

[39] See Stanford [2006], pp. 209, 211. Also [2014c], p. 124.

Obviously, much depends on how we understand these revolutionary changes. Here Stanford doesn't attempt a general characterization or posit any Kuhnian incommensurability. Instead, he takes the admirable path of explicating the notion by what he calls 'historical ostension' (Stanford [2018]), that is, he gives us examples of past theories that were superseded by such changes – Newtonian mechanics, Dalton's atomic chemistry, Lavoisier's caloric theory of heat, Maxwell's account of electromagnetic ether – all of which he takes to be 'radically false', 'fundamentally and profoundly mistaken', not 'even approximately true'.[40] Stanford assumes the reader shares his dismay at the defects of these theories – that gravity turned out not to be a force, that atoms turned out to have substructure, that there is no subtle fluid behind heat phenomena or mechanical medium for the transmission of electromagnetic waves – so that we would count no one a realist who thought her current theory are akin to these. But Perrin* would be delighted if his theory turned out to give us as much fresh insight into the workings of the world as Newton's or Dalton's or Lavoisier's or Maxwell's![41] This reaction is now commonly described as the claim that these theories weren't really so terrible after all, that they were in fact 'approximately true'.

Here the debate threatens to devolve into one over the viability of an appeal to 'approximate truth', but Stanford argues that this is

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[40] Descriptions like these appear throughout, but these three are from Stanford [2014], p. 117, Stanford [2006], p. 157, and Stanford [2014], p. 117, respectively.

[41] Section 3, below, expands on this reaction.

a distraction. The real disagreement remains that between Catastrophists and Uniformitarians, because any realist who professed also to be a Uniformitarian, who embraced the idea that her theory is comparable to the cited theories of the past, would be indistinguishable from an instrumentalist inspired by the historical critique (Stanford [2015a], pp. 876-877). Perrin* does believe that scientific progress will produce changes like those in the cases Stanford cites; this realization informs his conviction that theoretical science is especially epistemically risky. It would seem to follow that, despite his confidence in the particulate structure of matter, Perrin*'s 'realism' is indistinguishable from 'instrumentalism'. Fortunately, this puzzle can be set aside for now, because Stanford comes to concede that a realist can consistently embrace Uniformitarianism. So this effort to locate 'a difference that makes a difference' fails (though the charge of conservatism eventually reappears in the new context (see section 4).

For what it's worth, it seems to me that there may well be real methodological differences between Perrin* and the epistemic instrumentalist:[42] for example, if Perrin* has a theory incompatible with another well-confirmed theory, he counts this as evidence against his theory until a way can be found to reconcile them, and when Perrin* arrives at a new theory by a strong inference to the best explanation, he will want to know why this theory is so successful.

[42] Recall that Perrin*, as we're understanding him, doesn't accept Stanford's critique of his evidence. If he did, then (as noted above) there'd be a clear methodological difference: Perrin* would go back to the drawing board, while the epistemic instrumentalist would simply withhold belief.

(In the case of atomic theory, this took the form of a concerted effort to find a stringent test for its literal truth.) Stanford considers the first in passing, coming to what seems to me ambivalent stance,[43] and the second reappears in section 4, below (as the M/W principle). But before leaving this stage of the discussion, I'd like to make two observations.

The first concerns the route we've taken to arrive at this point. In his effort to give methodological teeth to 'but I don't believe it', Stanford has shifted his focus away from the distinctive critique of abductive evidence that separates his instrumentalism from a generic pessimistic induction and toward a more general feature that the two versions share: a distrust of current theories based on a negative assessment of past theories. The contrast between Uniformitarianism and Catastrophism rests on the identification of sample historical cases and two attitudes toward them – there will be more like this and there won't – where the former is taken (for now) to characterize instrumentalism in general and to preclude realism. This switch seems to me unfortunate, because it leaves behind the ground-level debate between Perrin* and the epistemic instrumentalist

[43] See Stanford [2006], p. 209-210: 'the epistemic instrumentalist will insist … perhaps even that we work to unify our various scientific theories with each other and with whatever else we believe to produce a single coherent, consistent, and systematic account of the natural world as a whole'. But then, in a footnote to this passage (ibid., p. 213), he admits that 'the rationale for this … may not be entirely obvious, as instrumentalism seems to offer little epistemic motivation for insisting that our theories not contradict one another'. He concludes that 'the instrumentalist's motivation for consistency is pragmatic rather than epistemic' and 'of course the instrumentalist would seem better positioned than the realist to make sense of and live with the fact that our best theories seem at present to be neither fully mutually consistent nor maximally unified'.

that we've been tracing. The question there is whether Perrin*'s specific evidence has the force he claims, not whether a selection of superseded theories from the past should give us pause about what we're tempted to believe now.

The second observation begins more locally, in Stanford's case against the selective realist. There the challenge for the realist is to identify those parts of her current theory that she takes herself to be in a position to stand by, so to speak – in Perrin*'s case, the particulate nature of matter and its attendant randomness. My question is what this 'standing by' comes to. I've been understanding it to characterize those elements of our current theory that we take to be confirmed, that we take ourselves, now, to be rationally justified in believing. Sometimes, it's possible to read Stanford's text in this way, but more often he includes a predictive element: to stand by a claim is to predict that it will survive in future theories.[44] Before taking up the final, or at least the latest, turn in Stanford's overall line of argument, I'd like to pause to lay out my concerns about this way of putting the point. This will raise a general question about the historicist method, but this remains tangential to the crucial ground-level debate just recalled.

3. On historicism

[44] That is, sometimes what matters is 'whether we should *trust* or *believe* the accounts of otherwise inaccessible natural domains given by our best scientific theories'; other times what's involved is 'distinguishing the parts of present theories that will be preserved or retained in their successors'. (Both quotations come from Stanford [2006], p. 183.)

Historically motivated instrumentalism obviously depends on a negative assessment of now-superseded theories of the past. When it comes to characterizing this attitude, we've seen that Stanford presents us with examples – 'historical ostension' – and counts on us to share his negative assessment. Often enough, the assessment is that they are simply false, indeed 'radically false' (Stanford [2014], p. 117), but his thinking on truth and falsity takes a subtle turn in his recent, delightfully titled ' "Atoms exist" is probably true, and other facts that should not comfort scientific realists' (Stanford [2015b]).

Stanford is considering a flat-footed objection:

> Historicist critics of scientific realism are often met with a wry (even indulgent) smile and some version of the understandably incredulous inquiry, 'Surely you don't seriously doubt that atoms exist?' (Stanford [2015b], p. 5)[45]

In response, he argues that contemporary confidence that 'Atoms exist' is true doesn't guarantee that earlier atomic theories weren't fundamentally and profoundly mistaken - thus able to contribute to the historicist critique - despite the implications of his cheeky interlocutors.[46] Along the way, he adopts a view about reference, and

[45] I blush to admit that I recognize myself among these annoying interlocutors, though I don't suppose that I'm alone in this!

[46] For what it's worth, I'm not sure these interlocutors are out to claim that our current acceptance of 'Atoms exist' undermines Stanford's disparagement of earlier atomic theories. Rather, their point seems to me resemble a Moorean anti-skeptical gambit: you've given an argument, I don't care what, that implies we don't know that there are atoms; which is more plausible, your argument or the claim that we know there are atoms? Or perhaps more bluntly, an attempted reductio: your argument implies that we don't know there are atoms; can you really live with that consequence?

hence about truth, that seems to me problematic for a proponent of the historicist approach. This is what I'd like to explore.

Stanford opens this line of thought with a quotation from Howard Stein at his delightfully irascible finest:

> For my part, I throw up my hands at this: why should we say that the old term 'ether' failed to 'refer'? – and that the old term 'atom' did 'refer'? … our own physics teaches us that there is *nothing* that has *all* the properties posited by nineteenth-century physicists for the ether *or* for atoms; but that, on the other hand, in *both* instances, rather important parts of the nineteenth-century theories are correct. (Stein [1989], pp. 56-57)

Stanford admits that it's hard to imagine any principled reason for this difference, going so far as to note that some decisions about terminological continuity seem to be inspired by public relations as much as anything else. Next comes Mark Wilson, making a persuasive case we imagine our concepts equipped to make definitive classifications in novel cases, when often enough a new usage in fact arises from irrelevant historical contingencies.[47]

Stanford's diagnosis of this situation calls on a proposal of my own for a disquotational theory of reference.[48] The feature of this account that matters for his purposes is one of two diagnostics I use to distinguish disquotational from correspondence theories: when Pierre says 'la neige est blanche', is the truth or falsity of his utterance completely determined by facts about Pierre, his language, and his worldly surroundings? For the correspondence theorist, the answer should be yes; the truth or falsity of Pierre's utterance

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[47] See Wilson's indelible example of the Druids from his [1982].

[48] See [2007], Part II, or more recently, Essay #6.

depends only on the disposition of the worldly referents of his words. For the English-speaking disquotationalist, on the other hand, the truth predicate is native to English, so the French sentence has to be translated before 'true' can be applied to it either way - which means that his having said something true involves not only facts about Pierre, French, and his surroundings, but also about English and the context and vicissitudes of translation. In cases like Pierre's, the proper translation is obvious, but our retrospective readings of past scientific theories are a more subtle matter.

Consider the much-discussed example of Joseph Priestley and his 'dephlogisticated air'. Suppose he remarks that 'dephlogisticated air is good for breathing'. To assess the truth of this utterance, the disquotationalist must first translate it into our current idiom – 'interpret' is a better term (though not in van Fraassen's sense!), since Priestley wrote in English – and here the choice of translation may depend on the context of translation: for beginning chemistry students, to impress them with Priestley's accomplishments, one might choose 'oxygen is good for breathing'; for historians of science out to track the development of ideas in early chemistry, the homophonic 'dephlogisticated air is good for breathing' would be more appropriate. Once the role of the translator's interests is recognized, any manner of other factors intrude; imagine the complex historical and sociological story we'd need to tell to account for the difference Stein notes between contemporary translations involving the 19th century terms 'ether' and 'atom'![49]

Stanford adopts this point of view:

> This sensitivity of our judgments of referential continuity to terminological decisions … helps to illustrate something that I think has been widely overlooked in approaches usually taken to questions about reference and meaning by philosophers: those approaches have tended to obscure the fact that judgments about referential continuity (and therefore about the truth of past existential commitments) always involve *interpretive* decisions concerning past speakers and linguistic communities. (Stanford [2015a], p. 406)

He takes various correspondence theories to task for supposing

> … that the empirical facts … at the time that Priestley used the term 'phlogiston' are sufficient to determine the facts about which objects or properties in the world were those to which [his term] did and did not refer. But this way of seeing the situation simply ignores the fact that we ourselves are making decisions about how to *interpret* … Priestley. (Stanford [2015a], p. 407)

The moral, for Stanford, is that our current confidence that 'Atoms exist' reflects a host of factors irrelevant to the underlying physical facts of the case, and for that reason, it doesn't conflict with his assessment that, for example, Dalton's atomic theory was radically false.

But surely this moral extends beyond existential claims. Was Maxwell right to think 'the ether must be substantial'? If we're out to document the historical fall of mechanism, then we'd say 'no, of course not'; if we're out to emphasize the fact that the electromagnetic field has mass-energy content, we might say 'yes,

[49] This might be called 'the indeterminacy of translation', if that term weren't already taken for a more ambitious claim. See Essay #6 for more.

though of course Maxwell was wrong to think that being substantial should be understood mechanistically'.[50] So, was Perrin* right to think that 'matter is particulate'? From the point of view of fairly recent quantum field theory, you might say 'it turned out there aren't any particles, just fields' or you might say 'it turned out that particles are small concentrations in the field'.[51] These vagaries would explain why the debate over the 'approximate truth' of past theories seems so stubbornly unresolvable: there's no fact about the theory's subject matter alone that decides the point; our answer also depends on our context of interpretation. The historical case for instrumentalism is mounted by pointing to past theories we're all supposed to agree were radically false, but those judgments depend on how we interpret the theories, and these interpretations are influenced by a range of factors unrelated to the physical facts of the case.

If this is right, the historicist critique of scientific realism is in serious trouble: it draws consequences for our current theories based on retrospective assessments of past theories, but those

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[50] Stanford considers this example in his [2006], p. 161, footnote 13. There he rejects the 'yes' answer on the grounds that Maxwell's 'substantial' must be understood as mechanistic. It's not clear whether this opinion would shift in light of Stanford [2015b].

[51] See Steven Weinberg [1987], pp. 78-79 (quoted by Malament [1996], p. 1): 'A quantum field theory is a theory in which the fundamental ingredients are fields rather than particles; the particles are little bundles of energy in the field'. (This comes close to saying both things in the course of one sentence.) The precise manner of recovering particulate structure in contemporary field theories is a subtle matter, a subject of continued discussion and debate.

retrospective assessments have been shown to be sensitive to irrelevant contextual factors. More locally, challenging selective realists to identify those portions of their theory that will survive in future theories is similarly misguided. For example, it would be pointless to ask Perrin* to predict that future theories will agree that matter is particulate.

The turning point in this line of thought is the recognition of the role of translation/interpretation in a disquotational theory of reference. If some form of correspondence theory could be defended instead – a theory according to which the referent of Pierre's 'neige' and the truth of his 'la neige est blanche' are determined by facts exclusively about Pierre, his language, and his physical environment – then there would be something for our retrospective assessments to get right or wrong, but without that, it's not clear that Stanford can afford to help himself to the idea that our assessments of past theories involve translation/interpretation in anything like the way Stein, Wilson, and I propose.[52] But Perrin* can so help himself, continuing to hold the theories of the past in high regard[53] and stating his commitments in terms of current confirmation, not prediction.

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[52] Stanford [2000] has championed a causal theory in the past, so this might be a natural resting place for him.

[53] Of course, Stanford could forego 'truth' and 'reference' altogether and simply challenge the realist to explain what reason she has to think her current theory is better than the superseded theories of the past. But the very considerations that tempt her to interpret those theories generously must also remove the sting from the suggestion that her current theories are 'no better'.

Perhaps surprisingly, I think this conclusion is less troubling for Stanford's core position than it appears, because I doubt that historical critique is essential to his epistemic instrumentalism. There was a hint of this in the observation that Perrin, Einstein, and no doubt others, were initially wary of atoms despite the overwhelming abductive support for atomic theory. Stanford has argued that they were right to be wary at that point, because the historical record shows that human scientists are bad at cataloguing the entire range of candidate explanatory theories. But it's unlikely that any of these practicing scientists were moved by anything like Stanford's painstaking historicist critique. It was enough for them to observe that any inference to the best explanation is really an inference to the best explanation we've thought of so far, and that as long as the only evidence for atomic theory was exclusively its explanatory and predictive power, there was always the open possibility that something else was responsible for the phenomena in question, that some other relationship between theory and world – more devious the literal truth – was responsible for its success. In this way, the initial ground-level challenge to Perrin* remains intact: does your new evidence extend beyond the abductive in a way comparable to fossil evidence for dinosaurs?

As it happens, the historicist contrast between Catastrophism and Uniformitaranism recedes in Stanford's more recent work, as we'll now see.

4. The middle path

So far, we've reviewed Stanford's distinctive historicist argument,[54] then traced the quest for a 'difference that makes a difference' through the advent of the Catastrophism/Uniformitarianism contrast, and finally to the charge that the realist must be theoretically conservative in objectionable ways. Now, in his latest paper, 'Realism, instrumentalism, particularism: a middle path forward in the scientific realism debate' (Stanford [202?]), we find Stanford identifying a new 'middle path' that's recently arisen between 'historically sophisticated realists and instrumentalists' (Stanford [202?], p. 1). This middle path has three tenets.

Perhaps surprisingly, the first of the three shared tenets of this middle path is Uniformitarianism, which has come to be embraced by some selective realists:

> So-called 'selective' scientific realists have argued, for example, that although we should indeed anticipate further radical and fundamental changes in our theoretical conception of the natural world, we can nonetheless identify particular elements, aspects, or features of our best scientific theories that we can justifiably expect to find preserved throughout the course of such future changes.[55] (Stanford [202?], p. 3)

As we've seen, Perrin* has been a Uniformitarian all along, so he passes this test for the middle path.

The second shared tenet is a denial that those 'radical and fundamental changes of our theoretical conception' involve any kind of Kuhnian incommensurability. The realist easily agrees it this, so the

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[54] Though I've just argued (in the previous section) that the historical element here isn't strictly necessary - and that that's a good thing.

[55] In light of the previous section, note that the selective realist could instead claim to identify those parts of current theory that they take to be confirmed, leaving out the problematic predictive element.

concession here, if any, comes from the instrumentalist side. In fact, incommensurability has never been part of Stanford's epistemic instrumentalism (see Stanford [2006], p. 22).

Third and finally, those on the middle path share this simple conviction:

> when scientific theories are able to achieve robust empirical and practical success, it is surely at least reasonable to think that the reasons for that success will consist in *some* systematic relationship or connection between how the theory represents (some part of) the world as being and how things actually stand there. (Stanford [202?], p. 7)

Stanford generously calls this the Maddy/Wilson principle, but it's not entirely clear that he understands it in the same way that Wilson or I would. The only guidance he gives is to note that the historical record is rich with cases of subsequent theories explaining success of their predecessors. Perhaps he intends a methodological injunction to seek such explanations.

If I may speak for Wilson, I believe we would both happily agree to this, but expect something more. Two of the overriding morals of *Wandering Significance* (Wilson [2006]) are that we shouldn't be too fastidious or dismissive about theories whose success we can't currently explain (e.g., quantum mechanics) and that we should seek such an explanation nonetheless and reasonably hope for one in the fullness of time (as we do for quantum mechanics). This describes Perrin*'s initial attitude: he has an extremely effective theory; he certainly doesn't intend to give it up (neither does Ostwald, for that matter), but he wants to understand why it works. One possible answer – the first answer to try – is that matter is in fact particulate, so Perrin* sets out to test this explanation as best he can. If he hadn’t succeeded, and if other attempts had also failed, presumably he would have looked for alternative explanations.[56] This is what scientists routinely do: it’s why Michelson and Morley attempted to measure the ether wind; it’s why Thorne, Weiss, and Barish sought out gravitational waves; it’s why more than half a dozen underground laboratories are now looking to detect dark matter.

When Stanford agrees that ‘there must *be* some reason why a cognitive instrument that works well does so’ (Stanford [202?], p. 7), he doesn’t explicitly say that this third shared tenet includes a methodological injunction to ask what that ‘systematic relationship or connection’ is, to actively seek an explanation of why a given theory works so well. Still, since he’s engaged in a search for ‘a difference that makes a difference’, it seems safe to assume that he would have noted a disagreement on this point if there were one.[57] In section 1, above, I suggested that their respective attitudes toward this injunction might mark a methodological difference between Perrin* and Stanford’s epistemic instrumentalist, but this potential way of distinguishing them seems to have vanished on the middle path.[58]

So, what difference does Stanford find? Though the middle path realist is no longer a Catastrophist, Stanford repeats the charge that she will be objectionably conservative about the portions of her

[56] Recall footnote 36.

[57] In comments on an earlier draft of this essay, Stanford confirmed that he agrees with Wilson and me on this point.

[58] Whether Stanford's instrumentalist has reason to seek to unify his various theories, as Perrin* does, is a question that remains open.

theory that she regards as confirmed, this time on the grounds that the middle path epistemic instrumentalist

> believes that even more instrumentally powerful alternatives radically distinct from contemporary scientific theories are actually out there still waiting to be discovered. (Stanford [202?], pp. 11-12)

But as a Uniformist, doesn't the middle path realist believe this, too? Assuming she shares Perrin*'s steadfast fallibilism, why shouldn't she, too, see the wisdom in pursuing or funding 'transformative' scientific possibilities? For that matter, Stanford once told us that the epistemic instrumentalist takes our current theories 'as the appropriate starting point in trying to determine how they themselves can be refined, improved, and developed', that 'she will not … pursue the further implications of our theories less doggedly, or invest those implications with less significance, than the realist' (Stanford [2006], pp. 209-210). So it's not entirely clear that there's a significant difference in levels of conservativeness here, without which Stanford hasn't yet given convincing evidence that his instrumentalist's 'but I don't believe it' has methodological teeth (though Perrin*'s 'but I may be wrong' apparently does – helping to guard him from a complacent conservatism).

However this question is resolved, 'Middle path' also contains a new proposal for a methodological separation between the middle path realist and the middle path instrumentalist, but first, there's a tantalizing return to what I've been calling the ground-level dispute. Recall how, in *Exceeding our Grasp*, Stanford emphasized that his epistemic instrumentalist wasn't issuing a blanket injunction against theoretical claims about inaccessible domains; his objection was to such claims that are justified by abduction only, claims for which there's no evidence comparable to fossil evidence for dinosaurs. Similarly, the realist needn't commit herself to every posit or claim in her current best theory, so the difference between them was just over where to draw the line. This point returns in 'Middle path':

> Realists and instrumentalists alike can recognize exceptions to their general or generic expectations in particular cases based on evidence or other considerations specific to the case in question. (Stanford [202?], p. 15)

Elsewhere, Stanford explains in some detail how 'evidence … specific to the case' works to convince the instrumentalist of the epistemic force of fossils.[59] But this is precisely the sort of thing Perrin* sees himself as doing: he presents what he takes to be 'considerations specific to the case' that go beyond the previous, exclusively abductive evidence; like Stanford's instrumentalist, he takes this new and distinctive evidence to support rational belief in the particulate character of matter and its attendant randomness. So it would appear that Perrin* qualifies as a middle path instrumentalist!

But if Perrin* is the instrumentalist, where is the realist in this story? We're told that, roughly speaking,

> realists are generously presuming our successful theories (or privileged parts thereof) innocent unless and until proven guilty, while instrumentalists are cynically presuming guilt unless innocence can be convincingly established. (Stanford [202?], p. 17)

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[59] See Stanford [2010].

So the question is how theoretical claims are 'proven guilty' for the realist, presumably something that hinges on 'evaluation of the details of the *specific* evidence … in support of that particular belief' (ibid., p. 15). If they're guilty by virtue of being supported by exclusively abductive evidence, then realism collapses into instrumentalism, so it must take more than that. For me, at least, this middle path realist remains a cloudy figure – perhaps she is conservative, as Stanford claims – but it's hard not to suspect 'guilty until proven innocent' is simply a better match for actual scientific method than 'innocent until proved guilty'.

However that may be, the middle path realist's attitude is apparently not Perrin*'s. So despite my initial assumption that Perrin* must end up as a 'realist' of some variety, let's leave the middle path realist aside for the moment and continue from here on the assumption that Perrin* is a middle path instrumentalist engaged in the ground-level argument that his evidence is as rationally compelling as fossil evidence. The question is just: does this particular evidence justify this qualitative change in our level of confidence?

Given what we've seen, perhaps it's no surprise that this isn't the discussion that interests Stanford. His focus is neither on the middle path instrumentalist's struggle with her conscience over whether some new, non-abductive evidence is strong enough to satisfy her scruples, nor on her ground-level disagreement with an imagined middle path realist over the adequacy of the evidence in some specific case. Rather, for Stanford, the relevant opponent of the middle path instrumentalist is the middle path *selective* realist who offers not just confidence in a particular case, but a *general* criterion for identifying which parts of any given scientific theory have been confirmed in which haven't.[60] Though Perrin* expresses confidence in the particulate structure of matter and its attendant randomness, he doesn't offer a general criterion of confirmation, and I suggested earlier that he would reject this demand.

This isn't to deny that interesting or illuminating generalizations about the kind of evidence that confirms in this area or that might be possible. For example, some have been tempted to seek out common features in Perrin*'s 'detection' of molecular motion and more recent cases like the 'detection' of gravity waves, and to put forward a specification of what counts as 'detecting' something. My second-philosophical thought, on Perrin*'s behalf, is that any such specification would most likely be descriptive, not normative – that is, if a new case came along with compelling evidence that looked as if it, too, should be classified as 'detection', and if this new case didn't fit the existing specification, I suspect that the specification would be altered, not the new evidence rejected. This is more-or-less what happened in Perrin's case: at the time, detection was observation, and the Brownian motion experiments prompted the move to a broader notion. When someone draws an inference in the form of modus ponens, it seems right to say that the reason the inference is good is because it has that form; modus ponens is the norm. But is it

[60] Assuming the criterion is taken to be normative for this version of realism, the charge of conservatism becomes somewhat more plausible.

plausible to think that the epistemic force of Perrin*'s evidence ultimately depends on features it shares with other evidential situations?[61]

Stanford sees this skepticism about the normative value of general criteria as 'a counsel of despair': we're being asked to content ourselves with the ordinary scientific evidence, specific to the individual case, but this is

> … presumably what scientists themselves have been doing all along, and many of their resulting sincere and carefully considered judgments … have repeatedly turned out to be spectacularly mistaken. (Stanford [202?], pp. 21-22)

Here we're returned to the familiar terrain of 'spectacularly mistaken' versus 'approximately true',[62] which once gave way to Uniformitarianism versus Catastrophism, and then to all-Uniformitarian middle path – which led to the conservativeness charge against the generalizing selective realist.

But what about the Uniformitarian, fallibilist Perrin*, who claims to have specific evidence that goes beyond the eliminative or abductive? His closest approximation in Stanford's taxonomy is the radical particularist:[63]

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[61] See [2007], pp. 402-403. Stanford [202?], p. 20, cites this passage in his characterization of 'particularism', below.

[62] Both problematic retrospective assessments in light of section 3.

[63] I say 'approximation', because Stanford's radical particularist also holds that 'there is not now nor was there ever any point' in generalizing about, e.g., confirmation, or in examining the record of scientific success in the historical record (Stanford [202?], pp. 18-19). The former might be useful in some ways without being normative, and the latter is presumably part of what colors Perrin*'s Uniformitarianism and his fallibilistic sense of the riskiness of scientific theorizing. Stanford also describes a 'modest particularist', but I use the term without adjective to mean the radical version.

> The particularist thinks the very best we can do in deciding whether some particular claim or commitment is true … [[64]] is to carefully evaluate the details of the specific evidence we have for and against that particular claim or commitment. That is, she thinks that the delicate and painstaking *scientific* work of evaluating particular claims and commitments *already* represents our most sophisticated efforts to determine the appropriate level of confidence we should have in particular claims about what things exist and how they interact. (Stanford [202?], p. 18)

This figure, it turns out, is rejected by both middle path realists and middle path instrumentalists:

> middle path realists and instrumentalists are … united … in contrast … to the … expectations of those who defend radical forms of what we might call 'particularism'. (Stanford [202?], p. 17, see also p. 22)

Strikingly, this united front views the particularist, not as someone to be confronted, but as someone irrelevant to the discussion, because

> Since its inception … , the modern realism debate has been predicated on the assumption that there is some *point* to ascending to levels of abstraction at which we generalize about 'mature scientific theories' and their 'empirical successes' or 'approximate truth'. (Ibid., pp. 17-18)

The philosophical debate over realism is concerned with 'broad reflections on the scientific enterprise as a whole or patterns in the historical record' (ibid., p. 19), not with the details or status of Perrin*'s evidential innovations. In other words, the ground-level debate of *Exceeding Our Grasp*, as the particularist understands it, is just beside the point.

I think it's no longer possible to avoid the conclusion that there are, on Stanford's account, two different

[64] The excised phrase, 'and/or will be retained and ratified throughout the course of further inquiry', includes the predictive element we're avoiding whenever possible.

realism/instrumentalism debates. The first is typified by a ground-level dispute – between the middle path instrumentalist and a middle path realist, or between the middle path instrumentalist and her own conscience – for example, over the efficacy of Perrin*'s evidence in the case of atomic theory: should the line between the real and the merely instrumental be moved? This is a particularist debate, to be settled 'on evidence or other considerations specific to the case in question' (Stanford [202?], p. 15). The second is a more general or 'abstract' exchange between the middle path instrumentalist and the middle path selective realist over whether there's a general criterion for picking out the confirmed parts in any scientific theory, or perhaps over the rationality of some brand of theoretical conservativeness. The important point for our purposes is that the latter – the general, 'abstract' debate – is what figures in 'the modern realism debate', and the former – the particularist debate – does not. (Stanford's initial identification of the instrumentalist attitude with that of the realist toward, say, quantum mechanics, would seem to involve a conflation of these two distinct disputes.)

So, in the end, we see that Stanford, like van Fraassen, simply doesn't engage Perrin* on the subject of atomic theory – or by extension, the Second Philosopher, either, with her cloud chambers and electron microscopes. For van Fraassen, Perrin* is a benighted Native, who persists in thinking that ordinary scientific evidence is relevant to the debate; for Stanford, he's a middle path instrumentalist struggling with a decision over where to draw the line between what he takes instrumentally and what he takes realistically, not an entrant in the more general, properly philosophical debate. Stanford is no transcendentalist – as an integrative naturalist, his conclusions are based on the historical record, not philosophical empiricism – but he, too, insists that Perrin* isn't functioning at the appropriate level. For van Fraassen, the appropriate level is the seminar room, separate from ordinary scientific inquiry; for Stanford, it's this higher level of generality or abstraction, still within a broadly empirical inquiry.[65] But either way, they both insist that believing in atoms isn't enough to qualify one as a 'realist' in the relevant sense. So, despite appearances, at least according to these two influential practitioners, the Second Philosopher never entered the realism/instrumentalism debate in the first place!

But labels and partisanship aside, I think this discussion has helped illuminate the nature both of Stanford's contribution and of our disagreement. By calling attention to his problem of unconceived alternatives in theoretical science, Stanford has shifted attention from the pessimistic induction and the status of unobservables to the shortcomings of purely abductive evidence. This is the same concern I raised for the special case of atomic theory in my own response to indispensability arguments and Quinean holism in the philosophy of mathematics, and I think the cases of Perrin, Einstein, and many others demonstrate that it's a concern shared by scientists themselves: it's not enough for atomic theory to explain and predict;

[65] Van Fraassen's seminar-room Empiricism is a stance, presumably freely chosen. Stanford seems to base his preference for the higher level of abstraction on tradition.

we demand some kind of direct contact with its subject matter.[66] The trick for us as methodologists is to examine what this 'some kind of direct contact' comes to and why it's a rational basis for belief. Stanford ([2010]) has done something analogous for the case of fossil evidence (we have access to the process of fossilization); I've made some halting steps on doing so for the case of Perrin's evidence (we detect the random walk).

So again, labels and partisanship aside, where do we two integrative naturalists ultimately differ? We agree that abductive evidence isn't enough by itself to rationally compel belief; we agree that determining where the line falls between what is and isn't fully confirmed (and thus rationally believed) is to be undertaken on a case-by-case basis; we agree that the relevant evidence in each particular case is specific to that case; and we agree it's unlikely that there's a general, normative criterion for what's confirmed in this way. Where we disagree is on that old ground-level problem - is Perrin*'s evidence good enough? – but Stanford takes the truly 'philosophical' disagreement to be the one between the two of us and those who think there is a general, normative criterion. So in the end – setting aside local disputes over the efficacy of specific evidence in particular cases – it seems we disagree only on where philosophers' efforts are most fruitfully deployed. Stanford remains focused on opposing those offering candidate criteria; I believe the real task is the methodological one of examining what has or might

[66] Again, as in section 3, historicism needn't come into it.

work in those local disputes and why. Could it be that, once the rhetoric is cast aside, the real difference between us is no more than one of emphasis?[67]

[67] Warm thanks to my study group – Adam Chin, Alysha Kassam, Charles Leitz, Stella Moon, Evan Sommers, and especially Christopher Mitsch and Jeffrey Schatz – and to Anjan Chakravartty for their most helpful reactions to an early draft. I'm also indebted to David Malament and Mitsch, again, for help with the physics. Finally, Stanford was good enough to read the penultimate draft and correct some of my misunderstandings of his position; I'm grateful to him and regret those misunderstandings that inevitably remain.