

## Chapter 4

### Reading Nature

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### Realist, Instrumentalist, and Quietist Interpretations of Scientific

#### Theories

P. Kyle Stanford

#### 1. Preliminary Reconnaissance: Realism, Instrumentalism, and Interpretation

Questions concerning how we are to interpret our best scientific theories, make sense of

what they are telling us, or even just connect them systematically to the world around us,

have a remarkably long intellectual pedigree.<sup>1</sup> And they have most often been motivated not by the sorts of rarified puzzles we encounter in the course of trying to decide how we could even *possibly* understand claims about, say, superpositions or complementarity in

quantum mechanics, but instead by concerns about whether we are really entitled to

believe what our theories say if we interpret them in what otherwise strikes us as the most

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[ My thanks to Aldo Antonelli; Jeff Barrett; Ludwig Fahrbach; Arthur Fine; Yoichi

Ishida; Pen Maddy; David Malament; Mark Newman; Michael Poulin; Jim

Weatherall;]au: OK to alphabetize the names? OK] students in graduate seminars at

the University of Pittsburgh Department of History and Philosophy of Science and the

University of California, Irvine, Department of Logic and Philosophy of Science;]au:

correct?PKS: yes] and many others I have inexcusably forgotten for useful discussion

and suggestions regarding the material in this chapter.

literal, natural, or straightforward manner. Before the relatively recent professionalization of academic fields, such concerns were well-represented among the figures who served

simultaneously as the leading practitioners and philosophers of science, a fact nicely illustrated by the strident debates throughout this community in the eighteenth and nineteenth centuries concerning whether only pure inductive methods were legitimate for scientific inquiry and/or whether the competing “method of hypothesis” could produce any genuine knowledge of nature (see Laudan 1981, chap. 8).

Having more or less settled that scientific inquiry really does proceed in large part by proposing theories or hypotheses about the entities, events, and dynamical principles at work in otherwise inaccessible domains of nature, the position known as scientific realism has often seemed the most natural or straightforward approach to interpreting those hypotheses. The scientific realist is a forthright and commonsensical soul who tells us that our best scientific theories simply offer (at least probably and/or approximately) true descriptions of how things stand in the world, including most contentiously those domains or aspects of the world about which it is extremely difficult to get information in any other way because the events and entities in them are extremely small, unavoidably remote, occurred in the distant past, or are otherwise inconveniently situated. But the realist also typically holds a distinctive position concerning how the truth of those

theories is to be understood. Her “correspondence theory” of truth holds that the claims of scientific theories or hypotheses are true when the propositions they express

“correspond to” or mirror the way the world or the natural order really is. Thus, she suggests, the claims of our best physical chemistry about negatively charged electrons

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orbiting atomic nuclei should be understood to be just like claims about the cherries in a cherry pie, although they are admittedly much more difficult to confirm or refute.

In the hands of thinkers like Richard Boyd and the early Hilary Putnam, belief in such correspondence truth for at least our best scientific theories was touted as part of the only naturalistic or scientific explanation of the success of the scientific enterprise itself. More recently, Philip Kitcher (2001) has emphasized the fundamental continuity of this realist line of thinking about scientific theories with inferences about success, truth, and correspondence to the world that we draw in more familiar and everyday contexts.

Kitcher's "Galilean strategy" argues that whenever the guides to the world (e.g., subway maps) we are using allow us to attain the kind of fine-grained and systematic success provided by many of our best contemporary scientific theories, we routinely and unproblematically infer that they do so because they reflect, or mirror, or correspond to the way the world actually is: that the order of the subway stations in the world really does follow the order in which they appear on the map, for instance. Kitcher points out that we have no trouble making sense of the correspondence metaphor in such contexts. And he argues that we have no compelling specific reason to doubt the effectiveness of this general "success-to-truth" inference in the case of theoretical science, just as Galileo's opponents could offer no compelling specific reason to doubt that his telescope

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was any less reliable and effective when pointed toward the heavens than he and others had demonstrated it to be in innumerable independently verifiable terrestrial applications like reading the writing on distant buildings or identifying the people on far-off ships coming into port.

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The scientific realist, then, holds a distinctive combination of views about our best

scientific theories. Such theories are to be understood as offering straightforward descriptions that correspond to how things stand in the world itself, and so understood, we are (at least typically) justified in believing what they say.<sup>2</sup> On this account, scientific theories are like fascinating travelogues about distant and exotic places: a realist will interpret the claim that electrons carry a negative charge or that we share a distant common ancestor with spiders and elephants simply as telling us how things stand in

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We might instead describe scientific realism in a broader fashion that makes room for a deflationary conception of truth as well. That is, one might suggest that the realist is really committed simply to the following conjunction of views: that the claims of theoretical science should be interpreted in just the same way that more everyday claims about tables, chairs, and cats on mats are, and that (so interpreted) we have sufficient reason to believe them. Thus, although deflationism about truth seems more naturally mated with quietism about the interpretive status of scientific theories (see below), it may be this broad interpretation of realism which invites the accusation that quietists are just realists in disguise. It is ironic, then, that because so-called internal realists explicitly abandon the correspondence conception of truth in favor of a coherentist or pragmatist alternative (while maintaining that we have good reasons to believe the claims of scientific theories to be true under such a pragmatist conception), such internal realism is widely regarded as not really being a form of scientific realism at all. Of course, the labels are unimportant, so long as we are clear both on what epistemic attitude we think we are justified in taking toward the claims to truth of our successful scientific theories and under what conception of their truth this attitude is warranted.

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parts or aspects of the world about which it is otherwise quite difficult to get information.

In just this same way, a realist will interpret the quantum mechanical claim that particles in a given experimental setup exist in a superposition until a measurement collapse forces them to assume determinate spatial positions as a straightforward report of how things stand in nature itself. Variants of the realist position also come in such flavors as the “structural” realism of John Worrall and the “entity” realism independently defended by Ian Hacking and Nancy Cartwright, which advocate taking this realist attitude only toward particular aspects or parts of our best scientific theories, such as the structure they ascribe to nature or the entities they recognize.

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The realist stands most directly opposed by the members of a long-standing minority tradition I will simply call “instrumentalism.” The instrumentalist doubts whether simply reporting the facts about otherwise inaccessible domains or aspects of nature is what even our most successful scientific theories actually do. But such doubt can originate in more than one way, and the grounds for the instrumentalist’s caution are as diverse as the challenges facing the realist position itself. These range from the relatively narrow appeal of a phenomenalist conception of the world or a verificationist conception of the meanings of our terms quite generally to the comparatively wider influence of the so-called pessimistic induction over the history of science and the underdetermination of theories by evidence. The pessimistic induction points out that even the most successful scientific theories of the past have ultimately proved to be mistaken, and it suggests that we have no convincing reason to expect the ultimate fates of our own theories (which are successful in just the same ways, even if sometimes to a greater extent or degree) to be any different. The problem of underdetermination suggests

instead that there is always more than one available theory capable of accommodating

any finite collection of empirical data, and thus that we are never in a position to confirm

just a single theory over its competition. More recently, I have argued that the most

important challenge for scientific realism arises from what I call the problem of

unconceived alternatives (Stanford 2006). I suggest that the historical record reveals our

repeated failure to even *conceive* of scientifically serious alternatives to the best scientific

theory concerning a given natural domain proposed at a given time even when such

alternatives were also well confirmed on the whole by the available evidence, and thus

that we have every reason to believe that similarly serious, promising, and well-

confirmed alternatives to our own scientific theories presently exist unconceived by us.

This provides some sense of the range of considerations that have motivated

instrumentalists to explore general alternatives to the realist conception of scientific

theories. Of course, resistance can also arise from more local sources, such as the

suggestion that we are simply unable to make any literal sense out of the sorts of claims

about superpositions and measurement collapses given above, and thus that they are not

even candidates for “corresponding with reality.” But such local grounds for resisting

realism serve simply to motivate interest in applying (or reinventing) in the case of some

particular scientific theory one or another alternative approach to the interpretation of

scientific theories that instrumentalists have sought to develop more generally.

One traditional strand of such instrumentalist thinking has challenged whether

scientific theories should be understood as making the claims that the scientific realist

sees them as making in the first place. Such semantic instrumentalists suggest either that

truth itself should not be conceived of in the realist’s preferred manner (whether for the

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claims of theoretical science or quite generally) or that scientific claims require some special semantic understanding or analysis. The other influential strand of instrumentalist thinking has concentrated its dissent instead on the realist's view of our epistemic entitlements: it does not fault the realist interpretation of the claims of our scientific theories, but argues instead that we are not justified in actually believing those claims to be true. Such epistemic instrumentalists have sought to articulate an alternative cognitive attitude toward successful scientific theories to which they think we can indeed be epistemically entitled. What unifies these two strands of instrumentalist thinking is their common commitment to an alternative positive construal of scientific theories as mere conceptual tools or "instruments" for achieving our practical goals and objectives, rather than descriptions of nature that are either probably or approximately true in the realist's correspondence sense. It is noteworthy that the phrase "the instrumentalist interpretation of scientific theories" is sometimes used to refer to just one of these alternatives, is sometimes ambiguous between them, and is sometimes used to refer to them jointly.

Of course, within the semantic strand of instrumentalism there is an important distinction between those who think that a special analysis or understanding of truth is only required for scientific claims (perhaps along with some other specific parts of our language) and those who think that some form of instrumentalist conception of truth is the right analysis for any and all claims whatsoever. The latter group is the inheritor of the intellectual legacy of American pragmatism (it was, after all, John Dewey who coined the term "instrumentalism"), with all of the attractions and challenges involved in its embrace of a distinctively pragmatic conception of truth: essentially that the truth about the world just is whatever beliefs about it we will (or idealized agents would) come to

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accept in the limit of inquiry ([C. S. Peirce](#)) or the beliefs about the world that experience would never encourage us to give up were we to adopt them now ([William James](#)). In the philosophy of science, this position is probably best captured by the “internal realism” variously defended by (a later version of) Putnam and by Brian Ellis. But this version of the instrumentalist position is perhaps the least widely discussed among philosophers of science, in large part because it allows that the truth of scientific claims (though perhaps “merely instrumental” in the requisite sense) is nonetheless perfectly on a par with the truth of all of our other kinds of claims about the world. Thus, there is no *special* challenge or concern here about the truth of scientific theories or claims: the further philosophical work to be done concerns the nature of truth in general, and thus neither raises issues of special significance to nor engages the particular expertise of most philosophers of science.

The quite different view that the claims of theoretical science require a distinctive kind of semantic reinterpretation or translation not shared by all claims whatsoever was perhaps most influentially pursued in the modern era by a number of the logical positivists, including Percy Bridgman and the early Rudolph Carnap. Initially, this took the form of arguing that the very meaning of the claims of theoretical science is exhausted by whatever implications they have concerning observations or observation statements. But the determined efforts of these thinkers (most notably Carnap) to effect a general reduction of scientific language to such a privileged phenomenological or observational basis ultimately foundered, and this reductionist project was eventually given up for lost even by some of its most important original architects (including Carnap himself). Some later positivists and logical empiricists would preserve the spirit of this

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broadly semantic approach, however, while giving up these reductionistic ambitions and adopting instead one of two influential views about the semantics of the claims of theoretical science. According to the first, such claims are devoid of *any* semantic content beyond the license they provide to draw inferences between observable states of affairs:

they are not assertions about the world at all (and therefore do not have truth conditions),  
but are simply “inference tickets” from one observable state of affairs to another. On the  
second view, scientific language making putative reference to theoretical or unobservable  
entities or states of affairs is both genuinely assertoric and irreducible, but nonetheless can  
in principle simply be eliminated from any given scientific theory altogether, producing a  
functionally equivalent linguistic apparatus that makes claims only about observable  
matters of fact. William Craig formulated and proved an influential theorem widely taken  
to show that such a transformation could in principle be made for any restricted part of  
the vocabulary of any (recursively axiomatized first-order) theory that was mutually  
exclusive of and mutually exhaustive with the rest of the (nonlogical) language of that  
theory. A fortiori, as Carl Hempel pointed out in connection with his famous  
“theoretician’s dilemma,” this could be done for a (mutually exclusive and exhaustive)  
partition of a theory’s language into “theoretical” and “observational” components,  
guaranteeing the eliminability of whatever part of our scientific description of some part  
of the world traffics in unobservable entities, events, and processes while leaving the  
theory’s observational consequences unchanged.

Even at the time, however, this claim of in principle eliminability was criticized  
as being of little genuine significance. Ernst Nagel (1961, 136–137) pointed out, for  
instance, that the axioms of the transformed theory would be infinite in number and

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correspond one-to-one with all the true statements expressible using the restricted observational vocabulary (rendering them “quite valueless for the purposes of scientific inquiry”), and that the transformation could only proceed after one knows (in advance of any deductions from them) all the true statements in the restricted observational language. In the intervening decades philosophers of science have only found more reasons to resist the philosophical as well as the practical significance of such Craig’s theorem-style eliminability. Perhaps most important here are the profound differences that have emerged between the sorts of artificial formal systems to which tools like Craig’s theorem apply and real scientific theories, which turn out not to admit of any neat division into strictly theoretical and observational vocabulary after all and often not to be especially helpfully understood as axiomatic formal systems in any case. Likewise, philosophers of science have found little to encourage them to adopt the view that the claims of theoretical science are not genuinely assertoric and many reasons to be skeptical of this counterintuitive analysis of the semantics of these apparently descriptive and truth-valued claims. Neither the “inference-ticket” view of scientific theories nor the claim of in principle eliminability has turned out to have much to recommend it as an approach to interpreting or understanding scientific claims beyond the hope of making the instrumentalist position defensible, and this now strikes even most instrumentalists as too high a price to pay for the privilege.

Perhaps understandably, then, in the decades since the heyday of logical empiricism philosophers of science have become increasingly and systematically skeptical of the idea that instrumentalism should be implemented by way of a special understanding of the truth, meaning, or semantic status of the claims of theoretical

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science. But the declining fortunes of this first, semantic approach to instrumentalism have been accompanied by a substantial revival of interest in the second, epistemic approach. This epistemic strand of instrumentalist thinking does not construe the truth or meaning of scientific claims in a special way, but instead simply denies that we have sufficient justification for believing that even the central claims of even our best scientific theories are in fact true.<sup>3</sup> Instrumentalists of this distinctively epistemic variety have therefore sought to identify some cognitive attitude we might take toward the claims of our scientific theories besides simply believing the descriptions they seem to offer of otherwise inaccessible domains of nature. Some influential recent versions of this approach have recommended that we believe only the implications our theories have concerning observable matters of fact (Bas van Fraassen's constructive empiricism) or believe only that our theories are effective tools for solving the various empirical and

<sup>3</sup> Note that such an instrumentalist need not accept the realist's correspondence

conception of truth. She might simply allow, for example, that the semantics and truth conditions for the claims of theoretical science are on a par with those of more familiar claims, whether the latter are understood in terms of a pragmatic, deflationary, correspondence, or some other conception of truth. Of course, this creates room to endorse a distinctive semantics for theoretical (or other scientific) claims *as well as* the view that we ought not believe the central claims of even our best scientific theories even when they are so understood. But this is surely as it should be. Notice, for example, that an internal realist or pragmatic conception of truth will not draw the sting from the pessimistic induction, which concerns the *transient* character of even the most successful past scientific theories, no matter how their truth is understood.

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theoretical problems we face (Larry Laudan's problem-solving model of scientific progress).

## 2. Losing Sight of a Distinction

Among the problems facing any attempt to formulate and defend instrumentalism, perhaps most fundamental and interesting of all is a deep and persistent line of argument to the effect that the intended contrast between interpreting scientific theories literally, realistically, or at face value, on the one hand, and as mere instruments, on the other, cannot ultimately be made out in any coherent way. Nagel famously posed a version of this challenge against the distinctively semantic variety of instrumentalism popular among his contemporaries. He argued that there was a "merely verbal difference" between the instrumentalist's contention that a given theory offers satisfactory techniques of inference or a reliable "inference ticket" from some observable states to others and the realist's contention that the theory is simply true (Nagel 1961, 139). But much the same challenge continues to be posed to the more recently influential and distinctively epistemic varieties of instrumentalism that instead question whether we are justified in believing the descriptions of the world offered by our scientific theories to be true. Paul Horwich (1991), for example, points out that some accounts of belief simply identify it as the mental state responsible for use, and suggests that epistemic instrumentalists will have to show why such accounts are mistaken before they are entitled to conclude that their position is any different from that of their putative realist opponents. Howard Stein (1989) offers a much more detailed argument to the effect that, once both realism and epistemic instrumentalism are sophisticated in ways that are independently required in any case, there remains no important difference between them. Realism must abandon, he

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argues, any pretensions to metaphysically transcendent theorizing or noumenal truth and

reference, and must also give up the idea that any property of a theory might somehow explain its success in a way that does not simply point out the use that has been made of the theory itself. Instrumentalism, by contrast, must enrich the conception it holds of the functioning of a theory *as an instrument* to include not only calculating experimental

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outcomes, but also adequately representing phenomena in detail across the entire domain of nature to which it can be fruitfully applied and (perhaps most importantly of all) serving as the foundation, resource, and starting point for all further inquiry into that domain. Once the realist's ambitions for our scientific theories have been appropriately restricted and the instrumentalist's ambitions for them appropriately expanded in this way, Stein argues, there is simply no difference that makes a difference left between the two positions, and he suggests that the twin aspects of the resulting view of theories have always been present simultaneously in the deepest scientific work in any case.<sup>4</sup>

The suggestion that there is no room for a distinction between realist and instrumentalist attitudes toward scientific theories has been developed in perhaps the greatest detail, however, by Simon Blackburn (1984, 2002). Blackburn's representative instrumentalist (van Fraassen) urges that we not believe scientific theories but instead merely "accept" them as "empirically adequate"; that is, we are to believe what they say about observable phenomena while remaining agnostic about their further claims concerning unobservables. But as Blackburn (2002, 117–119) notes, the character of the

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<sup>4</sup> This last suggestion is a close relative of one defended in much greater detail by Larry Sklar (2000).

acceptance van Fraassen recommends rightly involves a great deal more than simply using the theory to calculate predictions concerning observables:

The constructive empiricist is of course entirely in favor of scientific theorising. It is the essential method of reducing phenomena to order, producing fertile models, and doing all the things that science does. So we are counselled to *immerse* ourselves in successful theory. . . . Immersion will include acceptance as empirically adequate, but it includes other things as well. In particular it includes having one's dispositions and strategies of exploration, one's space of what it is easy to foresee and what difficult, all shaped by the concepts of the theory. It is learning to speak the theory as a native language, and using it to structure one's perceptions and expectations. It is the possession of habits of entry into the theoretical vocabulary, of manipulation of its sentences in making inferences, and of exiting to empirical prediction and control. Van Fraassen is quite explicit that all of this is absolutely legitimate, and indeed that the enormous empirical adequacy of science is an excellent argument for learning its language like a native. . . . Immersion, then, is belief in empirical adequacy plus what we can call being "functionally organized" in terms of a theory.

Like Stein, Blackburn here insists that instrumentalists recognize the full range of useful instrumental functions that our scientific theories perform. And once this recognition is complete, he suggests, there remains no room for a distinction between the

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thoroughgoing “immersion” in or “animation” by our theories that instrumentalism recommends and realism itself:

The problem is that there is simply no difference between, for example, on the one hand being animated by the kinetic theory of gases, confidently expecting events to fall out in the light of its predictions, using it as a point of reference in predicting and controlling the future, and on the other hand believing that gases are composed of moving molecules. There is no difference between being animated by a theory according to which there once existed living trilobites and believing that there once existed living

trilobites<sup>5</sup> . . . What can we do but disdain the fake modesty: “I don’t really

believe in trilobites; it is just that I structure all my thoughts about the

fossil record by accepting that they existed”? (Blackburn 2002, 127–128)

These passages frame the problem in perhaps its starker form. As the

instrumentalist strengthens “commitment,” “immersion,” or “acceptance” to capture all

of the various respects in which we rely on our theories instrumentally, she seems to

simultaneously eradicate any distinction between such “merely” instrumental reliance

and belief itself. And she thus winds up hard-pressed to say what could distinguish a

commitment to the *robust* instrumental utility of our best scientific theories from simply

believing those same theories to be true.<sup>5</sup>

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<sup>5</sup> One note of caution is in order here. Van Fraassen sometimes seems to suggest that it is only practicing *scientists* who must “immerse” themselves in the theories they work with, while philosophical interpreters of scientific activity (presumably including scientists when they act in this role) are free to rest content with mere belief in the empirical

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According to Blackburn, it is no accident that we make the mistake of thinking that there must be a well-posed distinction between realism and instrumentalism, for there are genuine differences in the character or degree of acceptance of a theory in actual scientific contexts to which the supposed divide between realists and instrumentalists might seem closely related. But he argues that closer scrutiny reveals these apparent similarities to be specious.

It is natural, for instance, to contrast the empirical adequacy of a theory to date with its final and total empirical adequacy. This contrast is itself perfectly legitimate, but it cannot mark any part of the difference between van Fraassen's instrumentalist and her

realist opponent, Blackburn suggests, because the fully immersed instrumentalist is no

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adequacy of those same theories. Whatever merits such a two-tiered view of epistemic commitment might or might not have, Blackburn's challenge retains its bite, for van

Fraassen ([1980, 73](#)) argues that his constructive empiricism "makes better sense of science, and of scientific activity" than the realist alternative, and this claim is rendered suspect if the "immersion" he is forced to recommend to scientists is itself

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indistinguishable from belief (i.e., requires from practicing scientists self-delusion on a massive scale). Perhaps partly for this reason, van Fraassen ([1980, 81](#)) argues explicitly that even the working scientist's "immersion in the theoretical world-picture does not

preclude 'bracketing' its ontological implications." Furthermore (and independently), many of the features of immersion to which Blackburn and Stein draw our attention (e.g.,

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the propriety of using the theory as the foundation for our future inquiry in a given scientific domain) are those about which the philosophical interpreter of science cannot afford to remain agnostic in any case.

less committed to a theory's continuing adequacy into the future (i.e., the *total* empirical adequacy of the theory) than is the realist. After all, a commitment to the empirical adequacy of a theory is a commitment to the truth of what it says about observable phenomena, full stop, whether in the past, present, or future, and whether actually observed or not. Of course, in some particular case the instrumentalist might have special reasons for doubting that a theory that has been empirically adequate to date or over some restricted range will continue to be so, but this is a difference of epistemic commitment drawn from *within* the instrumentalist position and thus cannot constitute the difference between such instrumentalism and the realist alternative. And Blackburn's point here would seem to apply equally well to Laudan's formulation of the instrumentalist commitment to the *ongoing* or *continued* ability of a theory to solve empirical and theoretical problems.

In a similar vein, Blackburn notes that we can legitimately contrast full and unreserved acceptance of a theory with embracing it in a more cautious or tentative spirit, but argues that this distinction is also inapposite: van Fraassen's fully immersed instrumentalist embraces the empirical adequacy of a scientific theory with no less confidence or conviction and in no more tentative a spirit than the realist embraces the truth of that same theory. As before, there may be room for greater and lesser degrees of confidence or commitment with regard to particular theories, but once again this potential variation will have to be recognized from *within* the instrumentalist position in a manner precisely parallel to the varying degrees of confidence a realist might have in the truth of a theory, and so it cannot mark the distinction between the two positions. And again the

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same point applies in just the same way to the Laudanian instrumentalist's confidence in a theory's ability to solve our empirical and conceptual problems.

I will here set aside what Blackburn cites as a third possible source of spurious plausibility for the distinction between realism and instrumentalism: the contrast between believing a theory and accepting that theory "in an instrumentalist frame of mind"

([Blackburn 2002](#), 121) while "bracketing it's ontological implications" ([van Fraassen 1980](#), 81; quoted in [Blackburn 2002](#), 117) because we see the theory as violating norms of consistency or appropriate description (quantum mechanics is perhaps the paradigmatic example here). He notes that this cannot be the distinction van Fraassen has in mind, because the latter's constructive empiricist does not limit his recommendation of mere acceptance rather than belief to theories facing special challenges of this sort (and again, we might note, the same is true for Laudan). I suspect that Blackburn does his own argument a disservice here, for the central question he raised was whether the distinction between belief and mere acceptance could be made out at all, not where we could

legitimately invoke it. If he really means to recognize a difference between genuine belief and such acceptance with ontological "bracketing" or in an "instrumentalist frame of mind" in these cases, this looks like a plausible candidate distinction between realism and instrumentalism quite generally: the remaining issue would not be whether there was a

distinction van Fraassen could [make use of](#), but whether he had sufficient reason to consign so many theories to the instrumentalist side of it. (Notice that, unlike the other two distinctions he considers, Blackburn does not show how this distinction is itself one that van Fraassen will need to be able to draw as a difference in degree of commitment to a theory from *within* the instrumentalist position in any case.) To put the matter another

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way, although Blackburn is right to say that the distinction between theories that face

special problems of norm violation and those that do not cannot be the distinction van

Fraassen has in mind, it simply does not follow that the distinction between belief and

acceptance in an instrumentalist frame of mind (while bracketing ontological

implications), if recognized as genuine, cannot be the distinction van Fraassen has in

mind.

However this last issue may stand, once any spurious sources of intuitive plausibility for the distinction between realism and instrumentalism are cleared away, the

instrumentalist is left with a vexing problem. She seems to owe us some account of how

an instrumentalist attitude toward our theories that is powerful and comprehensive

enough to capture and support our ordinary scientific practices will not be simply

indistinguishable from belief in those same theories. And as we will see in the next

section, we will be forced to decide whether we think there is any genuine distinction

between the realist and instrumentalist positions even if we ultimately decide to reject

both in favor of a recently influential alternative that we might describe simply as

“quietism” about the interpretation of scientific theories. We turn now to an exploration

of this quietist position, which seeks to move beyond both realism and the various forms

of instrumentalism by giving up their shared sense that scientific claims and theories

stand in need of any kind of “interpretation” or further philosophical analysis at all.

### 3. Keeping Quiet

The quietist insists not only that our scientific theories do not need to be “interpreted” in

the first place, but also that the very enterprise of trying to do so is misguided: like most

of the claims we make about the world, she tells us, scientific theories mean just what

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they say, no more and no less, with no need for a special metatheoretical interpretive  
stance or account that translates them for us or tells us in more philosophically rigorous  
terms what they “really” mean or amount to at the end of the day. This position is

represented most influentially by Arthur Fine’s Natural Ontological Attitude (NOA,  
pronounced “Noah”), but also and more recently by Blackburn (2002) as well. Fine  
(1986a, 128) begins by identifying a set of commitments that he suggests both the realist  
and the instrumentalist (called “the antirealist” in the quotation below) hold in common:

It seems to me that both the realist and the antirealist must toe what I have  
been calling ‘the homely line.’ That is, they must both accept the certified  
results of science as on par with more homely and familiar supported  
claims. . . . Let us say, then, that both realist and antirealist accept the  
results of scientific investigations as ‘true,’ on par with more homely  
truths. . . . And call this acceptance of scientific truths the ‘core position.’

What distinguishes realists from antirealists, then, is what they add onto  
this core position. (footnote omitted)

That is, both realists and instrumentalists accept the truths that emerge from  
scientific inquiry as well-established (just like more familiar claims), but realists do this  
by way of their correspondence conception of truth, while instrumentalists do so either by  
way of reconceiving what such truth amounts to (whether in general or just for scientific  
claims) or in virtue of their willingness to “accept” theories, or “use” them  
instrumentally, or treat them as “empirically adequate” in a perfectly thoroughgoing way  
while hastening to remind us that they do not *really* believe what they take those theories  
to say. After embracing this core position, both realists and instrumentalists feel the need

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to supplement it in their various distinctive ways: either with a foot-stamping shout of “really,” or with a special analysis of what the truth of scientific claims consists in, or

with the admonition that we are simply not to *understand* our (merely instrumental, though complete) acceptance of scientific truths in the same way that we understand our acceptance of the more homely truths of our everyday lives. By contrast, Fine suggests that the core position itself is all that we really need, and he recommends that we simply

embrace it without any of these further, extrascientific commitments. In the process we

should dispense with the idea that scientific theories themselves stand in need of any special philosophical interpretation or analysis: “What binds realism and antirealism

together,” he says, is that “[t]hey see science as a set of practices in need of an interpretation, and they see themselves as providing just the right interpretation. But

science is not needy in this way” (Fine 1986a, 147–148).

As even the name he gives to his own position suggests, Fine denies that scientific realism is somehow the obvious default or “natural” attitude to take toward the claims of our scientific theories. Only a perverse felt need to provide scientific inquiry with some kind of external authentication or grounding, he suggests, would lead us to append to it a special philosophical theory of what such truth amounts to and declare this to be its most natural “interpretation.” He argues that it is surely much *more* natural simply to recognize

that science gives us claims that are true in the homely way that NOA allows above, but

not insist on any further specification or analysis or interpretation of what such truth really amounts to in the end, just as (he suggests) we decline to do with the more familiar

claims we rely on in the course of our ordinary lives. Thus, Fine (1986b, 177) suggests,

the “naturalness” of NOA is “the ‘California natural’—no additives, please!”

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Indeed, a crucial aspect of Fine's view is its stubborn refusal to provide or ratify

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any general account of what "truth" is. He tells us that NOA instead "holds a 'no-theory' conception of truth" (Fine 1986b, 175), in large part because it denies that truth is any one

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thing: "Of course we are all committed to there being some kind of truth. But need we take that to be something like a 'natural' kind? This essentialist idea is what makes the cycle [from realism to instrumentalism and back again] run, and we can stop it if we stop conceiving of truth as a substantial something—something for which theories, accounts,

or even pictures are appropriate" (Fine 1986a, 142). While NOA accepts our ordinary

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uses of the term "true," it denies "that truth is an explanatory concept, or that there is

some general thing that makes truths true" (Fine 1986b, 175). Fine applies the same

antiessentialism to the very idea that the processes or products of scientific inquiry have a

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general aim or goal, or (therefore) admit of any general interpretation. Of course we

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certainly recognize goals or aims or purposes in scientific contexts, but Fine insists that

these are local in character and internal to the practice of science itself: "For what

purpose is this particular instrument being used, or why use a tungsten filament here

rather than a copper one?" It is simply a gross fallacy in quantifier logic, he notes, to

move from "They all have aims" to "There is an aim they all have" (Fine 1986b, 173).

And when we ask after the goal, or aim, or purpose of science itself, "we find ourselves

in a quandary, just as we do when asked 'What is the purpose of life?' or indeed the

corresponding sort of question for any sufficiently rich and varied practice or institution"

(Fine 1986a, 148). "The quest for a general aim [for science], like the quest for the

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meaning of life, is just hermeneuticism run amok" (Fine 1986b, 174). NOA invites us

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instead to see science as standing on its own bottom and emphatically *not* standing in

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need of any overarching external goal or aim, whether realist or instrumentalist in character. Rather, “NOA suggests that the legitimate features of these additions are already contained in the presumed equal status of everyday truths with scientific ones, and in our accepting them both as *truths*. No other additions are legitimate, and none are required” (Fine 1986a, 133).

In a widely influential discussion, Alan Musgrave (1989) has suggested that Fine does not actually manage to navigate between the realist and instrumentalist commitments he seeks to avoid, precisely because the “core position” Fine claims for NOA as the common foundation for realism and instrumentalism accepts “the results of scientific investigations as ‘true,’ on par with more homely truths” (Fine 1986a, 128) and the “equal status of everyday truths with scientific ones” (Fine 1986a, 133). Musgrave insists that Fine must either understand such truth in the realist’s preferred way, in which case he and NOA become realists after all (the outcome he prefers), or fail to articulate any coherent view (or even a coherent “attitude”?) at all.

The dilemma is sharpened, Musgrave argues, by considering the few things NOA does say about truth. First, Fine (1986a, 130) insists that NOA treats “truth in the usual referential way, so that a sentence (or statement) is true just in case the entities referred to stand in the referred-to relations” and “sanctions ordinary referential semantics, and commits us, via truth, to the existence of the individuals, properties, relations, processes, and so forth referred to by the scientific statements that we accept as true.” In a similar spirit Fine (1986a, 133) goes on to explain that NOA “recognizes in ‘truth’ a concept already in use and agrees to abide by the standard rules of usage. These rules involve a Davidsonian-Tarskian referential semantics, and they support a thoroughly classical logic

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of inference. Thus NOA respects the customary ‘grammar’ of ‘truth’ (and its cognates).”

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So NOA explicitly allows that the statement “There is a full moon tonight” is true if and

only if there is indeed a full moon tonight (Musgrave’s example, 1989 388). But compare

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this with a claim like the following: “The statement ‘George W. Bush gives me the

creeps’ is true if and only if George W. Bush gives me the creeps” (Musgrave’s example,

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updated). We endorse the ordinary referential semantics and rules of usage here only

because we think the statement (even as it appears without internal quotation marks, on

the right hand side) should be understood or interpreted as an idiom that is *not* to be taken

(as Musgrave says) “at face value”: rather than recognizing the existence of an entity or a

group of objects (“the creeps”) that are routinely presented to the speaker by Bush, we

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interpret the statement as saying something like Bush makes the speaker nervous and/or

uncomfortable.

What Musgrave argues is that NOA’s commitment to the idea that scientific

claims are true in just the way that more homely truths are must mean *either* that it uses

ordinary referential semantics *and* accepts the truth of scientific claims when they are

taken at “face value” (as we take the claim about the full moon)<sup>6</sup> or that it really does

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<sup>6</sup> Of course this sense of “face value” must be quite different from the homely acceptance

as true that Fine uses to describe the core position, above, for the latter was explicitly

intended *not* to prejudge the character of our acceptance at all. Musgrave’s sense of

taking a statement at “face value” might be best described as treating the statement’s

fundamental ontological commitments (to entities, events, properties, relations, etc.) in

precisely the manner suggested by its surface grammar. Of course, this particular example

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is a curious one even for Musgrave’s sense of “face value,” as we do not really think the

take *no position* on whether scientific claims are to be taken at face value rather than understood in some idiomatic or nonliteral way (like the creeps that Bush gives me). In the former case, instrumentalists do not actually accept the “core position” Fine describes because they deny that we are justified in holding scientific claims to be true at face value in Musgrave’s sense (either because scientific claims should not be accepted as true at all or because they should be reinterpreted before being accepted as true) *and* by endorsing the “core position” alone. Fine and NOA simply turn out to be realists after all. If instead NOA really means to remain agnostic concerning whether or not scientific claims should be taken at face value in this way, Musgrave insists that there is no *one* core position common to realism and the various forms of instrumentalism but rather many different core positions depending on what sense we give to the view that scientific claims are “true” in just the same way that the homely truths of our everyday lives are.

These different positions enjoy only the *apparent* unity that they can all agree to a common verbal formula, just as Berkeley could still use the same *expressions* that the man in the street would use to say things about the world, even though he meant by them something far removed from what the man in the street meant (or thought he did, in any case) when he used those expressions. And if the “core position” that exhausts NOA is

moon is “full” of anything but rather that a large proportion of its visible surface is illuminated. (Indeed I suspect that this sort of consideration should engender some suspicion about the idea that metaphorical or idiomatic uses of language can be clearly or cleanly separated from literal ones in general, but I will not pursue the matter here.) In any case, I suggest below that Fine would deny that Musgrave’s sense of “face value” was ever the matter at issue.

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based on this sort of simple equivocation, Musgrave argues, then NOA is really just a philosophical know-nothingism: what the NOAer is really saying, with an infuriatingly genial smile, is something like “I am happy to say that electrons orbit atomic nuclei, but I don’t really mean anything in particular by that.” Moreover, as NOA’s “core position” holds that scientific claims are true in just the same way that more homely truths are, it proudly insists on knowing nothing not just about science but about everything else as well.

Of course, something seems profoundly misguided about Musgrave’s analysis—it does not take sufficiently seriously the fact that NOA thinks there is nothing useful to be said in general about how “truth” should be understood. To be sure, there are uses of language whose accepted meaning does not follow their surface grammar, but Fine’s point was that there is nothing (that is, no *one* thing) to say about the truth of claims in general (or even just the truth of scientific claims in general) even *after* they are all “normalized” into a form that can be taken at what Musgrave calls “face value” (e.g., from “George W. Bush gives me the creeps” to “George W. Bush makes me nervous and/or uncomfortable”). NOA insists that truth no more admits of a unitary analysis after such transformations (and for claims that do not need them in the first place) than before. The various things that the truth of various claims might amount to, in science as in everyday living, are multiple and many-splendored: we are guided in what to make of our acceptance (or the “truth”) of a particular scientific claim in a particular context by circumstances specific to it, *and that is all there is to say.*<sup>7</sup> In particular, there is simply

<sup>7</sup> Musgrave endorses Fine’s skepticism about the idea that there is some one thing that makes all truths true, but he nonetheless seems to want to treat scientific claims as an

nothing more that is both useful and general to say about truth itself or about the kind(s) of truth that broad categories of claims (like the claims of science or the claims of everyday living) enjoy. Likewise, Musgrave's taunt of know-nothingism is a little unfair, for NOA insists on knowing nothing at just the point where it also insists that there is *nothing more to know*, at least nothing of the global, philosophical sort Musgrave has in mind.

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Of course, this analysis does help point out an important way in which NOA might be thought to be *unsatisfying*.<sup>8</sup> It requires that we say nothing more at a point where it makes us profoundly uncomfortable to do so. If there is indeed something general (and defensible) to be said about the kind of truth scientific claims (or important categories of

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identifiable *category* whose interpretation and/or truth conditions can be characterized in a uniform way (e.g., like claims about "the moon" or like claims about "the creeps"). But this is to seriously underestimate the sort of heterogeneity about truth that Fine means NOA to recognize. It is not clear to me that this refusal to see the point is not intentional and studied, however, as Musgrave does sometimes seem to be responding to NOA in the same ironic spirit in which Samuel Johnson or G. E. Moore **[au: Please provide full names. Samuel Johnson, George Edward Moore]** responded to skepticism, in this case by pointing out just what he thinks can indeed be said about the truth of such claims that is both general and useful in this connection.

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<sup>8</sup> Indeed, one of Fine's claims is that NOA shows how minimal an adequate philosophy of science can be, and this is where I think Musgrave's argument should really lead him to dissent (as perhaps he recognizes in glossing his accusation of know-nothingism as pointing out just how minimal NOA really is).

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them) have, or the general aims they can reliably achieve, or something similar, we surely

want to be able to say it. Thus, perhaps what we should hesitate to embrace is Fine's  
(1991, 93) suggestion that "the ambiguity over the character of acceptance in science that results from not raising the realism/instrumentalism question seems to be an ambiguity

we can quite well live with" while we "get on with other things." And perhaps this encourages us to regard NOA as a counsel of despair or last resort to be embraced with resignation if and when we finally come to agree that there is nothing both general and defensible to be said in this connection and after exhausting (at least) the realist and

instrumentalist alternatives. Of course, such reluctance and hesitation will seem mysterious to those who follow Fine in finding something "natural" about the attitude

NOA expresses. I myself find it natural to wonder whether there is not something general to be said about the kind (or kinds) of truth that independently identifiable categories of important scientific claims have (just as I do about the more homely and familiar claims of everyday life), and I therefore find NOA's robust minimalism disappointing, but perhaps with sufficient philosophical therapy I could get over it. Until then, whenever

"NOA whispers the thought that maybe we can actually get along without extra attachments to science at all" (Fine 1986b, 172), I will have to simply whisper back, "But I would prefer not to."

This response to Musgrave's analysis helps illuminate another important feature of Fine's position that has invited criticism of NOA from a fellow traveler on the quietist path. As Fine's remark about "the ambiguity . . . that results from not raising the realism/instrumentalism question" suggests, NOA recognizes a genuine *distinction* between realist and instrumentalist interpretations of science. Moreover, it even seeks to

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recapture something very like this distinction as a description from within the practice of science itself of the diversity of the various attitudes that the evidence can lead us to take

toward particular scientific claims (or that our acceptance of a scientific truth in the

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"homely" sense can amount to) in particular sets of circumstances: "One NOAer might

even find specific grounds in certain cases for bracketing belief in favour of commitment,

for instance, while another might go for some measure of belief" (Fine 1986b, 176–177).

But Blackburn criticizes NOA on this score—for as we have already seen, he does not

think this distinction can be drawn in a coherent way at all. As he says it, "By allowing

that there is a difference here, albeit one that is invisible in the practice of science, Fine

underestimates the problem. It is not that we acknowledge the difference, but then stop

worrying about it. It is that we have lost sight of a difference to acknowledge" (Blackburn

2002, 128).

Instead of turning our backs on the *interest* or *importance* of the distinction between instrumentalist acceptance and realist belief, Blackburn insists that there is no such distinction to turn our backs on in the first place. And this suggests quite a different form of quietist response to the successes individual scientific theories enjoy:

Suppose my practice is successful: my space rockets land where and when they should. What is the best explanation of this success? I design my rockets on the assumptions that the solar system is heliocentric, *and it is*.

Why is our medicine successful? Because we predicted that the viruses would respond in such-and-such a way, *and they do*. In saying these things we . . . are simply repeating science's own explanation of events. There is

no better one—unless there is a better scientific rival. . . . In other words

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there is no difference between explaining the gas laws in terms of the kinetic theory of gases, and explaining why we do well relying on the gas laws, which has to be done in the same terms. There is no getting *behind* the explanation. (Blackburn 2002, 130)

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Like Fine, Blackburn thinks that there is nothing more to say about the sense or senses in which the accepted claims of science are true. We do not go on, for instance, to say that what we mean in saying that “the solar system is heliocentric” is true is that this claim “corresponds” to the way the world is, or that the success of the heliocentric system demands the truth of this theory as its only plausible explanation, or any such thing. But unlike Fine, he also thinks that after we have accepted science’s own explanation of why a given scientific belief is successful, there is simply nothing more to say, *period*: there is not even any room “behind” such an explanation that we might disdain to occupy by

refusing to take a stand on whether we accept it in a realist or an instrumentalist spirit.<sup>9</sup>

As it turns out, then, we cannot avoid answering the question of whether there is any coherent distinction between realism and instrumentalism to be made out even if we hope to decide no more than what *kind* of quietist to be. Quietists as well as instrumentalists and realists will thus need a convincing answer to this question, as do we all if we are to know what (if anything) is at stake in adopting a realist, instrumentalist, or quietist line on the interpretation of scientific theories. Let us return once again, then, to the putative contrast between realism and instrumentalism and ask whether the latter

<sup>9</sup> Intriguingly, then, while Fine (1986, 175) embraces what he calls “quietism” and remarks that Blackburn (1984) treats the view “dismissively,” Blackburn (2002) ultimately embraces a quietism that is, if anything, even quieter than Fine’s own.

position can be formulated in an independently attractive way that leaves room for an important distinction of the sort that Stein and Blackburn have suggested cannot be coherently drawn.

## 4. Reclaiming Instrumentalism

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The fundamental intuition that there is an important difference between believing a scientific theory and merely using it to achieve our practical goals and objectives is not

far to seek. We routinely use Newtonian mechanics to send rockets to the moon, for example, but no one believes that Newtonian mechanics is even approximately true in the sense that matters for the debate over scientific realism. When it comes to the sorts of fundamental claims about the constitution of nature that the scientific realist thinks must be at least probably and/or approximately true in the case of our best contemporary theories, Newtonian mechanics is acknowledged on all sides to be radically false and yet nonetheless an extremely powerful and reliable tool for predicting the behavior of objects like billiard balls, cannonballs, and planets under a wide (though not unrestricted) range of conditions.

Let us begin, then, by asking what the *realist* means when *she* claims that Newtonian mechanics is a radically false theory that nonetheless serves as a useful instrument. On the one hand, *she* simply rejects the Newtonian account of the constitution of nature: *she* denies that gravity is a force exerted by massive bodies on one another, that space and time are absolute, and so on. But on the other hand, *she* knows perfectly well how a Newtonian would apply the theory to make predictions about and intervene with respect to entities, events, and phenomena *as they are given to her, by other theories that she does believe*. That is, while the relativity theorist has an

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independent account of the cannonballs, inclined planes, and rockets to which she could apply the theoretical mechanics she believes to be literally true, she also knows how a Newtonian would apply her own distinctive theory to characterize those same cannonballs, inclined planes, and rockets in terms of the masses, forces, collisions, and so forth that would allow her to predict and intervene with respect to them, so described. This has nothing to do with the fact that cannonballs, inclined planes, and rockets are observable entities: she knows equally well how a Newtonian would identify forces and masses so as to make predictions about the gravitational motions of subatomic particles.

And over whatever domain she thinks the theory is (more or less) instrumentally reliable she can make use of it, because she knows how to apply it like a Newtonian would to entities, events, and phenomena whose existence she countenances independently of the theory.

It may come as a surprise to note that this same fundamental strategy is also available to the instrumentalist. That is, a scientific theory demonstrates its instrumental utility in application to features or aspects of the world that we take ourselves to have some grasp of or route of epistemic access to that is independent of the theory itself. Thus, in place of the instrumentalist's usual attempt to separate a theory's claims into those we must believe to make effective instrumental use of it (e.g., its claims about observables, or its predictions and recipes for intervention) and those we need not, we might instead allow that an instrumentalist about a given theory simply believes *all* the claims of that theory *as those very claims can be understood independently of any theory or theories toward which she adopts an instrumentalist stance*. Take, for example, the claim that "a liquid supercooled below its standard freezing point will crystallize in the presence of a

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seed crystal or nucleus.” This is a highly theory-laden claim, but it grounds a specific set of implications and expectations concerning entities, events, and phenomena *as we understand them independently of contemporary chemical theory*. We know how to identify freezing points, create supercooled liquids and seed crystals, and recognize crystallization using procedures that could be followed by someone without any substantial knowledge of contemporary chemical theory—including the procedures we would use to exhibit these entities and processes to a neophyte in the laboratory (typically while providing a complementary explanation of what contemporary theoretical orthodoxy holds is going on).

This proposal for regaining instrumentalism is broadly sympathetic in spirit with the suggestion (found, for example, in Kitcher 1978, 1993; Stanford and Kitcher 2000) that the reference of natural kind terms in scientific contexts is fixed quite differently on different occasions of use in ways that are sensitive to the dominant referential intention of the term’s user on that occasion.<sup>10</sup> On some occasions, that is, Joseph Priestly’s tokens of “dephlogisticated air” referred to the substance whose inhalation had made his breathing particularly light and easy afterward or the substance he “exploded together” with “inflammable air” to produce water or nitric acid, while the references of some other tokens of this very same term were instead fixed on the relevant occasion of use by Priestly’s dominant intentions to satisfy the theoretical description “air from which the substance emitted in combustion has been removed” and thus failed to refer at all. Here we simply note further that the dominant referential intention that allowed Priestly’s

<sup>10</sup> For more details [regarding](#) this connection, see Kitcher 1993, esp. chap. 4; and Stanford 2006, chap. 8.

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genuinely referring tokens to do so were those that picked out the referent of “dephlogisticated air” in a way that relied on some further route of independent epistemic access, through his senses or through further beliefs about dephlogisticated air that did not depend in any fundamental way on the commitments of phlogistic chemistry (e.g., “the stuff in this jar,” “the stuff that makes my breathing so light and easy”).

Although this account of the matter has no use for the idea of a pure observation language or a foundational epistemic role for observability as such, it recognizes that our various sensory modalities will be among the routes most commonly used by us to secure an independent epistemic grasp of entities, events, and phenomena described by the theories toward which we adopt instrumentalist attitudes. For such an instrumentalist, a scientist who finds a new way of detecting an entity or phenomenon or its causal influence or of creating it in the lab gives us a new route of epistemic access to that entity or phenomenon that need not and often will not depend on the commitments of the theory that sent us looking for it in the first place.

It is not, however, a grasp or understanding of an entity or phenomenon that relies on the senses alone that instrumentalism of this sort requires; instead it simply requires that we recognize sources of knowledge about entities, events, and phenomena in the world that are independent of any particular theories toward which we adopt an instrumentalist attitude. Moreover, this remains a perfectly coherent possibility even if we ultimately decide that *all* our knowledge of the world is theoretical in character. If we suppose that W. V. O. Quine (1976) was right to suggest that the familiar middle-sized objects of our everyday experience are no less “theoretical” entities hypothesized to make sense of the ongoing stream of experience than are atoms and genes and that it is by

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means of theories in this broadest sense that we come to have *any* picture at all of what our world is like, we will then simply be faced with the need to decide precisely *which* theories we will regard merely as useful conceptual instruments (as well as why all and only those particular theories). To be sure, on this account instrumentalism is not an

attitude that can be coherently adopted toward all theories whatsoever (at least, not if Quine is right), but the example of Newtonian mechanics already illustrates why even scientific realists will need to decide which theories they are instrumentalists about on the

basis of individualized consideration of those theories instead. Even the scientific

instrumentalist will adopt a realist attitude toward a wide variety of hypotheses

concerning what Quine called (e.g., 1976 250) "the bodies of common sense<sup>b</sup>", and the

epistemic features or deficiencies that determine how broadly she will apply the

instrumentalist attitude in other cases will surely depend on what her reasons are for

adopting that attitude toward any theories in the first place.<sup>11</sup>

Of course, if we are instrumentalists about a theory that makes claims about entities or events to which we have no routes of independent epistemic access, our belief

<sup>11</sup> It is this form of instrumentalism for which I tried to provide a principled foundation in Stanford 2006, whose final chapter contains a more detailed development of the sort of

instrumentalist position I have briefly sketched here. Throughout that work I argue that

the crucial feature of any theory that should provoke us to adopt an instrumentalist

attitude toward it is its demonstrated vulnerability to the probable existence of

unconceived alternatives that are also well confirmed by the evidence. I also argued

(perhaps a bit too quickly; see Stanford 2011) that this includes most if not all of our

fundamental scientific theories.

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in those claims “as they can be understood independently of the theory” will be empty.

Because contemporary particle physics does not allow quarks to be isolated, for example, it posits the existence of gluons to bind quarks within a proton, but we have no routes of independent epistemic access to gluons beyond positing them to fulfill this hypothesized function. Those who are instrumentalists about particle physics, then, will not believe *any* of its claims regarding gluons, but will simply make use of them in the process of generating further claims whose content is specified by sources of knowledge (including other theories) toward which she does not adopt an instrumentalist attitude.

Indeed, on this account the fundamental difference between the realist and the instrumentalist turns out *not* to be a matter of their respective epistemic attitudes toward “theories” or theoretical knowledge as such. *Both* realists and instrumentalists regard some theories merely as useful instruments for predicting and intervening with respect to a given natural domain but not probably or approximately true descriptions of the domain in question, and those traditionally described as instrumentalists are simply willing to take this view of a much wider range of theories than their realist counterparts. The remaining difference would seem to consist simply in the fact that the instrumentalist is sometimes willing to regard even an extremely powerful and pragmatically successful theory as no more than a useful instrument *even when she knows of no competing theory that she thinks does indeed represent the truth about the relevant natural domain*. But there seems little reason to think that this difference turns what looks for all the world like a clear and coherent distinction between the scientific realist’s own attitude toward Newtonian mechanics and her attitude toward the special and general theories of relativity into a muddle.

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To be sure, it is a difference that makes a difference. When pressed, the realist

will use either ~~special or general relativity~~ and not Newtonian mechanics to make predictions when she thinks minute differences might be important. She will use relativity and not Newtonian mechanics to ground her further inquiry into and exploration of the domain of nature described by both theories. And she will use relativity and not Newtonian mechanics to extend her theoretical representation of nature, her predictions, and her interventions into new parts of the relevant theoretical domain where the approximate predictive equivalence of the two theories is either unknown or is known to fail. The instrumentalist can do none of this. Instead, she must admit that she has no choice but to use a theoretical instrument she does not believe to be true when she makes predictions, no matter how high the cost of even minute errors. She must also admit that she has no better point of reference from which to initiate further inquiry into new aspects of a given domain of nature than the most powerful instrument she knows of for thinking about it and/or related natural domains. And she must admit that her only route forward in extending our powers of pragmatic engagement in a given natural domain is to press on in trying to apply the best existing conceptual tool she has to that domain to see whether and/or how it succeeds or fails, finding only by painful trial and error the situations and circumstances that suggest where existing tools require improvement and even what the radically different alternatives that would perform even better might be.

The form I have here given instrumentalism is importantly connected to the second of the distinctions Blackburn cites as a source of ersatz intuitive plausibility for a difference between instrumentalism and realism: the difference between *fully* embracing (or fully “immersing” oneself in) a theory and doing so in a more cautious or tentative

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spirit. Unlike the realist (and even van Fraassen's constructive empiricist), the instrumentalist described here believes that even our best current conceptual tool for thinking about a given scientific domain will *ultimately* be found wanting and be replaced by an alternative that offers even more powerful resources for guiding our pragmatic engagement with nature. Thus she has a very different picture of the future of scientific inquiry than the realist does, and she will be commensurately less willing to reach a conclusion that depends on the long-term stability of our best scientific theories than will her realist counterpart.

Furthermore, although she knows of no better way to extend her investigation into a novel domain of nature or an unfamiliar context than to explore and test the predictions of her best scientific theory or theories about it, we should expect the instrumentalist to be systematically less willing than her realist counterpart to *trust* those predictions until the theory's predictive adequacy in that unfamiliar domain or novel context can be independently verified. This does not mean, however, that she will view the theory's predictions as no more likely to be true than predictions somehow generated at random.

After all, her best scientific theory enjoys a successful track record in guiding our pragmatic engagement in applications and/or domains of nature continuous with or closely related to one in which she is trying to extend it (cf. Stanford 2000).<sup>12</sup> Of course a sensible caution about novel applications of our scientific theories can be motivated for the realist by worrying about the complexities and/or unknown interactions involved in

<sup>12</sup> Of course, in the absence of any such successful track record, she might be even more circumspect about a theory's predictive prospects, but then so might (and should) her realist counterpart.

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applying a given theory in a new domain or context, but there is no reason to think that the degree or character of such caution will or should precisely mimic that motivated by the instrumentalist's much more general lack of trust in the truth of the theory's fundamental claims, or any reason to think that the circumstances in which these two sorts of caution respectively become acute will ultimately be the same.

Perhaps most importantly of all, however, we should expect the instrumentalist to be much more open than her realist counterpart to the serious exploration of fundamentally distinct alternatives to her best current scientific theories (though not necessarily any particular alternative, of course). For the realist, the exploration of such alternatives is rendered reasonable only by her judgment that it is only *probable* that her best current theory is true, and such exploration is a sensible investment of time and effort only in the small space (and to the limited extent) left open by the unlikely prospect that it is false. By contrast, the instrumentalist doubts that even the best presently

available conceptual tools she has for thinking about nature will retain that status indefinitely as future inquiry proceeds, and this will lead her to be systematically open to the thoroughgoing exploration of fundamentally distinct alternatives, at least one of

which she fully expects to replace her best current theory in the fullness of time.<sup>13</sup> Thus

<sup>13</sup> Of course this represents a judgment about the fruitful or legitimate directions and investment of efforts for scientific inquiry as a whole, not a prediction about how any individual scientist will or should spend her time, which presumably owes as much to personal taste and expertise as anything. Nonetheless, in this important sense the instrumentalist is much more fully committed than the realist to the open-ended character of scientific inquiry itself. I do not mean to suggest, however, that one or the other view

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my instrumentalist does not say, with Blackburn's, "I don't really believe in genes, or

atoms, or gluons; it is just that I structure all my thoughts by accepting that they exist."

Both her thoughts and her further inquiry into nature are structured quite differently than

they would be if she believed that our best current theories of inheritance or of the minute

constitution of matter were true. And notice also that the instrumentalism articulated here

dissents not only from realism but also from the "core position" that quietism embraces

and suggests that realism and instrumentalism hold in common: in the various ways

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more faithfully represents the attitudes of scientists themselves on this question: it seems

to me that scientists hold widely varying views on this matter. More importantly, even if

we became satisfied that one or the other attitude toward further inquiry really were more

widely represented among practicing scientists, this would not automatically be an

important consideration in favor of adopting it and/or regarding it as a better

interpretation of the institution of science *itself*. In this connection, scientists simply offer

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candidate interpretations of the institution in which they participate, on all fours with

other contenders, and they speak with extensive knowledge of the institution but no

special authority about its proper interpretation, just as (it has been suggested cf.

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Dworkin 1986) a knowledge of what an individual legislator hoped to accomplish by

voting for a particular law need not carry any special authority in determining how the

law itself ought to be interpreted or applied to future circumstances. And in this

difference between achieving a better fit to the attitudes of its practitioners and a better fit

to the practice and/or history of the institution itself lives the most important room for the

philosophy of science to play a legitimate normative role in guiding scientific inquiry.

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described above, our instrumental commitment to the claims of theoretical science is simply *not* like our embrace of the more homely and familiar truths of everyday life.

Will the instrumentalist be unjustified or irrational in thus making use of a theory she does not think is even probably or approximately true and that she ultimately expects to fail in application to new situations and in new contexts? I do not see why. This is irrational or unjustifiable only if she knows of some particular alternative that she expects to give better results in a given concrete application or to provide a better guide to her further inquiry, and of course this is just what she does not have. Moreover, though she expects that even the best conceptual or theoretical instrument presently in her possession will ultimately fail, she has no specific reason to think that the very next occasion she has to apply it will be the one on which its fundamental inadequacy will be revealed. In this she occupies a position something like that of a person who is trapped underground and whose only route of escape is to make use of a blasting device with an unknown but limited number of charges: if she really has no options, she must use the device she knows will ultimately fail and simply hope on each crucial occasion that it is not the one on which that failure will materialize. Each is *serially* rational in using an instrument or tool that she knows to have a fair chance of working on that particular occasion, and each is faced only with the alternative of doing nothing at all. All that either can do *because* she knows that the best tool she has will ultimately fail is to be cautious about assuming it has worked on a new occasion (i.e., in a new context) until this can be independently verified and to keep a weather eye out for other, potentially even better, tools to use. In all of this, perhaps a person trapped underground and using tools of unknown reliability to try to escape bears an uncomfortable but uncanny resemblance to the rest of us.

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