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"ATOMS EXIST" IS PROBABLY TRUE, AND OTHER FACTS THAT SHOULD NOT COMFORT SCIENTIFIC REALISTS*

> Total and sudden transformations of language seldom happen; conquests and migrations are now very rare: but there are other causes of change, which, though slow in their operation, and invisible in their progress, are perhaps as much superior to human resistance, as the revolutions of the sky, or intumescence of the tide.
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> From the Preface to Samuel Johnson's *Dictionary*, 1755[1]

Here I seek to clarify the actual points of disagreement between scientific realists and those critics of realism who are motivated by the historical record of scientific inquiry itself. I will suggest that a perfectly natural argumentative strategy deployed by such historicist critics has generated a fundamentally mistaken picture of what they themselves are committed to and what would be required to vindicate their resistance to scientific realism itself. I will go on to suggest that the central point of contention in debates concerning scientific realism is not whether particular existential commitments of contemporary scientific theories will be held to be true or whether particular theoretical terms will be regarded as referential by future scientific communities, but whether or not the future of science will exhibit the same broad pattern of repeated, profound, and unpredictable changes in fundamental theoretical

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[1] Samuel Johnson, *Dictionary of the English Language* (London: J. F. and C. Rivington, 1755), Preface, p. 138; quoted in Mark Wilson, *Wandering Significance: An Essay in Conceptual Behavior* (New York: Oxford University Press, 2006), p. 662.

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orthodoxy that historicist critics of scientific realism argue characterizes its past.

I. HOW WE GOT TO NOW: HISTORY, APPROXIMATE TRUTH, AND REJECTED EXISTENTIAL COMMITMENTS

At the dawn of the twentieth century, the French physicist Henri Poincaré offered a forthright articulation of what philosophers of science have come to call the pessimistic induction over the history of science:

> The ephemeral nature of scientific theories takes by surprise the man of the world. Their brief period of prosperity ended, he sees them abandoned one after the other; he sees ruins piled upon ruins; he predicts that the theories in fashion today will in a short time succumb in their turn, and he concludes that they are absolutely in vain. This is what he calls the *bankruptcy of science*.[2]

Poincaré's concern has a distinguished pedigree, extending back perhaps as far as we find substantial changes in our beliefs about the fundamental constitution and operation of various parts of the natural world. In more recent years, this historicist line of thought has been developed and extended in various ways by thinkers like Thomas Kuhn, Larry Laudan, and myself.[3] Although there are important differences between the views of these historicists, there is an even more significant commonality: each sees us as being in the *midst* of an ongoing and unfolding historical process in which successful scientific accounts of various parts of nature are repeatedly replaced with even more impressive and powerful successors making fundamentally different claims about the constitution and/or operation of those parts of nature, and on such grounds each has opposed the competing scientific realist view that the best or only explanation for the dramatic empirical and practical successes of the scientific theories of our own day is that those theories provide broadly accurate descriptions of how things actually stand in various otherwise inaccessible domains of nature.

Just as Poincaré suggests, most of us are somewhat taken aback upon first encountering such historicist challenges to scientific realism.

[2] Henri Poincaré, *Science and Hypothesis* (Reprint of the first English translation; originally published (Paris, 1902) as *La Science et L'Hypothèse*; New York: Dover, 1952 [1905]), p. 160, original emphasis.

[3] See Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962); Larry Laudan, "A Confutation of Convergent Realism," *Philosophy of Science*, XLVIII, 1 (March 1981): 19–49; P. Kyle Stanford, *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives* (New York: Oxford University Press, 2006).

Particularly troubling is the prospect that our native enthusiasm for scientific realism might simply represent an artifact of perspective: if we lived long enough to be repeatedly confronted *as individuals* with theories whose practical achievements convinced us that they must be at least approximately true, only to see them ultimately replaced with even more powerful successors making fundamentally distinct and/or inconsistent claims about the constitution of nature, we would have learned to be quite cautious about simply assuming that the domain of theoretical science is one in which we may safely deploy our usually reliable inference from systematic practical success to the truth of the beliefs used to achieve that success. Given the actual timescales of human lives and theoretical changes in science, however, we can escape this perspectival limitation and foster the appropriate caution only by studying the history of science deeply enough to be able to appreciate how the epistemic situation must have appeared to those who preceded us. Of course there are always important differences between each successive generation of theories (including our own) and their historical predecessors in a given domain of scientific inquiry, but there seems little reason to think that such differences are sufficiently *categorical* to warrant the conviction that contemporary scientific theories have now finally managed to more-or-less sort things out at last, given that the same inference as applied to earlier theories, predicated on the salient advances and advantages of *those* theories over *their* predecessors, has turned out to be so repeatedly and reliably mistaken.

Any such historicist muckraking, however, will be immediately confronted with the extremely natural and intuitive response (and staple of scientific self-understanding in many fields) that most genuinely or sufficiently successful past theories are now properly judged to have been "approximately true" rather than simply false, which is to say mistaken only in matters of detail, or at least broadly continuous with their contemporary counterparts in ways that undermine any suggestion that our fundamental conceptions of the various domains of nature have been repeatedly overturned. Of course, no clear, general, and suitable criterion of such "approximate truth" or fundamental continuity has ever been articulated. Therefore, historicist critics of scientific realism who aim to establish more than the anodyne fallibilist conclusion that our theories are probably not correct and complete in every detail have found themselves forced to try to secure their conclusions against a manifestly plausible and powerful response that has yet to be formulated with any precision.

As a consequence, historicist critics of scientific realism have tended to focus their attention disproportionately upon a particular sort of historical example: those cases in which successful past scientific theories have made central use of one or more theoretical terms that have subsequently been abandoned and/or judged non-referential, such as 'caloric', 'phlogiston', and 'luminiferous ether'. The rationale for this focus is that such theories seem to automatically incorporate central existential commitments (to the supposed referents of those abandoned terms) that cannot now be sensibly judged to have been even approximately true, and such examples are thus ones in which the hopeful appeal to approximate truth remains least plausible *no matter how that notion may ultimately come to be understood or cashed out.* As Larry Laudan articulates the strategic rationale for lavishing attention on such examples,

> I take it that *a realist would never want to say that a theory was approximately true if its central theoretical terms failed to refer.* If there were nothing like genes, then a genetic theory, no matter how well confirmed it was, would not be approximately true. If there were no entities similar to atoms, no atomic theory could be approximately true; if there were no sub-atomic particles, then no quantum theory of chemistry could be approximately true. In short, a necessary condition—especially for a scientific realist—for a theory being close to the truth is that its central explanatory terms genuinely refer.[4]

Laudan goes on to suggest that the historical record remains an embarrassment of riches for the historicist critic of realism even if we confine our attention exclusively to such examples. That is, even if we simply ignore the many past successful theories we would now judge not even approximately true despite the fact that all of their central terms were clearly (or at least arguably) referential, Laudan famously (or infamously) claims,

> I daresay that for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring.[5]

Although a strategic emphasis on examples of theories with subsequently abandoned central existential commitments is thus understandable, it has also unfortunately created and sustained a subtly but profoundly misguided conception of what would be required in order to ultimately vindicate the historicist's concerns regarding

[4] Laudan, *op. cit.*, p. 33, original emphasis. However, cf. Clyde Hardin and Alexander Rosenberg, "In Defense of Convergent Realism," *Philosophy of Science*, XLIX, 4 (December 1982): 604–15.

[5] Laudan, *op. cit.*, p. 35.

the truth of contemporary scientific theories. Historicist critics of scientific realism are often met with a wry (even indulgent) smile and some version of the understandably incredulous inquiry, "Surely you don't seriously doubt that atoms exist?," or "You don't *really* believe that there is no such thing as a gene, do you?" The central role played in these debates by examples in which existential commitments to entities and substances like caloric fluid, phlogiston, and the luminiferous ether that have clearly been subsequently abandoned or rejected has regrettably suggested to many observers that the historicist challenge fails unless the most central existential commitments of contemporary theories—"there are genes," "atoms exist"—also turn out to be false and/or the terms in them turn out to be non-referential. The burden of this paper will be to make clear both why this suggestion is profoundly misguided and, more generally, why such existential claims are among the least useful or informative we could consider in trying to decide whether or not the historicist's reservations are well founded. I will conclude by trying to identify genuine points of fundamental disagreement between contemporary scientific realists and their historicist critics.

II. EXISTENTIAL COMMITMENTS, REFERENCE, AND BELIEF CHANGE

II.1. The first point to make in this connection is simply that we rarely if ever wind up rejecting all of the central existential commitments of a successful past theory no matter how deeply mistaken it ultimately turns out to be. This is perhaps easiest to see in the case of the wave theory of light (and/or electromagnetism) and its famous commitment to the existence of the luminiferous ether. This theory included a wealth of other theoretical or hypothetical entities (such as "light ray," "transverse wave," "polarized light") whose existence was instead embraced by its historical successors, and the terms used by earlier theorists to designate these entities are judged by the lights of current theories to have been more-or-less straightforwardly referential. The wave theory is a powerful example for the historicist critic of scientific realism not because it contains *no* central existential commitments we still embrace (or terms we regard as referential), but because it clearly includes *some* particularly central existential commitment that has been subsequently rejected, which in turn makes it seem an unpromising candidate for any attempted retreat to "approximate truth." Even in such paradigmatic cases, however, we do not find that all or even most central existential commitments of a theory are ultimately overturned, nor are all or even most of its central terms ultimately judged to be non-referential.

This alone is enough to show why the realist's historicist opponent is simply not committed to claiming that future scientists and scientific communities will judge that there are no atoms, that the claim "atoms exist" is false, or that the term 'atom' failed to refer. Even when we have found one of a theory's central existential commitments overturned in the course of further inquiry, many otherwise similar commitments have remained in place, so even in cases in which a successful scientific theory does have central existential commitments that will ultimately be judged to be false and/or central theoretical terms that will ultimately be judged non-referential, we should not imagine that we can specify in advance which existential commitments and/or theoretical terms these will be. There is no reason to think that every existential commitment (or even every central existential commitment) will suffer this fate, nor that we can pick out a specific existential commitment and demand in advance that it in particular must do so in order for the theory that incorporates it to be judged not even approximately true. Asking historicist challengers of scientific realism whether they truly doubt if genes or atoms exist is thus a bit like asking those who are skeptical of Creationist biology "You don't really believe there's no such thing as an organism, do you?"

Of course, failing to qualify as approximately true *need* not involve the judgment that even one central theoretical term is non-referential. Contemporary realists hold that Newton's celestial mechanics radically misconceived the fundamental character of gravitation, for example, not that the term 'gravity' did not refer or that the claim "gravity exists" has turned out to be false. Indeed, it might be argued that Newton's mechanics involves *no* central existential claim of the form "*X* exists" or "there are *X*'s" that can now be held to be *straightforwardly* false, but it is nonetheless a paradigm case of a scientific theory whose description of a given natural domain has been replaced by that of a radically and fundamentally distinct successor. Thus, there are no grounds for thinking that the abandonment of a scientific theory, or even a judgment that the theory cannot be "approximately true," must involve relinquishing any existential commitment ("there are genes," "atoms exist") at all, much less a particular one that can be specified in advance.

II.2. But there is a second and deeper reason to resist investing such existence claims and associated judgments of referential continuity for particular terms with dispositive significance in this context: such claims and judgments also turn out to be sensitive to considerations that are quite far removed from the merits of the case for or against scientific realism. Perhaps foremost among these is the fact that our judgments of referential continuity (and the associated existence claims) are profoundly sensitive to whether or not particular *terms* used in ultimately rejected past theories have been retained or abandoned. Howard Stein expresses the point with characteristically (and inimitably) irascible eloquence:

> For my part, I throw up my hands at this: Why should we say that the old term 'ether' failed to 'refer'?—and that the old term 'atom' did 'refer'? Why, that is, except for the superficial reason that the word 'atom' is still used in text-books, the word 'ether' not?…in brief: our own physics teaches us that there is *nothing* that has *all* the properties posited by nineteenth-century physicists for the ether *or* for atoms; but that, on the other hand, in *both* instances, rather important parts of the nineteenth-century theories are correct….The two cases—that of the ether and that of atoms—are, in my view, so similar, that the radical distinction made between them by the referential realists confirms in me the antecedent suspicion that this concern for reference…is a distraction from what really matters.[6]

The point might be less troubling if we doubted that such decisions about terminological continuity are in fact as "superficial" as Stein suggests: we might optimistically suppose that we abandon existing theoretical terminology just when the differences between the beliefs we would now be required to associate with the term and those with which it was originally introduced or widely adopted would thereby exceed some principled (though possibly vague) threshold. But the historical trajectory of the term 'atom' itself, from Democritus through (among others) Proust, Dalton, Perrin, Thompson, Rutherford, Bohr, Heisenberg, and contemporary quantum mechanics, seems likely to singlehandedly dispose of nearly any concrete proposal for such a threshold. And when questions of terminological continuity and abandonment arise in more explicit and systematic ways, such as the revolution in chemical nomenclature with which Lavoisier sought to support his new oxygen chemistry, the motivations in play typically seem to have more to do with the ongoing efforts of scientists to win acceptance for scientific ideas by *positioning* them with respect to earlier or contemporary competitors[7] (highlighting particular continuities or discontinuities) than with even any attempt to determine whether the magnitude of revision in associated beliefs has or has not exceeded some principled or systematic threshold.

Indeed, detailed investigations by philosophers of science[8] testify instead to the considerable role of historical and circumstantial

[6] Howard Stein, "Yes, but…Some Skeptical Remarks on Realism and Anti-Realism," *Dialectica*, XLIII, 1–2 (June 1989): 47–65, original emphasis.

[7] See Arthur Donovan, *Antoine Lavoisier: Science, Administration, and Revolution* (Cambridge, MA: Blackwell, 1993).

[8] See Mark Wilson, "Predicate Meets Property," *The Philosophical Review*, XCI, 4 (October 1982): 549–89; Joseph LaPorte, *Natural Kinds and Conceptual Change* (New York: Cambridge University Press, 2004); Wilson, *Wandering Significance*, *op. cit.*

happenstance in determining terminological evolution in science. As Joseph LaPorte notes, for example, following recent empirical discoveries about the class of organisms previously regarded as 'rodents', cladistic taxonomists faced a choice between retaining 'rodent' as a legitimate taxonomic category but radically revising its extension (for example, to exclude guinea pigs), as actually occurred both in this case and similar cases like 'fish' and 'dinosaur', or instead demoting 'rodent' to a "folk" category that does not correspond to any legitimate phylogenetic taxon, as was done instead in otherwise comparable cases involving terms like 'algae', 'reptile', and 'lizard':

> Neither option seems to have been forced. The headline-making conclusion that 'the guinea pig is not a rodent' was not discovered to be true, and scientists would not have been mistaken to have concluded otherwise. More precisely, neither the conclusion that guinea pigs are 'rodents' nor that they are not 'rodents' is quite right or quite wrong on the earlier usage of 'rodent'. The earlier usage is vague about the matter. To make one conclusion standard or correct, the meaning of 'rodent' had to be altered. Scientists would have been entitled to alter language either way, so neither possible conclusion seems to represent a discovery about what have all along been called 'rodents'. It is not as if one conclusion gets the facts right and the other gets them wrong….A term like 'rodent' might meet a variety of fates after phylogenetic disruption. Which fate attends it is for the working taxonomist to choose.[9]

The point here is simply that the magnitude of the change in meaning or extension that would have been required in order to retain 'rodent' as a legitimate taxonomic category within evolutionary theory did little to settle whether the term itself could be retained or would have to be abandoned for scientific purposes. Preserving 'rodent' while radically altering its extension or instead abandoning it to "the folk" both remained live options even when the comparative magnitude of the associated change in extension and/or meaning was made both precise and explicit.

In a similar vein, Mark Wilson has argued that the original extension of a term as it is used by a given linguistic community is limited by a 'range of application' parameter, with a (not necessarily explicit) conventional decision required about whether and how to modify that extension when faced with a novel case outside that original range of application:

> In truth, the *implicit parameters* appropriate to these predicates will have widened enormously over the past four centuries and no linguist could

[9] LaPorte, *Natural Kinds and Conceptual Change*, *op. cit.*, pp. 67–68.

> have legitimately predicted how their application was to be extended in the new circumstances. We observed this earlier for "weight" and "electron"; the same moral holds for "momentum" as well. Given this change of parameters, it becomes misleading to say that the extensions of these predicates haven't changed over time (although it is equally inappropriate to claim that they have).[10]

In sum, how a term will come to be extended or applied by speakers in novel, changed, or unexpected scientific contexts seems to owe as much the vicissitudes of later historical fortune or circumstance as anything else. Or as Wilson has more recently summarized this point:

> [W]e have plainly invested excessive philosophical hope in the expectation that the contents of our concepts can be held firmly fixed, if only we remain sufficiently vigilant. We need to frame, I think, a far more mitigated appraisal of our capacities to anticipate our linguistic futures.[11]

We can make the point looking forward rather than backward by considering the contemporary term 'gene'. A persistent and growing minority tradition in biology has argued that what the molecular revolution has ultimately revealed is that there simply *are no* such things as Mendelian genes: that literally nothing in the world (and certainly no *one* physically contiguous type of thing) systematically exhibits even a substantial majority of the features ascribed to a classical Mendelian gene.[12] As Alexander Rosenberg notes:

> Of course, another thing molecular biology did *to*, and not *for* Classical genetics, was gravely to undermine its ontology. Molecular genetics reveals that there is no one single kind of thing that in fact does what Classical genetics tells us (classical) genes do.[13]

A footnote to this claim argues (in part) that "molecular biology drastically shifts causal roles away from the classical gene and towards so many molecules as to extirpate the entire gene concept." A salient consequence of this shift, reaching at least as far back as Seymour Benzer's

[10] Wilson, "Predicate Meets Property," *op. cit.*, p. 580.

[11] Wilson, *Wandering Significance*, *op. cit.*, p. 11.

[12] For challenges that arise for any attempt to identify classical Mendelian genes with particular stretches of DNA, see Petter Portin, "The Concept of the Gene: Short History and Present Status," *Quarterly Review of Biology*, LXVIII, 2 (June 1993): 173–223; Kim Sterelny and Paul Griffiths, *Sex and Death: An Introduction to Philosophy of Biology* (Chicago: University of Chicago Press, 1999), chapters 6 and 7; Lenny Moss, *What Genes Can't Do* (Cambridge, MA: MIT Press, 2003), especially chapter 1; and Peter Godfrey-Smith, *Darwinian Populations and Natural Selection* (New York: Oxford University Press, 2009), chapter 7.

[13] Alexander Rosenberg, "Reductionism Redux: Computing the Embryo," *Biology and Philosophy*, XII, 4 (October 1997): 445–70, at p. 447.

famous proposal (1957) to replace the terminology of 'genes' with that of 'cistrons' (the genetic unit of function), 'mutons' (unit of mutation), and 'recons' (unit of recombination),[14] has been the periodic suggestion that we would be better off abandoning the terminology of genes altogether and instead talk about the properties possessed by various stretches of DNA (and perhaps other molecules as well). The point here is not to endorse this terminological proposal, but instead simply to notice that *if* it were to be accepted today, this would considerably strengthen any future case for the non-referential character of the term 'gene' and the falsity of the existential commitments of Mendelian geneticists going forward, even in the absence of *any* additional or corresponding change in our substantive beliefs *about* DNA or the processes of heredity, development, reproduction, evolution, or any empirical matter. But the disagreement between the scientific realist and her historicist critic was supposed to depend on the extent or depth or fundamentality of precisely such differences, not on historical accidents of terminological legislation. Perhaps those who advocate abandoning the term 'gene' altogether will ultimately win the day, and future scientists will judge that 'gene' (like 'phlogiston') did not refer or that the claim that "there are genes" was false. But whether or not they do so will not be an especially sensitive indicator of the magnitude of change in our associated beliefs about the causal agents implicated in heredity, and the character and magnitude of *that* change is what really matters for debates concerning scientific realism.

This sensitivity of our judgments of referential continuity to terminological decisions that are at the very least not *determined* by the magnitude of associated change in beliefs or meaning helps to illustrate something that I think has been widely overlooked in the approaches usually taken to questions about reference and meaning by philosophers: those approaches have tended to obscure the fact that judgments about referential continuity (and therefore about the truth of past existential commitments) always involve *interpretive* decisions concerning past speakers and linguistic communities. Philosophers of language and philosophers of science alike, whether they are defending 'causal' accounts of reference, 'descriptive' accounts, hybrid views, or something else altogether, have often approached questions about the reference of terms as used by both past and contemporary

[14] Seymour Benzer, "The Elementary Units of Heredity," in William D. McElroy and Bentley Glass, eds., *The Chemical Basis of Heredity* (Baltimore: Johns Hopkins University Press, 1957), pp. 70–93. In the time since Benzer made this suggestion, things have gotten much, much worse (see previous footnote).

speakers armed with intuitions about the continuity and discontinuity of reference that they treat simply as data that it is the job of a philosophical "theory" of reference to recover and ratify.[15] And this in turn has led them to suppose that the empirical facts at the time Aristotle used the Greek word 'hudor' and at the time that Priestley used the term 'phlogiston' are sufficient to determine the facts about which objects or properties in the world were those to which those terms did and did not refer. But this way of seeing the situation simply ignores the fact that we ourselves are making decisions about how to *interpret* Aristotle and Priestley. The fact that treating Aristotle's 'hudor' as referentially continuous with our own term 'water' is an easy or even automatic decision should not obscure the fact that it is an interpretive decision nonetheless—or that it is in such decisions that our intuitions about referential continuity and discontinuity are ultimately grounded.[16] (Of course, the same is true for the references of terms and the interpretation of speakers within our very own linguistic communities.) The fact that many such decisions are undertaken automatically, unreflectively, and without hesitation misleads us into treating their results as brute facts about the world, facts which must be entailed or implied by any acceptable philosophical "theory" of the reference relation itself. But this treats the relation of reference on too close an analogy with relations like distance or paternity. What theories of reference must actually do is explain how and why the interpretive principles we unreflectively deploy in these cases make the relevant judgments about reference and referential continuity universal (when they are) among competent speakers in possession of a set of further beliefs about the origin, genealogical history, and/or use of a given term and the state of the world. And we would do better to deliberate about the conditions under which our own uses of terms like 'atom' and 'gene' will be *held* to be referential (and our claims that "atoms exist" or "there are genes" will be *held* true) by the members of future linguistic communities who interpret *us* than about whether such referential status or truth is straightforwardly established by even the sum total of facts that are presently settled.

Recognizing this (usually suppressed) interpretive dimension of such judgments might also lead us to think somewhat differently

[15] One clear example is P. Kyle Stanford and Philip Kitcher, "Refining the Causal Theory of Reference for Natural Kind Terms," *Philosophical Studies*, XCVII, 1 (January 2000): 99–129, but the modern *locus classicus* is Saul Kripke, *Naming and Necessity* (Cambridge, MA: Harvard University Press, 1980).

[16] I have been helped to see this point by discussions with my colleague Penelope Maddy, who defends a more detailed version of the idea in section II.4 of her *Second Philosophy: A Naturalistic Method* (Oxford: Oxford University Press, 2007).

about Philip Kitcher's[17] influential suggestion that different *tokens* of a given term-type (that is, different instances of actual usage) can have their referents fixed in different ways on different occasions of actual use. Kitcher argues that different instances of the very same term (even as used by the very same speaker) will have their references fixed differently depending on the speaker's dominant "referential intentions" on that particular occasion of use: he suggests, for example, that the references of some of Priestley's tokens of 'dephlogisticated air' were fixed by his intention to refer to air with the substance emitted in combustion removed from it (and therefore failed to refer, since there is no such substance), while the references of others were fixed by his intention to refer to the substance whose inhalation was rendering his breathing particularly light and easy or to the substance he "exploded together" with "inflammable air" to produce water or nitric acid (and thus referred to oxygen). But this picture again suggests that whether a given token of a given term refers (and to what) is timelessly fixed by the facts in place about the speaker (including his referential intentions), history, and the state of the world at the time that token is produced. We would do better to say that our judgments concerning how to *interpret* a particular token of a given term-type by a particular speaker and how to assign a reference to it are often sensitive to facts that can vary across different occasions or contexts of usage, and that while these considerations can certainly include a speaker's "dominant referential intentions" when these happen to be sufficiently explicit, determinate, and/or evident to impact our interpretive inclinations, they will also routinely include subsequent historical developments not dictated in turn by those intentions.

II.3. But whether or not we adopt this more elaborate general view of how different tokens of a given term-type can come to have widely varying referents assigned to them, simply accepting the fact of such variation itself directs our attention to yet a third reason that existence claims are among the least informative or useful to consider in trying to decide whether the historicist is right to doubt even the approximate truth of contemporary scientific theories. For whatever variation there is in the demands imposed as a condition on successful reference for theoretical terms between different occasions of use, it seems clear that these demands will be *least* demanding in the case of bare existence claims like "there are atoms" or "genes exist." That is, the

[17] Philip Kitcher, *The Advancement of Science* (New York: Oxford University Press, 1993), chapter 4. See also Stanford and Kitcher, "Refining the Causal Theory of Reference for Natural Kind Terms," *op. cit.*

truth of a bare existence claim or existential commitment is consistent with a maximal degree of substantive change in our beliefs *about* the putative targets of that commitment (which can be transmitted down a chain of shared and copied usage), while nearly every other sort of claim one can make using the term in question will entail additional substantive commitments which will also have to be satisfied if the token of the term in question is to be judged referential. Note that Kitcher judges particular tokens of 'dephlogisticated air' to have been referential in just those cases where the descriptive demands he takes to be imposed by Priestley's dominant referential intentions ("the substance whose inhalation was rendering his breathing particularly light and easy" and "the substance he 'exploded together' with 'inflammable air' to produce water or nitric acid," respectively) were in fact satisfied (we now think) by oxygen. Of course, those demands might not have been satisfied at all—suppose it had turned out that there *was no* substance rendering Priestley's breathing particularly light and easy (say if Priestley's sensation of altered breathing was a perceptual illusion induced by some feature of the experimental situation)—and in such cases we might well decide that the relevant tokens of 'dephlogisticated air' did not refer to anything, just like those supposedly governed by Priestley's intention to refer to the substance emitted in combustion. But notice also that any descriptive demands imposed (whether by Priestley's referential intentions or by anything else) as conditions for successful reference would seem to be at an *absolute minimum* in the case of claims like "dephlogisticated air exists" or "there is dephlogisticated air." For us to hold such a claim true (whether shouted defiantly by Priestley at recalcitrant colleagues or whispered reassuringly to himself in the middle of the night) it might well be enough that there is *some* specific substance (or even just a combination of such substances) reliably implicated in *any* of the causal/theoretical roles or concrete experimental circumstances to which Priestley was disposed to apply the term, no matter how remote those substance(s) might be from Priestley's original conception of them. And once again, disputes concerning scientific realism were supposed to turn on the relative accuracy of such substantive conceptions, not on the fact that our interpretive inclinations are maximally liberal or charitable when we consider a particular class of linguistic claims.

We can also see this point by returning to the case of 'gene' and considering the vantage point of future scientists trying to interpret *us*. The "dominant referential intentions" or other constraints on our interpretive freedom associated with tokens of 'gene' used by a contemporary speaker to make any *substantive* claim about what genes are or do—that "genes are the bearers of hereditary information," that "the sequence of this gene was determined using a chain termination method," that "single genes often influence many different phenotypic traits," that "offspring receive a single allelic form of a gene at each locus from each parent"—will be considerably more demanding than those imposed by the simple claim that "genes exist" or "there are genes." This again illustrates that the truth of the latter claims (or better, the dispositions of future scientists and scientific communities to *hold* such claims true (and/or their central terms referential) when interpreting our own utterances) tolerates the maximum possible degree of change in our beliefs *about* genes and between those later thinkers' theoretical conception of nature and our own: it may well be that we continue to hold "genes exist" to be true indefinitely while our beliefs *about* genes change in just the sorts of profound and unpredictable ways that the historicist critic of scientific realism supposes they will. And whether or not we continue to hold such claims true or such tokens of terms to have been referential will thus be quite tenuously connected to whether or not realism has turned out to be the right attitude to adopt towards classical Mendelian genetics.

Note that these considerations might also lead us to question the emphasis placed by some recent thinkers on ways of directly detecting the presence of the entities posited by a given scientific theory[18] and/or on so-called "detection properties."[19] Chakravarrty introduces the latter as "causal properties one has managed to detect; they are causally linked to the regular behaviors of our detectors."[20] To be sure, such properties and techniques matter, for they anchor our uses of the term naming a given entity (the hypothesized bearer of a given causal role) quite directly to the ultimate causes of particular experiences or instrumental representations in ways that makes it *much* harder to ultimately decide that the term in question referred to nothing at all (or that the associated existence claims were false). But interpreting earlier speakers to preserve reference because of such perceptual and instrumental contact is perfectly consistent with massive changes in our beliefs *about* the referent of the associated term or existence claim, as illustrated by our earlier beliefs about such directly detectable entities as "chromosomes," "planets," or "fossils." Similarly, although we now assign atoms a sufficiently wide array of causal roles in different theoretical contexts and we have a sufficiently large and diverse

[18] See Maddy, *Second Philosophy*, *op. cit.*, section IV.5.

[19] See Anjan Chakravartty's *A Metaphysics for Scientific Realism: Knowing the Unobservable* (Cambridge, UK: Cambridge University Press, 2007).

[20] *Ibid.*, p. 47.

set of ways to detect their presence, absence, number, and so on in a wide variety of heterogeneous experimental circumstances that (at the acknowledged risk of anticipating our linguistic future too confidently!) I sincerely doubt future generations of scientists will ever come to judge that our talk of atoms was really about nothing at all, that there is "no such thing as" an atom, and/or that the term 'atom' as we use it failed to refer. But this confidence is predicated on the robust principles of charity we deploy in interpreting earlier speakers rather than any assurance that our substantive theoretical beliefs *about* atoms will not change profoundly as science moves forward. And of course it was the assumption of or commitment to continuity in those substantive beliefs about nature that the historicist was concerned to call into question in the first place.

II.4. In short, there are a variety of reasons that we should decline to saddle the scientific realist's historicist opponent with the belief that there are no such things as genes or atoms or that the terms 'gene' and 'atom' do not refer. Even in the case of theories we now regard as quite clearly and thoroughly discredited, only a small minority of such existential commitments (if any at all) are ultimately rejected, and no particular claim of this sort can be specified in advance whose falsity is a plausible *requirement* for a theory not being even approximately true. Moreover, our interpretive judgments concerning which existence claims are true and/or when their central terms are referential are sensitive to considerations quite remote from the issues in dispute between scientific realists and their opponents, including a variety of later stipulative decisions about how to revise linguistic usage in light of new information that do not even *attempt* to hold continuity of such usage hostage to a threshold continuity of shared beliefs between users of the term. And the truth of bare existence claims (and/or the referential status of the central terms that figure in them) seems assured more by the extraordinary weakness of the demands we impose for truth (and/or successful reference) in the case of such claims, whether or not there is substantial continuity in our beliefs about the subjects of those claims. Although there are good reasons that cases in which bare existence claims that now seem to us to have been clearly false have played an important role in debates concerning scientific realism, these have from the outset been intended to serve simply as a crude proxy for what really matters: changes of belief sufficiently fundamental as to undermine claims of "approximate truth" for earlier theories. The former have significance only insofar as they help us pick out some especially dramatic and uncontentious cases of the latter, not because they establish or even suggest what would be *required* to vindicate the historicist critic of scientific realism's concerns regarding scientific theories of the present day.

III. DOES ANY SERIOUS DISAGREEMENT REMAIN?

If the historicist is not committed to the falsity of claims like "there are genes" or "atoms exist" and/or the non-referential character of terms like 'gene' or 'atom', just what *is* she claiming about contemporary scientific theories? Could it be that scientific realists and their historicist opponents differ simply in whether they choose to focus on the important continuities between past scientific theories and their successors or instead on the equally important discontinuities?

I do not think so. To see why not, we need only return to what I suggested was the most fundamental motivation held in common by historicist critics of scientific realism: the historical record of scientific inquiry itself. The historicist is convinced by that history that *whether or not* particular existential commitments of current theories are held to be true and *whether or not* particular terms are held to be referential, our own historical successors will someday view even the leading scientific theories of our own day in very much the same way that we regard those of our historical predecessors: as having discovered what later scientific orthodoxy would regard as a wide variety of important and foundational truths about the natural world, but also having embraced many central and foundational beliefs about nature that would ultimately come to seem no less misguided, misleading, or simply mistaken than many of the most fundamental (and referential) claims of Newtonian mechanics or Dalton's atomic chemistry or Weismann's theory of the germ-plasm now seem to us. By contrast, at least the classical scientific realist is committed instead to the idea that future scientists and scientific communities will embrace what will seem both to us and to the members of those communities simply to be expanded, corrected, updated, and/or improved versions of the theories that we ourselves have accepted.[21]

In forthcoming work[22] I argue that because this is the point most fundamentally in dispute between classical scientific realists and their

[21] The strongest pillar in support of scientific realism has always been the "miracle" (or "ultimate") argument that realism offers the best or only explanation for the incredible practical and empirical success of our best scientific theories. As a leading textbook (Robert Klee, *Scientific Inquiry: Readings in the Philosophy of Science* (New York: Oxford University Press, 1999), pp. 313–14) characterizes this argument: "Here is the realist's explanation [of that success]: Our mature scientific theories, the ones used to underwrite our scientific projects and experiments, are mostly correct. What errors our mature theories contain are minor errors of detail."

[22] P. Kyle Stanford, "Catastrophism, Uniformitarianism, and a Realism Debate That Makes a Difference," *Philosophy of Science* (forthcoming).

historicist critics, the dispute itself can be helpfully conceived along the lines of the great clash between Catastrophism and Uniformitarianism in nineteenth-century geology. In that debate, Uniformitarians held that the geographic and topographical features of the Earth were the result of familiar natural causes like floods, earthquakes, and volcanoes acting over immense periods of time at roughly the same frequencies, degrees, and magnitudes that they do now. Catastrophists, by contrast, held that such natural causes operated in considerably stronger magnitudes in the past than they now do, on the order of the difference in magnitude between the floods of our own day and the great Noachian Deluge reported in the Christian Bible, and that such events have steadily diminished in severity, magnitude, and/or frequency over the geological history of the Earth itself. That is, Uniformitarians believed that in the fullness of time even the central features of the Earth's geography and topography would be modified by present-day natural causes just as profoundly and thoroughly as they had been in the past, while Catastrophists instead believed that those central features had been generated by far more violent and dramatic natural events now confined to the Earth's distant past and remained open to further modification by present-day causes only in comparatively much more limited and marginal ways. Like Uniformitarians, historicist critics of scientific realism think that the further progress of scientific inquiry as it has traditionally been practiced will ultimately generate changes in the central commitments of our leading scientific theories just as profound and fundamental as those we find throughout the historical record. Like their Catastrophist counterparts, classical realists view at least the most central and fundamental claims of our most successful scientific theories as firmly established in ways that may be supplemented or modified but are quite unlikely to be overturned in the course of further inquiry.

Accordingly, at least those classical scientific realists who concede that the history of science is indeed characterized by a pattern of widespread, dramatic, and profound changes in our central theoretical beliefs about nature would seem to be committed to a kind of exceptionalism concerning (some or all) contemporary scientific theories. Such realists typically point to characteristics of some contemporary theories intended to protect them from invidious comparison with their historical predecessors (such as their greater "maturity" or their ability to predict novel and/or surprising phenomena) and seek to convince us that theories with these characteristics or exhibiting such especially demanding forms of success should not be expected to share the ultimate fate of their abandoned historical predecessors. If the realist instead simply agrees with her historicist opponent that the future of science will be characterized by the same extent and degree of fundamental theoretical revolution and upheaval as its past, it is hard to understand why she ever resisted her historicist critics in the first place. Did she mean *simply* to insist that because there are profound continuities between past and present theoretical conceptions of nature she thinks it is fair to describe such past conceptions as "approximately true" (or "mostly correct") notwithstanding equally profound discontinuities? If so, there was never anything in dispute in the first place except the proper application of the *term* 'approximately true', and this is certainly not how realists have generally responded to familiar cases of influential scientific theories that were subsequently overturned or abandoned—the central issue has always been whether a similar fate awaits our own theoretical conceptions of nature, not whether there is some conception of 'approximately true' sufficiently liberal as to encompass all such abandoned theories.

It might seem profoundly unfair, however, to tar all recently influential forms of scientific realism with this same exceptionalist brush, for in recent decades a growing number of scientific realists have responded to the mounting evidence of widespread, fundamental change over time in our scientific beliefs by qualifying or limiting their claims of "approximate truth" for some or all of our own scientific theories in ways that seek to recognize important continuities between the likely fates of such theories and those of their historical predecessors. These more sophisticated latter-day forms of scientific realism argue that genuinely successful past scientific theories have not turned out to be simply false but have instead turned out to have true and false parts or components (the true parts or components typically having been responsible for their successes). These sophisticated forms of scientific realism have sought to extrapolate from particular historical cases to a more general view of what parts, aspects, features, or components of theories (for example, just their claims about the "structure" of the world, or just the entities they posit, or just the "working" posits that are actually required for their empirical successes[23]) we should expect to find *preserved* in the transition from any suitably successful theory to its historical successors. On closer examination, however, it seems that such "selective" realists have not so much abandoned the exceptionalist impulse as simply restricted its scope. That is, although such latter-day selective scientific realists

[23] These suggestions are most famously associated with the work of John Worrall, the work of Ian Hacking and Nancy Cartwright, and the work of Philip Kitcher and Stathis Psillos, respectively.

do not claim that contemporary scientific *theories* will stand as exceptions to the broad pattern of repeated and fundamental theoretical change we find throughout the historical record, they nonetheless seek to convince us that we can know in advance which parts, components, or aspects of those theories will remain safely immunized exceptions to any such general pattern of fundamental transformation and upheaval.[24] In short, the realist seeks to argue *either* that some contemporary theories (classical realism) *or* that some parts, aspects, features, or components of those theories (latter-day, selective realism) are likely to constitute exceptions to the broader pattern of repeated fundamental change that characterizes the historical record of scientific inquiry more generally.

At the end of the day, then, what the historicist critic of realism is most fundamentally committed to is the idea that *whether or not* particular existential commitments of current theories are held to be true and *whether or not* particular terms are held to be referential, the central commitments of future theoretical orthodoxy will (or would) ultimately be separated from those of the present by differences *as fundamental, profound, far-reaching, and unpredictable* as those that separate our own theories from their historical predecessors. The most fundamental problem with Weismann's "germplasm," Ptolemy's "wandering stars," and Priestley's "dephlogisticated air" is not that these terms did not *refer* to anything but instead that they have not ultimately turned out to be part of the most practically powerful and successful conceptual apparatus we have for thinking and talking about the phenomena to which they (arguably) *do* refer; the most natural reaction to an antiquated claim such as "heating the red calx of mercury generates dephlogisticated air" is not to insist that (all) such claims were *false* as that "calx of mercury" and "dephlogisticated air" have simply turned out not to be the most useful conceptual categories to deploy in trying to understand those parts of the natural world they purport to describe. Likewise, if one or more of our own theories are ultimately discovered to be fundamentally mistaken, we will not want to say that all or even most of their claims about the bare existence of mutant phenotypes, pure solvents, tectonic plates, distant nebulae, and even genes or atoms were simply false or that such entities did not exist, so much as that the relevant conceptions of 'mutant phenotypes',

[24] Elsewhere (P. Kyle Stanford, "No Refuge for Scientific Realism: Selective Confirmation and the History of Science," *Philosophy of Science*, LXX (December 2003): 913–25; Stanford, *Exceeding Our Grasp*, *op. cit.*, chapters 6–7) I argue in detail against this claim, but here I am simply concerned to identify the most fundamental points of actual disagreement between realists and their historicist critics.

‘pure solvents’, ‘tectonic plates’, ‘nebulae’, ‘genes’, and/or ‘atoms’ have turned out not to be the most useful conceptual tools with which to engage phenomena in these domains after all. Thus, what ultimately matters is not whether some future community will judge the claim “there are atoms” as uttered by early-twenty-first-century scientists to have been true, but whether future scientific beliefs *about* atoms will be separated from our own by differences as profound and fundamental as those separating the successive conceptions offered by Democritus, Proust, Dalton, Perrin, Thompson, Rutherford, Bohr, Heisenberg, and contemporary quantum mechanics. Those who agree that they will be and that there is no reliable way to identify in advance which features, aspects, or components of our own theoretical conceptions of the atom will be preserved through such transformations might wish to retain the realist label, but they will have diluted their supposed scientific realism into something so weak that, to borrow a phrase, no historicist opponent will think it worthwhile to contend against it.

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