Realism, Instrumentalism, Particularism: A Middle Path Forward in the Scientific Realism Debate

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 1. Introduction

Here I propose a particular conception of both the current state of play and what remains at stake in the ongoing debate concerning scientific realism. Recent decades have witnessed considerable evolution in this debate, including a welcome moderation of both realist and instrumentalist positions in response to evidence gathered from the historical record of scientific inquiry itself. In fact, I will suggest that a set of commitments has gradually emerged that are now embraced by many (though by no means all) who call themselves scientific realists and also by many (though by no means all) who would instead characterize themselves as instrumentalists or (perhaps less helpfully) “antirealists.” I will go on to suggest that these commitments collectively constitute a shared “Middle Path” on which many historically sophisticated realists and instrumentalists have already made substantial progress together, perhaps without realizing how much closer their respective views have thereby become to one another than either is to the classical forms of realism and instrumentalism whose labels and slogans they nonetheless conspicuously retain. I will then go on to suggest, however, that at least one crucial disagreement still remains even between realists and instrumentalists who walk this Middle Path together, and that this remaining difference actually makes a difference to how we should go about conducting scientific inquiry itself. Recognizing this difference poses a further challenge for instrumentalists both on and off the Middle Path, however, and responding to it will illuminate a further dimension of the realism debate itself: namely, the level(s) of abstraction or generality at which we should seek and expect to find useful epistemic guidance concerning scientific theories or beliefs. Along this dimension of the debate, I suggest, Middle Path Realists and Instrumentalists are once again united, but this time against both the shared presuppositions of their classical predecessors on the one hand and radical or extreme forms of particularism on the other.

2. Finding a Middle Path

The first of the shared commitments that I suggest constitute a Middle Path between classical forms of both realism and instrumentalism is to what I’ve elsewhere (Stanford 2015) called “Uniformitarianism,” in parallel with the great battle in 19th Century geology between Catastrophists and Uniformitarians concerning the causes and pattern of changes to the Earth. Famously, Uniformitarians (like Charles Lyell) argued that the broad topographic and geographic features of the Earth were the product of natural causes like floods, volcanoes, and earthquakes operating over immense stretches of time at roughly the same frequencies and magnitudes at which we find them acting today. Their Catastrophist opponents (like Georges Cuvier) held instead that such natural causes had operated in the past with considerably greater frequency and/or magnitude than those we now observe, on the order of the difference between a contemporary flood and the great Noachian deluge reported in the Christian Bible. Catastrophists thus held that the Earth had steadily quieted down over the course of its history, that truly fundamental and wide-ranging changes to its topography and geography are now confined to the distant past, and that contemporary natural causes will further modify that topography and geography only in comparatively marginal and limited ways. By contrast, Uniformitarians held that if given enough time to operate present-day natural causes would continue to transform the existing topography and geography of the Earth just as profoundly as it was transformed in the past.

Likewise in the case of the realism dispute, Uniformitarians take the view that the future of the scientific enterprise will continue to be characterized by theoretical revolutions and conceptual transformations just as profound and fundamental as those we have witnessed throughout the history of that enterprise. By contrast, a textbook description of what I will call classical or Catastrophist realism holds that “Our mature scientific theories, the ones used to underwrite our scientific projects and experiments, are mostly correct” and “[w]hat errors our mature theories contain are minor errors of detail” (Klee 1999 313-4). Such Catastrophists take the view that truly profound and fundamental revisions to our scientific understanding of the world are either largely or completely confined to the past and that the future of scientific inquiry will not be characterized by the sorts of fundamental revolutions or transformations by which theories like Newton’s mechanics, Dalton’s atomism, and Weismann’s theory of the germ-plasm ultimately came to be profoundly modified, qualified, amended, or simply replaced. That is, Catastrophist realists hold that the theoretical orthodoxy embraced by future scientific communities will include what seem both to them and us to be simply expanded, amended, and more sophisticated versions of the most successful theories we ourselves have already adopted. Thus, while Uniformitarians see us as being in the midst of an ongoing historical process of fundamental revolution or transformation in our scientific beliefs, rather than as having the enviable good fortune of living at or near the end of that process, Catastrophist scientific realists instead seem forced to adopt some form of exceptionalism concerning at least the most successful scientific theories of the present day.

Such classical or Catastrophist realism has not only been influentially championed by philosophical luminaries like Smart (1963) and Putnam (1975) but is also often claimed to be the view of the matter favored by common sense itself. It is therefore striking that in recent years many prominent historically sophisticated scientific realists have abandoned such Catastrophism in favor of the Uniformitarian alternative while insisting that this concession does not undermine the more modest realist epistemic entitlements that they themselves defend. 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 further changes, whether those privileged elements are held to be their claims about the “structure” of nature (Worrall 1989), their “working posits” (Kitcher 1993), their “core causal descriptions” (Psillos 1999), their “detection properties” (Chakravartty 2007, Egg 2016), the posits that “ unify the accurate empirical claims” of a theory (Peters 2014), the verae causae they identify (Novick and Scholl 2020), or something else altogether. This commitment to Uniformitarianism would seem to unite such selective realists with historically motivated defenders of instrumentalism or antirealism and against classical, commonsense, or Catastrophist varieties of scientific realism itself.

But realists and instrumentalists on the Middle Path are similarly united in rejecting central commitments of many classical forms of instrumentalism or antirealism as well, including the idea, perhaps most familiar from Thomas Kuhn in his most exuberant moods, that the changes still to come in our scientific conception of the world will periodically be so radical and profound that they will render it impossible to impartially, neutrally, or fairly compare theories on either side of such a revolutionary divide either to one another or to the available body of empirical evidence. Instead, Middle Path instrumentalists allow not only that we can perfectly well articulate the competing claims of two scientific theories about a single shared world using language that privileges neither theory, but also that we can characterize the available evidence in support of each theoretical alternative in ways that are at least neutral between those two competing theories (if not independent of any and all ‘theorizing’ whatsoever) in reaching an impartial judgment that one is better or worse confirmed by the existing evidence, or even that one or both theories are definitively refuted (within the bounds of fallibilism) by that evidence. They similarly reject the further consequence drawn by various interest-driven theorists of science that the outcomes of such theoretical competitions are therefore typically determined less by the available evidence in support of each theoretical alternative than by the comparative power, standing, resourcefulness, and determination of the social groups who advance and defend them. Where Uniformitarianism assures us that further profound and fundamental changes are still to come for our scientific picture of the world, a further commitment to what we might naturally call commensurability assures us that such changes will neither prevent us from fully understanding the competing conceptions of nature thereby proposed nor from impartially adjudicating the character and strength of the evidence in support of each of those competing conceptions. This is certainly not to suggest that resolving such competitions is simple or straightforward, or that an individual scientist’s own theoretical (and other) sympathies do not influence her evaluation of the evidence, or even that such comparisons can always be convincingly resolved by the particular body of evidence available at a given time. But it is to claim that their ultimate resolution typically depends on the accumulation of evidence that the relevant scientific community rightly sees as objectively and impartially favoring one proposal over the other. Such a commitment to commensurability would seem to similarly unite instrumentalists and realists on the Middle Path together with one another and against such radically Kuhnian varieties of instrumentalism or antirealism.

Realists and instrumentalists who travel this Middle Path together are also united against other classical varieties of instrumentalism or antirealism by their commitment to what I will call the Maddy/Wilson Principle, which I first encountered when my colleague Penelope Maddy skeptically responded to my own instrumentalist sympathies with this especially pithy formulation: “well, nothing works by accident or for no reason.” That is, she seemed to regard instrumentalism as committed not simply to the view that our best scientific theories are powerful cognitive instruments that guide our predictions, interventions, and other forms of practical engagement with nature successfully, but also that it is misguided or somehow illegitimate even to ask, much less try to discover or explain, how they manage to achieve that success when and where they do. Having recognized the ability of our best theories to guide our practical engagement with nature successfully, such instrumentalism rejects even the demand for any explanation of how or why they are able to do so.

When I reminded her of this conversation some years later, she was quick to credit the influence of Mark Wilson’s Wandering Significance as having inspired this particular expression of her disquiet. And indeed, Wilson does articulate something very like this same concern in his own inimitable way:

However, I regard this [“instrumentalist”] terminology as misleading because successful instrumentalities, whether they be of a mechanical or a symbolic nature, always work for reasons, even if we often cannot correctly diagnose the nature of these operations until long after we have learned to work profitably with the instruments themselves. (Wilson 2006, 220, original emphasis)

Both Maddy and Wilson, then, endorse the broad principle that when a scientific theory enjoys a track record of fine-grained and wide-ranging success in guiding our practical engagement with the world, it typically does so in virtue of some systematic connection or relationship between the description of the world offered by that theory and how things actually stand in the world itself.

 It is no accident that Maddy and Wilson both regard instrumentalism as committed to the idea that there is simply nothing more to say or know about how and why our instrumentally powerful scientific theories work as well as they do when and where they do. Scientific instrumentalism has had a wide variety of incarnations over the last several centuries, and some of the most influential are indeed characterized by explicit or implicit versions of this commitment. Perhaps most famously, some logical positivist and logical empiricist thinkers held that the claims of our best scientific theories are not even assertions about the world in the first place, nor, therefore, even candidates for truth or falsity, but instead simply ‘inference tickets’ allowing us to predict or infer some observable states of affairs from others. More recently, van Fraassen’s Constructive Empiricism argues that it is illegitimate to demand anything more than the empirical adequacy of a theory as an explanation of its success, insisting instead “that the observable phenomena exhibit these regularities, because of which they fit the theory, is merely a brute fact, and may or may not have an explanation in terms of unobservable facts ‘behind the phenomena’” (1980 24). Thus, there is indeed a long and distinguished philosophical tradition advocating forms of instrumentalism that either reject or remain agnostic concerning the Maddy/Wilson Principle, insisting instead that once we have recognized the instrumental utility of our scientific theories it is somehow misguided or illegitimate even to ask how or why those theories manage to be so instrumentally useful when and where they do.

The influential legacy of such classical instrumentalism makes it critical to recognize not just the plausibility but also the foundational importance of the Maddy/Wilson Principle. In science as in ordinary life, it is generally true that when things work, they work for reasons, and 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—otherwise that empirical success really would be miraculous! Indeed, the Middle Path not only embraces the Maddy/Wilson Principle’s insistence that there must be some reason why a cognitive instrument that works well does so, but also goes so far as to insist that the historical record itself provides us with abundant exemplars of at least the broad sorts of such systematic relationships that presumably also constitute the reasons for the successes of various contemporary scientific theories as well, whether or not we are ever in a position to specify those reasons more precisely in particular cases.

To see how, suppose for a moment that Einstein’s relativistic mechanics represents a true and complete account of how things stand in the otherwise inaccessible domain of nature that it seeks to describe. On that assumption, we can explain how and why Newton’s gravitational mechanics is so spectacularly successful when and where it is by describing in detail how aspects or elements of Newtonian mechanics are systematically related to features of the actual world (as described by General Relativity) and how these systematic relationships permit the theoretical apparatus of Newtonian mechanics to make accurate predictions and guide interventions successfully across a wide though not unlimited range of circumstances. If contemporary theoretical orthodoxy in mechanics simply describes how things really stand in nature, the details of those systematic relationships themselves constitute the “reasons” why Newtonian mechanics works as well as it does. Assuming the truth of contemporary theoretical orthodoxy more broadly would similarly allow us to identify the actual reasons for the systematic instrumental utility of many other successful but ultimately rejected scientific theories of the past such as the caloric theory of heat, phlogistic chemistry, the wave theory of light, Weismann’s theory of the germ-plasm, and many other familiar examples. And even if we think all contemporary theories are fundamentally mistaken, we should fully expect that highly successful contemporary scientific theories stand in one or more systematic relationships to the truth of the matter regarding various otherwise inaccessible domains of nature that are like the relationship between Newtonian and relativistic mechanics, or like that between caloric theory and contemporary thermodynamics, or like that between Weismann’s theory of the germ-plasm and contemporary molecular genetics, and so on. Of course, even this small collection of familiar examples makes clear that there is no single such relationship holding between all earlier successful theories and their contemporary successors: realists would need to appeal to a wide and heterogeneous array of different systematic relationships or “reasons” of this sort in explaining the diverse particular empirical successes of the various instrumentally powerful past scientific theories that have since been abandoned or replaced.

I suggest that many realists and instrumentalists alike, especially those who rely on evidence drawn from the historical record, have gradually come to embrace these shared commitments to Uniformitarianism, commensurability, and the Maddy-Wilson Principle, perhaps without fully realizing how much more they have thereby come to agree with one another than either of them does with the more classical doctrines whose labels and slogans they nonetheless retain. Their broadly shared conception of the past, present, and future of scientific inquiry anticipates many substantial continuities between future scientific orthodoxy and that of the present day (just as there are between present theoretical orthodoxy and that of the past), but also many substantial discontinuities just as profound and significant as those we now find in past historical episodes of fundamental revolution or upheaval. Moreover, I suggest that realists and instrumentalists who travel this Middle Path have already made considerable progress in using further historical evidence to refine and elaborate that broadly shared conception. Important recent work conducted by self-described realists and antirealists alike, for example, has revealed that the mistaken (i.e. ultimately rejected) claims or components of successful past theories have often played crucial and ineliminable roles in generating those theories’ empirical successes (e.g. Lyons (2002, 2006, 2016, 2017), Saatsi and Vickers (2011); for a fairly comprehensive listing, see Vickers (2013) and the references contained therein). Likewise, Peter Vickers (2013, 2017) has recently argued that we can at least identify posits (including some that do genuine work in generating a theory’s successful implications) that are nonetheless inessential or eliminable from those theories in a sense that renders them poor candidates for realist commitment. Such increases in the nuance and sophistication of their broadly shared vision of the past, present, and future of scientific inquiry should be recognizable as important forms of progress by both realists and instrumentalists alike on the Middle Path.

3. Trouble in Paradise: A Remaining Difference that Makes a Difference

I do not mean to suggest, however, that these shared commitments of realists and instrumentalists on the Middle Path leave no room for significant disagreement between them. As we noted above, “selective” scientific realists are largely motivated by the idea that the historical record itself (perhaps together with other considerations) puts us in a position to reliably distinguish the particular elements, aspects, or features of our own best scientific theories (e.g., working posits, core causal descriptions, structural claims, etc.) genuinely responsible for their empirical successes from those that are instead ‘idle’ or otherwise not required for those successes (e.g., the electromagnetic ether). Even if we expect further profound and dramatic changes in theoretical orthodoxy as scientific inquiry proceeds into the future, Middle Path realists insist that we can nonetheless justifiably expect these privileged elements, aspects, or features to be retained and ratified in some recognizable form throughout the further course of scientific inquiry itself. Note that without a commitment to at least this minimal form of stability or persistence, the selective realist loses not only her grounds for claiming to have identified secure epistemic possessions on which we can safely rely as inquiry proceeds, but also her grounds for claiming that these particular elements, aspects, or features of our best scientific theories were those genuinely responsible for their various empirical successes (for which they have turned out not to be essential or required after all).

By contrast, those on the Middle Path who embrace the ‘instrumentalist’ label certainly accept the Maddy/Wilson Principle’s insistence that there must be such reasons for the successes of our best scientific theories, but they nonetheless deny that we ourselves are generally in a position to know what those reasons are in particular cases, or to reliably identify which particular elements or features of our best scientific theories we should therefore expect to find persisting throughout the course of further inquiry. Such instrumentalists will see the wide variety of such reasons that (as we noted above) contemporary scientific orthodoxy would need to invoke in explaining the various successes of various past scientific theories as piling up counterexamples rather than confirmation for the idea that any single aspect, element, or feature of a successful scientific theory is invariably or even just reliably preserved in its successors: sometimes equations or claims about the ‘structure’ of nature are preserved from one successful scientific theory to its successors, but other times it is the fundamental entities posited by that theory, or the ‘core causal descriptions’ of those entities, or something else altogether—no one of these forms of continuity (or corresponding versions of selective realism itself) seems to capture even an especially wide range of central historical examples.

Rather than seeking to adjudicate this remaining central point of disagreement dividing even realists and instrumentalists traveling together on the Middle Path, I will instead argue for its importance. It truly matters, I suggest, because it actually makes a difference to how we should go about conducting further scientific inquiry itself. To see why, recall first the case of classical, commonsense, or Catastrophist realists, who are confident that the theoretical orthodoxy embraced by future scientific communities will include what seem both to us and to the members of those communities to be simply updated, expanded, and more sophisticated versions of at least the most successful theories that we ourselves have already embraced. At least with respect to those successful theories, then, the classical or Catastrophist realist simply does not see any real need for what the NSF and other granting agencies call “transformative science” and characterize explicitly as “revolutionizing entire disciplines; creating entirely new fields; or disrupting accepted theories and perspectives” (Bement 2007). The realist should be perfectly happy (or at least systematically happier than her instrumentalist counterpart, see below) for review boards to reject lines of research or theoretical proposals that fundamentally contradict or violate existing successful scientific theories, as she thinks it quite unlikely that any such alternative will ever become part of the theoretical orthodoxy we come to embrace in the future. Indeed, the more conflict there is between a given theoretical proposal and the central claims of existing successful theoretical orthodoxy, the more confident she will be that it is misguided in some fundamental and fatal way.

Of course, Middle Path realists instead hold only that we can justifiably predict which specific elements, aspects, or features of our best scientific theories are responsible for their successes, but they similarly expect those same elements, aspects, or features of our own theories to persist and be preserved in some recognizable form in any future scientific theory we ultimately adopt. (Again, if that expectation is defeated, the realist loses her claim to have picked out either the trustworthy elements of contemporary scientific theories or those actually responsible for their empirical successes.) Accordingly, it seems that they too should be systematically skeptical of theoretical proposals or avenues of research that contradict or fail to preserve whatever elements, aspects, and/or features of our best scientific theories engender those realist commitments. In both cases, realists seem to have systematic grounds for prioritizing investments in finding, exploring, and testing theoretical alternatives that preserve whatever it is that they are realists about over the pursuit of theoretical alternatives that fail to do so.

The realist may, of course, have instrumental or strategic reasons for taking seriously or exploring particular theoretical possibilities that conflict with what she takes herself to already know: as Ronald Fisher famously suggested, for example, “No practical biologist interested in sexual reproduction would be led to work out the detailed consequences experienced by organisms having three or more sexes, yet what else should he do if he wishes to understand why the sexes are, in fact, always two?” (1930, ix). She might even be convinced (for whatever reason) that the best way to make incremental progress in improving existing theories is by trying to articulate radically different alternative theoretical proposals with which to compare them. But any such strategic or instrumental reasons the realist may have for exploring or developing a theoretical alternative conflicting with (some privileged part of) existing theoretical orthodoxy the instrumentalist also has, plus another that is ultimately far more important: she believes that even more instrumentally powerful alternatives radically distinct from contemporary scientific theories are actually out there still waiting to be discovered.

Evaluating the promise, interest, or appeal of any particular theoretical proposal is, of course, a complex and multi-dimensional affair, so the suggestion here is certainly not that realists must always favor investing in the pursuit of any theoretical proposal (about anything) consistent with existing orthodoxy over the pursuit of any theoretical proposal (about anything) contradicting that orthodoxy. Nor is there some threshold degree of theoretical conservatism that realists must meet or exceed, either in general or in any particular case. The point instead is that those who are realists regarding some particular scientific theory (or privileged part thereof) should be prepared to treat the inconsistency of a given alternative theoretical proposal with the central claims (or otherwise privileged elements) of that theory as a reason to doubt that the alternative in question is even a viable candidate for representing the truth about the domain of nature it seeks to describe. All else being equal, this in turn constitutes a reason (for the realist) to discount or disfavor that alternative in competition for funding or support against otherwise equally attractive alternatives that do not similarly contradict (the relevant parts of) existing orthodoxy and therefore remain more plausible avenues for successfully extending, expanding, sophisticating, or supplementing our existing scientific conception of ourselves and the world around us. But this same reason for increased skepticism remains in force for the realist even when the alternatives being compared are anything but otherwise equally attractive, that is, even when it is simply one among a wide range of considerations bearing on the comparative attractions of investing in the pursuit of one theoretical proposal rather than another. Thus, holding all such further considerations fixed in any particular case reveals that realists have reasons instrumentalists lack for skepticism about just those theoretical proposals that violate (the relevant parts of) existing theoretical orthodoxy.

Of course, the instrumentalist no less than the realist will need to make difficult choices about how to invest and distribute the scarce resources available to support scientific inquiry itself. Many of the considerations she will weigh in making such judgments or decisions will operate for her in precisely the same way that they do for the realist: both, for example, will see alternative theoretical proposals as more attractive or promising targets for investment the better able they are to recover and/or explain whatever empirical consequences or implications of existing theories have already been independently confirmed by experiment and observation. Realists and instrumentalists will likewise appeal in much the same way to a wide array of further considerations to which granting agencies already direct the attention of their referees and review boards, such as the extent to which a proposal is well-reasoned, well-organized, and based on a sound rationale; the extent to which it promises to benefit society or advance desired societal outcomes; and the extent to which the proposers are well-qualified and have access to the resources needed to carry out the proposed activities. And both realists and instrumentalists are entitled to make nuanced, discriminating judgments driven by the details of the theory and evidence in question concerning the extent to which the exploration and development of any particular theoretical alternative or line of investigation is comparatively more or less likely to help us make progress either in refining and extending existing theoretical orthodoxy or in finding and developing even more empirically successful theories that will ultimately supplant those we now embrace. But for those who are realists about a given scientific theory (or particular elements, aspects, and features of that theory) that calculation should reflect what she already takes herself to know about the claims, elements, aspects, or features of contemporary theoretical orthodoxy that will be retained and ratified in some recognizable form as part of any theoretical orthodoxy we come to embrace in the future. This same consideration simply does not arise for those who are instead instrumentalists about that theory.

Theoretical Conservatism: A Double-Edged Sword

Although realists may bristle at the suggestion that they defend a systematically more theoretically conservative form of scientific inquiry than instrumentalists do, this difference seems to generate an even more direct and immediate challenge to instrumentalism instead. After all, if such commitments to various parts of contemporary theoretical orthodoxy are what enable realists to dismiss alternative theoretical possibilities out of hand when they conflict with too much of what we think we already know about the world, it seems natural to worry that the instrumentalist will, by contrast, wind up forced into an absurd permissiveness with respect to the alternative theoretical possibilities that she is prepared to take seriously and/or consider potentially deserving investments of the time, energy, money, and other scarce resources available for pursuing scientific inquiry itself. Surely even the instrumentalist thinks we should take a dim view of investing scarce resources in finding and/or developing alternative theoretical proposals contradicting claims like “fossils are the remains of once-living organisms,” “many diseases are caused by bacterial or viral infections,” or “the world around us is filled with microscopic organisms” as well as many others that seem put beyond serious question or reasonable doubt by the evidence we already have. Although the instrumentalist remains open to the possibility that future scientific communities may ultimately express these firmly established facts using a very different theoretical vocabulary than our own,[[1]](#footnote-1) she should nonetheless take a skeptical view of alternative theoretical proposals asserting or implying the falsity of such claims (even as we ourselves express them). (Here and throughout I assume that the theoretical alternatives in question simply contradict, explicitly or implicitly, the relevant claims concerning fossils, infections, microscopic organisms, etc., and do not offer convincing alternative explanations of the evidence we presently take to support those claims.) But if the instrumentalist restricts her beliefs to only the empirical consequences or observable implications of her best scientific theories, she might seem to lose any ground for thinking that theoretical alternatives contradicting such claims should be taken any less seriously or regarded with any more suspicion than those which instead simply contradict, say, the far more speculative claims of our best scientific theories concerning the nature of dark matter or dark energy.

Recall, however, that the instrumentalist’s view is not that all claims of contemporary theoretical orthodoxy in science will ultimately be abandoned, but instead simply that many central and fundamental claims will be and that we are not generally in a position to predict in advance just which claims these will be. What she rejects is the realist’s claim to have identified general or categorical features of scientific theories and/or their supporting evidence from which their approximate truth (or some analogue) can be reliably inferred. That is, the realism debate itself has been most fundamentally concerned with whether there is any general or categorical variety of empirical success or evidential support that serves as a reliable indicator that a theory (or its privileged parts) will be retained and ratified throughout the course of further inquiry. But 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. Many realists, for example, are prepared to make such an exception in the case of quantum mechanics, whose empirical success is extraordinary but whose very intelligibility to us as a description of how things actually stand in some otherwise inaccessible domain of nature remains controversial. Moreover, many realists are inclined to see the theory of evolution by natural selection as extremely well-confirmed despite the fact that the evidence supporting that theory includes little in the way of the ‘novel predictive success’ that they argue is generally required to justify the claim that a theory (or its privileged parts) will persist throughout the course of further inquiry. While the instrumentalist holds instead that no generic characteristic or category of theories or the evidence available in support of them entitles us to any more than the expectation that the theory in question is a useful conceptual tool or instrument for guiding our practical interaction with nature, she nonetheless remains just as free as the realist to recognize particular cases as exceptions to this generic expectation on the basis of considerations specific to those cases. What she cannot do is respond to the challenge by specifying generic epistemic characteristics or categories that distinguish trustworthy from untrustworthy scientific beliefs, for such generic characteristics and categories are just what she thinks we have yet to identify successfully.

Accordingly, her judgment that a particular belief or claim is established beyond a reasonable doubt will have to be a function of her evaluation of the details of the specific evidence she has in support of that particular belief. I have argued at length elsewhere (Stanford 2010), for example, that the details of the evidence we now have supporting the (once highly contentious) hypothesis that fossils are the remains of previously living organisms should lead us to conclude that this hypothesis is not merely a useful cognitive instrument but is in addition an accurate description of how things stand in nature itself. Of particular importance in that case, I suggested, was an abundance of what I called “projective” evidence in support of this hypothesis (esp. from the field of experimental taphonomy) in addition to merely eliminative and abductive forms of evidence. In a similar fashion, it will be the details of the evidence available in particular cases which convince the instrumentalist that any particular belief is established beyond a reasonable doubt. (Indeed, this same response might also appeal to selective or Middle Path realists, who face their own version of the problem insofar as proposed theoretical alternatives might well preserve the “working posits”, “structural claims”, or other privileged elements of contemporary theoretical orthodoxy while nonetheless disqualifying themselves from serious consideration by contradicting claims like “many diseases are caused by bacterial infections” or “fossils are the remains of once-living organisms”.)

Here an important strategic difference emerges between the realist’s and the instrumentalist’s respective engagements with the historical record. The realist begins from the attempt to explain the success of science and seeks to defend the credibility of one particular explanation against potentially undermining historical counterevidence, in the process refining whatever criterion of epistemic security she proposes for identifying just which categories of scientific claims and commitments she thinks can be trusted to persist throughout further scientific inquiry. By contrast, the instrumentalist begins from the demonstrably serious threat to the persistence of our scientific beliefs posed by the historical record and tries to find ways to restrict our beliefs so as to obviate or sufficiently mitigate that threat. Because she knows of no general way to distinguish contemporary scientific claims or beliefs that will be abandoned or overturned from those that will instead be ratified and retained in future theoretical orthodoxy, she will proceed cautiously, presuming that a generically successful scientific theory is simply a useful conceptual tool or instrument (like Newtonian mechanics or caloric thermodynamics) unless and until presented with compelling specific reasons to regard a particular theory, belief, or commitment as an accurate description of some otherwise inaccessible part of nature (like the organic origins of fossils). That is, she starts with strict constraints regarding the beliefs to which a generically successful theory entitles us (such as the theory’s “empirical implications” or its claims about “observable” matters of fact, though cf. Stanford 2006, Ch. 8) and then looks for reasons to relax these restrictions in particular cases. In the meantime, to oversimplify, given a motley collection of suspects with checkered pasts and conflicting evidence, 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.

 This freedom of both realists and instrumentalists on the Middle Path to countenance exceptions to their respective general expectations or inferential entitlements concerning the fates of generically successful theories does, however, represent a further departure from their classical predecessors, who instead (as we noted above) typically saw themselves as articulating competing proposals concerning the appropriate epistemic attitude to take towards “successful scientific theories” as such. In fact, recognizing this further divergence of Middle Path realism and instrumentalism from their classical counterparts invites renewed attention to a much less widely recognized further dimension of disagreement in the modern realism debate concerning the level of abstraction or generality at which we should seek or expect to find useful epistemic guidance concerning our scientific beliefs. Along this dimension, I will now suggest, Middle Path realists and instrumentalists are once again largely united with one another, not only in contrast to their classical predecessors’ fully general ambitions and expectations, but also to the diametrically opposed expectations of those who defend radical forms of what we might call “particularism” regarding the scientific realism debate.

Particularism and Generality in the Realism Debate

Since its inception in the writings of Smart, Putnam, Van Fraassen, Boyd, Laudan and others, the modern realism debate has been predicated on the assumption that there is some point to ascending to the levels of abstraction at which we generalize about our “mature scientific theories” and their “empirical successes” or “approximate truth.” For realists, the point of that ascent was to explain the successes of such theories in a way that revealed general or abstract epistemic categories we might use to reliably pick out those particular theories or otherwise privileged elements of contemporary scientific orthodoxy that represent secure epistemic possessions we can justifiably expect to persist in some recognizable form throughout the remaining course of scientific inquiry itself. The instrumentalist thinks we learn quite different lessons from considering matters at this level of abstraction and generality: she is convinced by the historical evidence, for example, that we should expect many of even the most fundamental commitments of our most successful contemporary theoretical orthodoxy to be eventually overturned and that there are no such general epistemic features or categories we might use to form reliable expectations regarding which of those commitments will or will not be preserved in some recognizable form throughout the course of our further scientific investigation of the world. Of course, if the instrumentalist goes on to claim that we should believe only a successful theory’s empirical implications, or what it says about observable matters of fact, or some such (cf. Stanford 2006, Ch. 8), she too is offering a (competing) abstract and general criterion of epistemic security for our scientific beliefs.

In contrast to both realism and instrumentalism, particularism holds that there is not now nor was there ever any point in ascending to these heights of abstraction and generality in the first place and suggests that no useful guidance for evaluating the epistemic status of scientific claims can be gleaned from doing so. The particularist thinks the very best we can do in deciding whether some particular scientific claim or commitment is true and/or will be retained and ratified throughout the course of further inquiry 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 behave, and she has more confidence in the outcome of those efforts than in any far less specific guidance we might hope to find by seeking broad patterns in the historical record or from any generic conception of how science works. That is, she thinks that the evidential import of any purported general relationship between empirical success and truth, or facts about how often past successful theories have turned out to be not even approximately true, or how reliably we’ve failed to conceive of well-confirmed theoretical alternatives when they existed, or how frequently past scientists themselves have held spectacularly mistaken beliefs about which parts of their own theories were conclusively confirmed by the available evidence, simply pales into comparative insignificance when confronted with the ordinary first-order evidence we have in favor of or against any particular scientific claim. In this way the particularist sees the existing debate between scientific realists and instrumentalists as simply superfluous to our real efforts to find out anything about the world: broad reflections on the scientific enterprise as a whole or patterns in the historical record simply add nothing of substance to the outcomes of those investigations in particular cases. Moreover, such generalities and abstractions are simply insensitive to precisely the sorts of variation in the details of the evidence we have in different cases that the particularist thinks really should generate varying degrees of confidence concerning various claims about the existence and character of fossils, or dark matter, or electrical charge, or bacterial infections.

Particularist sentiments of this sort, I suggest, constitute an important part of Arthur Fine’s motivation for embracing what he calls the Natural Ontological Attitude (NOA). Fine insists that realists and antirealists alike make a profound mistake when they seek to provide universalizing interpretations that purport to characterize the general aim of science, what our scientific claims really mean or say, and/or the epistemic accomplishments of science as a whole. “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” (1984 147-8). Instead, he suggests, the first-order scientific claims and counterclaims of ordinary scientific practice already represent our most careful efforts to decide what entities exist and what claims are true (in the humble, quotidian, philosophically unanalyzed senses of those terms), and we must treat such claims and beliefs as standing on their own bottom rather than standing in need of any further, distinctively philosophical analysis of what they really mean, or which ones we should actually believe, or even the point of making such claims in the first place. In fact, Fine rejects the idea that the scientific enterprise has any general aim or goal. Although scientific activity is replete with important goals and purposes, these are specific to particular contexts and practices, such as “For what purpose is this particular instrument being used, or why use a tungsten filament here rather than a copper one?” But he notes that it is simply a gross fallacy in quantifier logic to move from “They all have aims” to “There is an aim they all have” (1986 173). When we go on to try to identify the general goal, aim, or purpose of science itself, he says, “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” (1984 148): “the quest for a general aim [for science], like the quest for the meaning of life, is just hermeneuticism run amok” (1986 174).

Fine insists that it is similarly misguided to appeal to any global philosophical analysis or interpretation of science in order to decide which scientific claims to believe (e.g. those concerning “observable” states of affairs) or how much confidence to invest in them. Instead,

NOA’s attitude makes it wonder whether any theory of evidence is called for. The result is to open up the question of whether in particular contexts the evidence can reasonably be held to support belief (regardless of the character of the objects of belief). Thus NOA, as such, has no specific ontological commitments. It has only an attitude to recommend: namely, to look and see as openly as one can what it is reasonable to believe in and then to go with the belief and commitment that emerges. Different NOAers could, therefore, disagree about what exists, just as different, knowledgeable scientists disagree (1986b 176-177).

What binds these NOAers together, it seems, is simply their skepticism about whether ascending to some more general or abstract philosophical level of investigation, analysis, or reflection on science itself will help them make any progress in deciding what to believe about the world. Scraping away or refusing to indulge in the universalizing interpretations offered by both realist and antirealist philosophers of science simply leaves us with the first order practices of advancing, challenging, and defending particular scientific claims in particular scientific contexts. ‘The general lesson,” Fine suggests, “is that, in the context of science, adopting an attitude of belief has as warrant precisely that which science itself grants, nothing more but certainly nothing less” (Fine 1986a 147)

In a similar fashion, Penelope Maddy sees the trouble at the root of both realism and instrumentalism as their shared inclination to try to ascend to some higher philosophical court of epistemic evaluation in which we leave behind the ordinary sorts of evidence and evaluative standards that are characteristic of science itself. She sees van Fraassen, for example, as conceding that the evidence in favor of the existence of atoms is perfectly adequate and convincing for scientific purposes but insisting that this does not settle whether or not we have sufficient epistemological or philosophical grounds for holding such beliefs:

As far as methodology goes, the actual practice of science, it is perfectly reasonable for our scientist to take the Einstein/Perrin evidence as establishing the real existence of atoms. But for the proper ‘interpretation’ of atomic theory, we must adopt a point of view other than that of the practicing scientist: ‘stepping back for a moment,’ we adopt an ‘epistemic attitude’ towards the theory ([Van Fraassen, 1980] 82). Only then, answering the question as epistemologists, do we determine that the Einstein/Perrin evidence is not enough, and indeed, that no evidence can be enough to establish the existence of entities that cannot be perceived by unaided human senses. Here we have yet another two-level theory: at the ordinary scientific level, we have good evidence that atoms are real; at the interpretive, epistemic level, we do not. (Maddy 2001 43-44)

She suggests that realists like Boyd mistakenly take the bait here, trying to rise to the challenge of defending our scientific beliefs in this higher, philosophical court of inquiry where ordinary scientific evidence is disallowed, and thereby being pushed “away from the details of the local debate over atoms and towards global debates over such questions as whether or not the theoretical terms of mature scientific theories typically refer” (2001 46).

By contrast, Maddy’s “Second Philosopher” (2007) simply declines the invitation to leave ordinary scientific evidence and methods of evaluation behind and ascend to any such extrascientific or philosophical court of evaluation for scientific beliefs. Indeed, she feels no temptation to follow realists and antirealists down this shared rabbit hole unless and until someone can convincingly explain what this further distinctively epistemological or philosophical inquiry is supposed to achieve or accomplish and why the ordinary scientific evidence that actually convinces her of the reality of atoms should be treated as inadequate or irrelevant to that inquiry. She insists instead that scientific inquiry itself already represents our best efforts to decide (for any purposes we do or should actually care about) whether or not particular scientific claims are true and/or how much confidence in any particular claim is warranted, and that such questions can only be convincingly answered in the case-by-case or piecemeal manner that science itself employs:

where the constructive empiricist issues a blanket rejection of all unobservable posits, the Realist issues an equally blanket endorsement; the Second Philosopher faults both for passing over the details of the evidence for each particular posit, for shirking the responsibility to evaluate each case individually (2007 310n).

Although both Fine and Maddy deny that there are any useful answers to questions about science at the level of generality at which philosophers have traditionally sought them, they are both careful to leave room for at least the bare possibility that we might secure claims of somewhat greater generality (regarding confirmation, explanation, and the like) ‘bottom up’, as it were, by generalizing over and abstracting away from the details of particular investigations (Fine 1996 179-180; Maddy 2007 403n). But at least some particularists are explicitly skeptical regarding this possibility. Magnus and Callender, for example, have argued influentially that “profitable” realism debates must be conducted at the “retail” level of individual, particular scientific inquiries rather than “wholesale.” And although they concede that it might be “logically possible” to abstract away from or generalize over the results of such retail investigations, they doubt that the resulting guidance will be “either interesting or useful,” suggesting instead that “[w]e should pay attention to particular cases for their own sake and not as proxies for something else” and that “the great hope for realism and anti-realism lies in retail arguments that attend to the details of particular cases” (2004, 336). And Maddy herself has argued that when any such general epistemic guidance we embrace conflicts with the more detailed, informed, and contextual judgments of confirmation we make regarding particular cases, we typically prioritize the normative force of the particular judgments over that of the more general guidance. This is in part because such general guidance must itself remain sufficiently vague, indeterminate, and open-ended to permit further specification or adjustment in response to new developments: even if we generalized and abstracted our way bottom-up from historical or other evidence to the conclusion that we should only believe in entities we can ‘detect,’ for instance, she suggests that we will subsequently adjust the boundaries of what we count as ‘detection’ (thereby including or excluding some new method) so as to preserve the normative force of our reflective confirmational judgments about individual scientific cases (including those making use of the new method). In this way, as Maddy says it, “the ordinary science, not the [general] criterion, is doing the work” (2007 402).

 Moreover, even many of those who do still seek ‘interesting and useful’ general epistemic guidance concerning our scientific beliefs have in recent decades done so in ways that reflect growing particularist sympathies, suggesting that such guidance may well consist in the accumulation and synthesis of principles each of which applies to only a limited range of cases rather than the sort of fully general criterion of epistemic security sought by their classical predecessors. Richard Miller (1987), for example, argues that convincing realist commitments must be grounded in “topic-specific truisms” whose force is restricted to particular contexts or domains of inquiry, while Jamin Asay (2019) suggests that realism debates should be conducted at the level of particular sciences rather than science as a whole. More generally, Larry Sklar argues that “by far the most interesting issues will be found in the detail of how the global, skeptical claims…become particularized and concrete when they appear as specific problems about theories within the context of ongoing physical theorizing” (2000 78). And more recently, Juha Saatsi has advocated a similarly modest particularism, arguing first that it is “a manifestation of philosophical arrogance to think that as a realist philosopher one commits oneself to providing a global recipe—largely independently of the science steeped in relevant details—for revealing what aspects of theory-world correspondence makes any given theory in mature science tick,” but explicitly contending nonetheless that we should seek to generalize from the way particular exemplars latch onto the world to other cases “relevantly similar to those exemplars” (2017 3240-3241; see also Saatsi and Vickers 2011).

 Those who harbor modest particularist sympathies of this sort will presumably welcome my earlier suggestion that Middle Path realists and instrumentalists alike remain free to recognize exceptions to the general expectations they defend concerning the fates of generically successful scientific theories. Like such modest particularists, Middle Path realists and instrumentalists are prepared to abandon their classical predecessors’ aspirations to fully general criteria of epistemic security for our scientific beliefs (what Saatsi calls a ‘global recipe’), but they regard the radical particularist’s contrary conviction that there is little or no useful epistemic guidance to be found at any level of generality or abstraction higher than that of the individual case (what Asay calls “hyperlocalism”) as a counsel of despair. After all, “to look and see as openly as one can what it is reasonable to believe in and then to go with the belief and commitment that emerges” (as Fine and NOA recommend above) is presumably what scientists themselves have been doing all along, and many of their resulting sincere and carefully considered judgments have been among those ultimately overturned and abandoned in the course of further inquiry. Moreover, scientists’ own explicit judgments of conclusive confirmation for particular scientific theories, particular parts or aspects of those theories, and particular scientific claims have repeatedly turned out to be spectacularly mistaken (Stanford, 2006 Ch. 7). Our hope is to make more reliable judgments of this sort, and Middle Path realists and instrumentalists alike think that we can and do find considerable useful epistemic guidance informing those judgments at levels of abstraction and generality higher than that of the individual scientific investigation.

My own earlier examination of the case of organic fossil origins (2010), for example, suggested a tentative general moral: the greater the extent to which the supporting evidence for a given scientific theory or belief is eliminative or abductive in character, the greater its vulnerability to the problem of unconceived alternatives and the more cautious we should be about endorsing its truth. The historical record might also seem to support the view that highly successful scientific theories are more likely to be subsequently overturned in favor of previously unconceived alternatives when they concern questions of fundamental ontology in domains of nature far removed from ordinary human experience (like particle physics and cosmology as opposed to ecology or geology). And earlier we noted the detailed case made by Lyons and by Vickers for the claim that the subsequently abandoned components of successful past theories have regularly played crucial and ineliminable roles in generating the empirical successes (including the novel predictive successes) of those theories. None of these claims amounts to anything like the fully general criteria of epistemic security sought by classical realists and antirealists, but each is nonetheless a clear example of epistemic guidance intermediate in generality between those classical ambitions and the radical particularist’s competing conviction that useful epistemic guidance for science can only be found in the details of the evidence we have in support of particular scientific claims. Middle Path realists and instrumentalists are thus once again united, this time charting a course between the fully general commitments and expectations of their classical predecessors on the one hand and the sharply opposed expectations of radical or extreme forms of particularism on the other.

I’ve suggested here that the modern scientific realism debate is most centrally concerned with whether or not we have yet discovered some particular form(s) of empirical success or evidential support allowing us to reliably predict whether particular theories (or privileged parts thereof) will be retained and ratified throughout the course of further scientific inquiry. As I’ve tried to emphasize, realists and instrumentalists fundamentally disagree on the answer to this question (and thus on the predictability of further changes in our theoretical conception of the world around us) in ways that actually make a difference to how we should pursue our further scientific investigation of the world. But I have also argued that in recent decades many contemporary realists and instrumentalists have come to share a set of fundamental commitments that unite them more closely with one another than either is to their classical predecessors, including Uniformitarianism, commensurability, and the Maddy/Wilson principle, as well as to seeking useful guidance concerning the epistemic security of our scientific theories and beliefs at a level of abstraction and generality intermediate between that anticipated by radical or extreme forms of particularism and the perfectly general criteria of epistemic security envisioned by classical forms of realism and instrumentalism alike. For such Middle Path realists and instrumentalists, arguably more important than any remaining point of debate or disagreement between them is the shared epistemic project in which they are jointly engaged: realists and instrumentalists alike on the Middle Path are already actively seeking to identify, evaluate, and refine candidate indicators of epistemic security for our scientific beliefs, and both see the historical record of scientific inquiry itself as the most important source of evidence we have available to us for pursuing that joint project together.

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1. For example, when Joseph Priestly reported that after breathing “dephlogisticated air….my breast felt peculiarly light and easy for some time afterwards” we judge that he made a true claim about the effects of breathing oxygen using flawed or dated theoretical vocabulary, not a false or empty claim about a substance that does not exist (see Kitcher 1993 100). [↑](#footnote-ref-1)