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Scientific Realism, the Atomic Theory, and the Catch-All Hypothesis: Can We Test Fundamental Theories Against All Serious Alternatives?

P. Kyle Stanford

ABSTRACT

Sherri Roush ([2005]) and I ([2001], [2006]) have each argued independently that the most significant challenge to scientific realism arises from our inability to consider the full range of serious alternatives to a given hypothesis we seek to test, but we diverge significantly concerning the range of cases in which this problem becomes acute. Here I argue against Roush's further suggestion that the atomic hypothesis represents a case in which scientific ingenuity has enabled us to overcome the problem, showing how her general strategy is undermined by evidence I have already offered in support of what I have called the 'problem of unconceived alternatives'. I then go on to show why her strategy will not generally (if ever) allow us to formulate and test exhaustive spaces of hypotheses in cases of fundamental scientific theorizing.

1 Roush, Stanford, and Unconceived Alternatives

A number of recently influential challenges to scientific realism have embodied an important turn away from the idea that the limits of observability mark a crucial epistemic boundary distinguishing justifiable from unjustifiable scientific beliefs. In its place, it has been suggested that the scope of justifiable scientific

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belief is limited instead by a concern for our ability to effectively consider the space of *alternatives* to a hypothesis we seek to evaluate and to consider their empirical consequences. Sherri Roush ([2005]), for example, has argued that assessing the confirmation of a scientific hypothesis requires us to evaluate the likelihood conferred on the evidence we have by the 'catch-all' hypothesis consisting simply of the *negation* of the original hypothesis (i.e., the term P(e/-h)), and that assessing this term responsibly is something we are typically if not invariably unable to do in the case of what she calls 'high-level' theories. And I myself ([2001], [2006]) have grounded a more general argument against realism in the claim that the historical record of scientific inquiry itself provides us with abundant empirical evidence that there are probably scientifically plausible alternatives to even the best contemporary fundamental scientific theories that are equally well-confirmed by the evidence available to us but that simply remain unconceived by contemporary scientists. For both Roush and me, then, it is not claims about unobservable entities or events (at least, not as such) which are insufficiently justified to warrant belief, but instead claims made by scientific theories or hypotheses when we have good reasons to doubt that we have a firm grasp of what the serious scientific alternatives to those hypotheses are and what empirical consequences they have.

As we both acknowledge, however, this general epistemic challenge can be met in a wide variety of cases, including many scientific ones. My simple example ([2006], p. 32) points out that when we set out to test among the competing hypotheses that per capita alcohol consumption among American high school students has increased, decreased, or remained steady over the last decade, there is simply no room to worry that there are important alternative possibilities we are failing to consider. And Roush uses a much more sophisticated example to argue convincingly that we are sometimes able to evaluate the likelihood conferred on our evidence by the catch-all hypothesis even for claims concerning unobservable entities and events. As she points out, when a blood test detects the hormones characteristic of the unobservable earliest stages of pregnancy, we know the likelihood conferred on a positive test result both by the hypothesis that the test subject is pregnant *and* by the negation of that same hypothesis: we estimate the likelihood of a positive result when the subject is not actually pregnant in the standard ways that we uncover the rate of false positives for any diagnostic test, by testing random or representative samples of the population at large. Roush and I seem to agree, then, not only that the problem of testing a hypothesis against a knowably exhaustive space of alternatives (or of reliably estimating the likelihood conferred on the evidence we have by the catch-all negation of a hypothesis) can be convincingly solved in a variety of scientific contexts, but also that our ability to do so simply cross-classifies the distinction between observables and unobservables that opponents of realism have traditionally regarded as so central.

At first blush, it might seem that we also agree on when and where this epistemic problem becomes acute. I argue that the historical evidence supporting the 'problem of unconceived alternatives' reveals that it offers a substantial challenge to what I call 'fundamental theories of nature', that is, to our theories 'about the fundamental constitutions of the various domains of the natural world and dynamical principles at work in those domains', or more concretely, 'when we theorize about such matters as the constitution of matter itself, the remote history of the Earth and its inhabitants, the most minute workings of our bodies, and the structure of the farthest reaches of the universe' ([2006], p. 32). This would certainly seem to overlap with Roush's estimate of the scope of the problem: she argues that while we have convincingly evaluated the likelihood conferred on our evidence by the negation of a hypothesis 'for hypotheses that go beyond observables, it has been only slightly beyond, and any claim that we have evaluated this term for high-level theories like Quantum Mechanics would be preposterous' ([2005], p. 193), adding that her analysis shows why 'anti-realists were always right to doubt that we have actually got much further than [confirming claims about observables]; further than that is very hard to get' ([2005], p. 194). But a closer look reveals that the similarity here is more apparent than real, and that it would be a deep mistake to identify Roush's 'high-level theories' with the products of what I have called 'fundamental theorizing'. In the final analysis, it turns out that Roush and I remain deeply divided on the question of what sorts of theories or hypotheses are vulnerable to the problem, and we disagree about cases that are central to ongoing disputes concerning scientific realism.

As part of her argument that we should reject philosophically motivated prior restraints on what advances in scientific methodology may be able to accomplish, Roush offers a detailed analysis of a much more dramatic example than pregnancy-testing in which she claims we have indeed managed to confirm hypotheses that go beyond observables: Jean Perrin's efforts near the turn of the twentieth century to confirm the atomic theory of the constitution of matter by a careful experimental investigation of the phenomenon of Brownian motion. Thus, while the atomic hypothesis surely falls under the description I give of 'fundamental theorizing', Roush apparently does not regard it as the sort of 'high-level theory' that we are unable to confirm against an exhaustive space of alternative possibilities. Thus, even those of us who agree about the character of the real challenge for scientific realism seem to disagree about where this problem applies or becomes acute, and this crucial residual disagreement comes perspicuously into focus concerning Roush's central example of the atomic theory of the constitution of matter.

The first task of this paper will be to consider Roush's analysis of this illuminating case. I will argue that she mischaracterizes the example in a number of ways that matter, and that the most important of these illustrate precisely how her conclusion regarding what Perrin managed to accomplish is undermined by some of the same historical evidence I have already marshaled in support of the problem of unconceived alternatives. I will then go on to argue that the case suggests a general moral concerning when we should and should not expect to be able to confirm theoretical hypotheses against a knowably exhaustive space of alternatives, one that supports my own more expansive conception of the scope of the problem.

It is perhaps worth noting explicitly that the sort of realism at issue here is not the only variety that might be thought interesting or important: we are not here concerned, for example, with the question of whether or how we know there is even a real world with which our best scientific theories are able to conflict. The issue that concerns Roush and myself is instead whether we are justified in believing that the claims made by our best scientific theories about various otherwise inaccessible domains of nature are at least probably and/or approximately true (whether such truth is conceived of in terms of correspondence, deflationism, convergence in the limit of inquiry, or whatever). That is, should we believe a claim about nature if the only reason we can give for doing so is that it is part of or follows from an empirically successful scientific theory? Our concern with this sense of realism will affect the sorts of considerations that we take to be relevant to confirmation in this context. We will insist as a requirement for confirmation, for example, that we be able to justify attributing a relatively high probability to a hypothesis or theory given all the evidence we have and a relatively low probability to the catch-all alternative: although this is not a requirement for confirmation on any possible account of the matter, it is at least a plausible necessary condition for claiming that we have good reason to believe that the account of some otherwise inaccessible natural domain provided by a theory is actually true.

2 Perrin and Brownian Motion

The phenomenon of Brownian motion consists of the constant, erratic motion of particles suspended in a liquid that are large enough to be seen under a microscope but still small enough to register the impacts of hypothesized collisions with individual atoms and/or molecules making up the liquid. The phenomenon itself was well known and extensively studied for nearly a hundred years before Perrin (following some ideas of Einstein's) sought to use it as a convincing experimental demonstration of the truth of the atomic theory of matter.

Of course, Perrin's experimental work on Brownian motion was only part of a much larger systematic and powerful case he offered for the truth of the atomic hypothesis. Indeed these experiments are sometimes appealed to as just a single element in the larger confirmational strategy of arguing from a variety of phenomena to a common cause implicated in each of them, in this case of the similar values calculated for Avogadro's number from a wide range of independent phenomena (cf. Salmon [1984], following the historical account given in Nye [1972]). But according to Roush, Perrin was able to provide convincing confirmation for the atomic hypothesis even before the famous step in his argument where he offers this variety of independently calculated values for Avogadro's number. Instead, Roush argues that Perrin's careful experimental investigation of Brownian motion *alone* sufficed to substantially confirm the 'modest hypothesis that there are atoms and molecules, understood merely as spatially discrete sub-microscopic entities moving independently of each other, i.e., at random' ([2005], p. 219). That is, Roush argues that Perrin managed to confirm this 'modest' atomic hypothesis without any recourse to the confirmational strategy of arguing to a common cause.

The genius of Perrin's investigation, she explains, was to find evidence that not only had a high likelihood conferred on it by the modest atomic hypothesis, but that we could also know to have a very low likelihood conferred upon it by the catch-all, that is, by the negation of that same hypothesis, *without the need to formulate any of those alternatives individually*:

> If there are atoms [...] then the motion of the Brownian particles will be a random walk, that is, that motion will exhibit no systematic effects, no dependencies or correlations between the motions of one particle and another or tendencies in the motion of a single particle. Since the modest atomic hypothesis is so devoid of detail, for example, as to the structure and precise size of atoms, there do not seem to be any hypotheses that could explain a random walk in the Brownian particles that are not included within this atomic hypothesis. The hypothesis of atoms and molecules is not equivalent to the hypothesis that Brownian motion is fully random, but it is close to being so. Thus, what Perrin had to verify in order to confirm that there are atoms and molecules was that the motion of Brownian particles has no systematic effects.
>
> The possibilities that Brownian motion is a random walk and that it exhibits a systematic effect exhaust the logical space, since the motion is either random or it is not. ([2005], p. 219)

Roush is certainly right to suggest that 'random or not' exhausts the space of possibilities for the Brownian motion, but notice that in the passage above she parlays this into conclusive confirmation for the atomic theory only by means of a crucial further assumption: that 'there do not seem to be any hypotheses that could explain a random walk in the Brownian particles that are not included within this atomic hypothesis' and thus that '[t]he hypothesis of atoms and molecules is [close to] equivalent to the hypothesis that Brownian motion is fully random'.[1] That is, Roush argues that Perrin's demonstration that the motion of Brownian particles is indeed a true random walk was sufficient to establish what she calls the 'modest atomic hypothesis' precisely because it enabled us to know not only the likelihood conferred on his experimental results by the presumption that the modest hypothesis was true, but also the likelihood of the evidence on the negation of the atomic hypothesis as well, and together these form a mutually exclusive and exhaustive set of possibilities. But this ability depends absolutely on her further claim that nothing *besides* an atomic structure for matter could explain a random walk by the Brownian particles.

As we noted above, the argument from the randomness of Brownian motion was far from the whole of Perrin's case for the atomic theory, but it is perhaps reasonable for Roush to search among Perrin's arguments for one with a special probative or dispositive force. As several philosophers have pointed out (see esp. Miller [1987] and Maddy [1997], [2007]), Perrin's work converted a large and influential group of working scientists, including trenchant skeptics like Ostwald and Poincaré, from instrumentalism to realism about the atomic theory, but the distinctive varieties of confirmation that philosophers of science have traditionally regarded Perrin's work as conferring on the atomic theory are ones that it was already known to enjoy in substantial degree well *before* Perrin's famous experiments:

> Philosophers of science have reconstructed the case for atoms in a bewildering variety of ways: atoms are indispensable posits of a theory with simplicity, familiarity of principle, scope, fecundity, conformity with experimental tests (Quine); we infer the existence of atoms as the best explanation of the various phenomena (Harman); the existence of atoms is part of the only plausible explanation of the success of our scientific theories (Boyd); we know atoms exist because they cause observable effects (Cartwright). Unfortunately all these general accounts founder on the same rock as Quine's: the atomic hypothesis already enjoyed the preferred features by 1900; the Einstein/Perrin evidence 'should have been an anti-climax [...] simply more of the same' (Miller [1987], p. 470). [Wesley Salmon (following Mary Jo Nye) and more recently Peter Achinstein] attend more carefully to the particulars of Perrin's experiments, but in both cases, the type of evidence highlighted was already available [...] again this would appear to be 'simply more of the same'. (Maddy [2007], p. 404; footnotes omitted)

[1] Roush cannot, of course, mean that the *content* of the atomic hypothesis is equivalent or close to being equivalent to the randomness of the Brownian motion (which it is supposed to explain). What she seems to have in mind instead is the suggestion that we can freely infer from the truth of truly random Brownian motion to the truth of the atomic hypothesis (as its only possible or plausible explanation), and thus that the demands for convincing *confirmation* posed by the two hypotheses are in this sense equivalent or nearly so.

In this context, Roush's claim that the randomness of Brownian motion sufficed on its own to confirm the atomic hypothesis becomes highly suggestive. If she is right, perhaps we can solve the mystery of why Perrin's case was not regarded as just 'more of the same' and managed to convince nearly all of his contemporaries where earlier efforts had failed to do so. While Roush's central concern is not with the historical question of what ultimately convinced the skeptics, she is very much concerned to defend the claim that Perrin's experimental demonstration of the randomness of Brownian motion enjoyed a special evidential significance.[2]

Roush's suggestion here is somewhat reminiscent of Richard Miller's efforts to defend realism about particular scientific theories piecemeal in the late 1980's by means of what he called 'topic-specific truisms'. Indeed, Perrin's work on Brownian motion was a flagship case for Miller, too, in which he argued that one such truism in conjunction with the available evidence left us without any reasonable alternative to embracing a similarly modest form of the atomic hypothesis ([1987], pp. 476, 480).[3] Thus, we might consider the response offered by Arthur Fine to Miller's strategy as applied to this particular episode. Fine glosses the relevant topic-specific truism to which Miller appeals in this case as 'Non-living matter does not jump about erratically unless something external is moving it about', and notes that it is taken to ground a further inference: 'If a non-living thing is in constant erratic motion, that is a reason to believe it is constantly being moved to and fro by an external agent' (Fine [1991], pp. 88–9). But Fine goes on to point out that 'Miller's truism, which in the light of the quantum theory is not true, also was not considered to be true in physics, even *prima facie*, during the period of concern' ([1991], pp. 91–2). More concretely, Fine insists that the physics community of the early twentieth century had specific reasons for doubt about the local truism Miller formulates and the associated inferential move from the erratic motion of Brownian particles to the existence of an 'external agent' bouncing them around:

[2] A referee for this journal has helpfully pointed out several respects in which Perrin's empirical case was qualitatively novel, though it is not clear to me whether Miller and/or Maddy would count these as 'more of the same' or not. I will not consider the matter further here, as our primary concern is with whether Roush is right to suggest that the randomness of Brownian motion alone sufficed to confirm the atomic theory and did so by ruling out the alternatives without the need to formulate them individually. Of course, if these claims are rejected we must remain open to the possibility that Perrin's results did simply add incrementally to the already impressive support for the atomic theory, but represented a sufficient improvement of quantitative and/or qualitative detail to convince (rightly or wrongly) the erstwhile skeptics.

[3] John Worrall has offered a related analysis of Newton's method of 'deduction from the phenomena', suggesting in effect that the method involves deduction of theoretical claims from the phenomena *along with* background assumptions that are uncontentious because shared by all parties to a particular scientific debate. But Worrall is well aware of the perils of this strategy (see below).

> In the case of Brownian motion, two sources of specific doubt stood in the way, historically, of granting the applicability of the truism, even *prima facie*. The first has to do with the electro-magnetic view of matter, long the dominant view, and arguably so in the period in question. It was, for example, the view of Lorentz, who was Einstein's scientific patron saint in 1905 and even much later. This view would lead one to look not for external movers banging the Brownian particles about, but for the interplay of electrostatic forces among the particles themselves, in conjunction with exchange forces with the medium. This idea had a good deal of life to it, and until it was fully played out Miller's truism would not have had any special pull with scientists of the time. In 1905–15, it didn't. That period, moreover, especially in German science, marked the beginning of the decline of the classical causal world view. By 1913, as Miller notes, Bohr was free to introduce his atomic model with its uncaused orbit-jumping electrons. But before then the scientific air was full of the idea that maybe causality had had its day. Thus it would by no means have seemed unreasonable to wonder whether Brownian phenomena weren't just the sort of thing that might succumb to an analysis involving a fundamental randomness in the behavior of material objects. A scientist proposing to work on such a project in 1905, or holding out for a program that would involve such work, might not have been in the mainstream of physics, but he would not have been considered on the lunatic fringe either, which is where Miller would place him. ([1991], pp. 91–2)

Of course, the alternative theoretical possibilities Fine notes here serve at least equally well to undermine Roush's claim that '[t]he hypothesis of atoms and molecules is not equivalent to the hypothesis that Brownian motion is fully random, but it is close to being so'. That is, the recognition of serious scientific alternatives to the atomic hypothesis that also confer a high likelihood on Perrin's evidence threaten to turn the razor-thin gap Roush recognizes between the random walk of the Brownian particles and even a modest version of the atomic hypothesis she describes into a yawning chasm instead. It is simply false that 'there do not seem to be any hypotheses that could explain a random walk in the Brownian particles that are not included within this atomic hypothesis' and historical investigation does not have far to seek in order to uncover at least some of them.

Of course, the bare appeal to 'electrostatic forces' does not immediately or straightforwardly entail random motion of the Brownian particles: this would depend in turn on the characteristics of the electrostatic forces and their interaction with the exchange medium. Fine is perhaps best thought of as identifying a broad *category or class* of alternative hypotheses one of whose members plausibly includes an alternative explanation of truly random Brownian motion that puts electrostatic interactions in the role assigned to particulate collisions by the atomic theory. Moreover, the details of the suggested alternatives will be important: note, for instance, that while such a hypothesis of electrostatic forces could plausibly recapture Perrin's further elegant explanation for the vertical distribution of Brownian particles at equilibrium (assuming an increased rate of electrostatic interaction with increasingly dense concentration of the Brownian particles themselves), it is not easy to see how the alternative appeal to 'fundamental randomness in the behavior of material objects' could do so.

In light of these qualifications, it might seem that charity invites us to read Roush as more modestly insisting simply that there are no alternative explanations for the randomness of Brownian motion that could have fit in with the rest of the case Perrin presented. But this cannot be her argument: her claim is that Perrin's genius (and the key to the effectiveness of his case) lay in managing to eliminate the alternatives to the atomic theory *without the need to formulate them explicitly or individually in the first place*, and the question of consistency with the rest of his case or evidence cannot even be evaluated unless such alternatives are first given explicit formulations. Her argument, then, must be that there is no scientifically plausible alternative explanation of truly random Brownian motion full stop, and that this in turn is why the randomness of the Brownian motion was alone sufficient to substantially confirm the modest atomic hypothesis. Accordingly, we are not here primarily concerned with whether Fine's or other alternative theoretical explanations of the randomness of Brownian motion would have fit equally well with the rest of Perrin's case for the atomic theory, or whether that case was on the whole sufficient to justify a conversion from instrumentalism to realism about atoms, but instead with the cogency of Roush's claims that (i) nothing besides the atomic hypothesis could explain the randomness of Brownian motion; (ii) this allowed Perrin to eliminate the alternatives to the atomic theory without formulating them individually; and (iii) the demonstration of true randomness in the Brownian motion was therefore alone sufficient to confirm the atomic hypothesis. And Fine's alternatives serve perfectly well to demonstrate why these claims are unfounded.[4]

Perhaps Roush would instead want to suggest in reply that the further possibilities we have considered do not actually conflict with the atomic hypothesis in the appropriately modest form she has given it. When she introduces the modest atomic hypothesis, after all, she does go on to say that

> The sense in which these entities [atoms and molecules] are spatially discrete must be understood as vague, to accommodate the possibility, later discovered, that atoms exhibit the quantum mechanical property of not being fully localized, which did not make us cease to believe there are atoms and molecules, and was not intended to be excluded by the modest atomic hypothesis. ([2005], p. 219)

[4] My sincere thanks to two anonymous referees for this journal for helping me to substantially clarify these issues.

It is hard to know what we should make of this reservation. It is certainly true that we have retained the *word* 'atoms' for hypothesized constituents of matter within the quantum theory, which implies that at some point it seemed to us better to preserve linguistic continuity with earlier accounts and that sufficient conceptual continuity existed to make this possible. But there is no reason to think that Perrin or other nineteenth and early twentieth century atomists explicitly or implicitly reserved judgment on the question of the spatial localizability of the atoms they postulated. It is only in light of later theoretical developments that we are even tempted to go back and qualify the beliefs to which Perrin's experiments supposedly entitled us in the first place in this way, and such retrospective retelling of the story threatens to treat the modest atomic hypothesis simply as a placeholder or a bare name for whatever further inquiry ultimately decides about the causes of the phenomena that occasioned its introduction. Perhaps more importantly, however generously we qualify the 'modest' atomic hypothesis, it seems that we will be unable to encompass the further theoretical possibilities Fine describes without eviscerating that hypothesis by construing it so broadly as to include electrostatic fields and/or uncaused random motion of the Brownian particles as potential referents for the 'atoms' it postulates. If we do this it is hard to see what the modest atomic hypothesis excludes, or, therefore, why its supposedly hard-won confirmation should be treated as any sort of epistemic accomplishment.

What is ultimately most important about the alternative theoretical possibilities Roush has here neglected, however, is less the conceptual distance they point out separating the randomness of Brownian motion from the modest atomic hypothesis than what they illustrate about the *kind of inference* on which Roush relies when she overlooks it. She reasons that the likelihood of the evidence on the catch-all hypothesis is low simply because *she cannot conceive of* any plausible alternative to the modest atomic hypothesis that would also predict a random walk for the Brownian particles, or any other way in which truly random Brownian movement could be produced. But some of the evidence I originally offered in support of the problem of unconceived alternatives suggests that *this very pattern of reasoning is demonstrably unreliable in the domain of fundamental scientific theorizing*. Let us see how.

3 Retention and Possible Alternatives: New Lessons from Some Familiar History

What I called ([2001], [2006]) the New Induction over the History of Science seeks to show that even when we can conceive of only a single well-confirmed theoretical explanation for a particular set of natural phenomena the historical record suggests that there typically *are* multiple scientifically serious and well-confirmed alternative theoretical explanations remaining unconceived by us.

In the course of defending this thesis from both existing and possible realist replies, I confronted cases in which scientists themselves judged that particular aspects or elements of their own theories would have to be retained in any successful successors, and I argued that the historical evidence reveals even the sincere convictions of expert scientists on this question to be demonstrably unreliable. But it turns out that it is characteristically *because* a scientist judges that there is no possible alternative to a given line of explanation for a particular set of phenomena that she insists some aspect or element of a given theory will have to be preserved in any successful successors. Thus, some of the very *same* historical evidence I have used to try to show that scientists' own convictions concerning which parts of their theories must be preserved in their successors are unreliable ([2003a], [2003b], [2006]) will serve equally well to undermine the form of inference on which Roush must rely to hold the atomic hypothesis confirmed against an exhaustive space of all the serious alternatives.[5]

Consider, for example, James Clerk Maxwell's argument for the claim that there is simply no alternative to postulating the existence of a substantival ether if we are to account for the propagation of energy waves. Concluding *A Treatise on Electricity and Magnetism*, Maxwell writes:

> If something is transmitted from one particle to another at a distance, what is its condition after it has left the one particle and before it has reached the other? If this something is the potential energy of two particles, as in Neumann's theory, how are we to conceive this energy as existing in a point of space, coinciding neither with the one particle nor with the other? *In fact, whenever energy is transmitted from one body to another in time, there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other*, for energy, as Torricelli remarked, 'is a quintessence of so subtile a nature that *it cannot be contained in any vessel except the inmost substance of material things*.' Hence all these theories lead to the conception of a medium in which the propagation takes place, and if we admit this medium as an hypothesis, I think it ought to occupy a prominent place in our investigations, and that we ought to endeavor to construct a mental representation of all the details of its action, and this has been my constant aim in this treatise. ([1873/1904], p. 493; my emphasis, cited in Stanford [2006], p. 152)

Here Maxwell reports that he finds it impossible to conceive of how the wave-like propagation of light and electromagnetism could occur without a substantival medium in which those waves are propagated. By Roush's lights, this would entitle him to conclude that the likelihood conferred on his evidence by the negation of a 'modest' ethereal hypothesis is very low and that such a

[5] This is certainly not to suggest that Roush has missed or ignored some aspect of my earlier argument. I did not explicitly draw this particular moral from the historical evidence, and Roush was not responding to my discussion in any case, but the evidence to which I appeal nonetheless serves to undermine the inference on which she relies in the way I go on to describe below.

substantival medium actually exists. But of course, subsequent scientific history has revealed theoretical possibilities that were simply beyond Maxwell's ability to imagine. Still, it is this very same form of inference on which Roush must rely to reach the conclusion that the likelihood conferred on the randomness of Brownian motion by the negation of the atomic hypothesis is low: she reasons that nothing *besides* an atomic structure for matter could cause truly random motion in the Brownian particles simply because she cannot conceive of any other way in which truly random motion could be produced.

Of course, Maxwell's appeal to this form of inference is anything but an isolated incident in the history of theoretical scientific inquiry. In his defense of the caloric theory of heat, for example, Antoine Lavoisier argues that the caloric fluid must exist because he can conceive of no other way in which a wide variety of thermodynamic phenomena could be caused. Concerning the thermodynamic expansion and contraction of bodies, for example, Lavoisier writes in his 1785 'Memoir on Phlogiston' that

> One can hardly think about these phenomena without admitting the existence of a special fluid [whose accumulation causes heat and whose absence causes cold]. It is no doubt this fluid which gets between the particles of bodies, separates them, and occupies the spaces between them. Like a great many physicists I call this fluid, whatever it is, *the igneous fluid, the matter of heat and fire.* (Lavoisier (1785) as translated in Donovan [1993], p. 171; original emphasis, translation modified; as cited in Stanford [2006], p. 154)

And in the posthumous *Traité de Chimie*,

> It is difficult to conceive of these phenomena without admitting that they are the result of a real, material substance, of a very subtle fluid, that insinuates itself throughout the molecules of all bodies and pushes them apart. [...] This substance, whatever it is, is the cause of heat, or in other words, the sensation that we call heat is the effect of the accumulation of this substance [...]. (*Traité de Chimie*, in Lavoisier [1965], volume 1, pp. 1–3, as translated in Stanford [2006], p. 154)[6]

Finally, consider August Weismann's argument for the claim that the germinal material *must* be divided into qualitatively different portions and distributed throughout the cells of the body if we are to explain the fact that different cells come to possess different characteristics in the course of ontogeny:

[6] In addition to the phenomena of thermodynamic expansion and contraction explicitly under discussion in these passages, Lavoisier offered explanations for a variety of thermodynamic phenomena that could not be straightforwardly translated into the terms of the competing 'dynamical' account of heat as motion (see below). Perhaps most important among these were the phenomena of state (solid, liquid, aeriform fluid) and changes in state of matter, which Lavoisier explained by the chemical *combination* of substances with the caloric fluid. For more details, see Stanford ([2006], pp. 176–9).

> As the thousands of cells which constitute an organism possess very different properties, *the chromatin* which controls them *cannot be uniform; it must be different in each kind of cell.*
>
> The chromatin, moreover, cannot *become* different in the cells of the fully formed organism; the differences in the chromatin controlling the cells must begin with the development of the egg-cell, and must increase as development proceeds; for otherwise the different products of the division of the ovum could not give rise to entirely different hereditary tendencies. This is, however, the case. Even the first two daughter-cells which result from the division of the egg-cell give rise in many animals to totally different parts. [...] The conclusion is inevitable that the chromatin determining these hereditary tendencies is different in the daughter-cells. ([1893], p. 32; all emphases original, cited in Stanford [2006], pp. 153–4)

Here Weismann argues that each cell of the body must inherit a qualitatively different portion of the organism's hereditary material, as he can think of no other way in which the development of such cells could even possibly be controlled by this hereditary material and yet allow them to come to possess very different characteristics.

The point here is not that these thinkers were simply unable to conceive of any possible alternatives to the hypotheses they championed, nor is this claim even true: Lavoisier took very seriously the alternative 'dynamical' account of heat as motion, for example, while Weismann knew that many of his contemporaries embraced the view that the germinal material was reproduced entire and complete in each (somatic) cell of an organism. But in each of these cases we nonetheless find the argument *that a particular group or class of phenomena* simply cannot be explained except by means of a given hypothesis, which therefore must be accepted. In Maxwell's case this phenomenon was the wavelike propagation of electromagnetism, in Weismann's case it was the differentiation of cells throughout the course of ontogeny, and in Lavoisier's case it was a variety of thermodynamic phenomena prominently including those involving expansion and contraction. In each case we find eminent scientists inferring that the natural world must have a certain structure or contain certain entities simply because they cannot conceive of any possible alternative means or mechanism whereby a particular phenomenon or set of phenomena could have been produced. And each of these cases testifies to the unreliability of such inferences in the domain of fundamental scientific theorizing.

In a precisely parallel fashion, Roush argues that nothing *but* the atomic hypothesis can explain random Brownian motion, and that demonstrating the movement of the Brownian particles to be truly random was therefore tantamount to confirming the atomic hypothesis itself. But we simply have no reason to think that what Roush finds it easy or even possible to imagine today is any better guide to what the world or the space of possible theoretical explanations for some phenomenon must be like than what Maxwell was able to imagine in 1873, Lavoisier in 1785, or Weismann in 1892.[7] Thus, even if we set aside the specific alternatives to the atomic theory of matter that Fine points out and Roush neglects, Roush cannot reach the conclusion that the likelihood conferred on the randomness of Brownian motion by the negation of the atomic hypothesis is low, or that Perrin's demonstration that the motion is a true random walk therefore suffices to substantially confirm the modest atomic hypothesis against an exhaustive space of possible alternatives, without relying centrally on a form of inference whose reliability is severely challenged by the very same historical evidence I have elsewhere used to demonstrate the unreliability of scientists' own convictions about which elements of the theories they support will have to be retained in any successful successors. Roush is quite right to insist that Brownian motion is either a random walk or not, and that Perrin showed convincingly that it is, but quite wrong to think that she is entitled to treat this as equivalent to or sufficient to confirm even a 'modest' version of the atomic hypothesis against all possible alternatives simply because she cannot imagine how else such a phenomenon could even possibly be produced.

4 Whither Exhaustion?

Even if Roush has not made a convincing case regarding the particular example of the atomic hypothesis, however, I have conceded that Perrin was able to test among at least one exhaustive space of possibilities: viz., the Brownian motion is either random or not. Although this would seem to be neither an instance of what I called 'fundamental theorizing' nor of Roush's own 'high-level theories', it might nonetheless encourage us to expect that we will prove able to test among similarly exhaustive spaces of hypotheses in other cases that do fall into these categories.

In fact, I think this case offers convincing support for just the opposite conclusion. Notice that the space of hypotheses Perrin considers is exhaustive in a very special way—it consists simply of a hypothesis and its negation: random motion or not. When it comes to fundamental theoretical science, the problem is not that we cannot formulate exhaustive spaces of hypotheses in this way, and even know them to be exhaustive, but that when we do we will not generally be able to test them because we will not know what empirical predictions or evidence to associate with the negations. It is actually an easy matter to generate exhaustive spaces of hypotheses in the way Roush suggests:

[7] Interestingly, a similar moral seems to apply to Worrall's ([2000]) analysis of Newton's 'deduction from the phenomena'. As Worrall points out in the example he uses to illustrate the method, elements of the uncontentious background knowledge shared by all parties to the debate (specifically, the assumption that light comes in discrete 'parts' which if left to themselves are propagated in straight lines) that are used along with the phenomena to 'deduce' the theory have subsequently been rejected.

Newtonian mechanics is true, or not; Weismann's theory of the germ-plasm is true, or not; the atomic hypothesis is true, or not. But in each case we have no idea what empirical phenomena or consequences are implied by just the *negation* of the relevant fundamental hypothesis, and therefore we don't know what evidence would *discriminate* between members of the exhaustive hypothesis-pairs we seek to test. The simple hypothesis that Newtonian mechanics is false, for instance, does not give us any reason to expect gravitational light-bending, or the precession of the perihelion of Mercury, or any of the phenomena that ultimately served to confirm relativistic cosmology in its place, even when supplemented with any relevant auxiliary hypotheses we might also have accepted. We are similarly at a loss to say what we should expect in nature if our only relevant hypothesis is the falsity of the atomic theory of matter, or Weismann's theory of the germ-plasm, or the modern synthesis of Mendelian genetics with Darwinian evolutionary theory. These simple negations each stand proxy for a loose and open-ended disjunction of disparate hypotheses, only a few of which we know how to spell out at any given time. But we derive empirical predictions from a specific positive account of how some domain of nature works, and an important part of the challenge to realism posed by the problem of unconceived alternatives is that there seems to be no obvious way for us to formulate collections of such *positive* proposals that we can have good reason to think exhaust the space of theoretical possibilities. Robbed of Roush's inference from our inability to conceive of any alternative to a given hypothetical means of producing the evidence we have to the prediction of a low likelihood of such evidence from the catch-all complement to that hypothesis, it seems that we will not in general be able to responsibly predict *anything* from the catch-all complements of fundamental theoretical hypotheses. Thus, Roush's perfectly correct claim that Perrin manages to test the hypothesis that Brownian motion is truly random against an exhaustive field of serious alternatives actually serves to illustrate why this will not generally (if ever) be possible in the cases that matter most for the dispute over scientific realism.

5 Conclusion

More generally, it seems that the one case Roush singles out as a convincing example of how sufficient scientific ingenuity can indeed allow us to test what I have suggested is a fundamental theoretical hypothesis against an exhaustive space of alternative possibilities actually turns out in the end to support just the opposite moral instead. Perrin did establish convincingly that the Brownian motion is truly random, but Roush's further claim that there are no theoretical possibilities besides the atomic hypothesis capable of producing such a random motion (and thus that the likelihood conferred on this evidence by the negation of the atomic hypothesis is low) is flawed in several ways. First, it ignores the sorts of actual historical alternatives to the atomic hypothesis that Fine identifies as potentially live contenders among physicists themselves at the time of Perrin's investigation that were capable of explaining the genuinely random motion of Brownian particles. Second, it absolutely depends upon inferring from the fact that she cannot conceive of even a possible alternative to the atomic hypothesis capable of accounting for such random motion to the conclusion that the probability of there being any such alternative is low, but some of the same historical evidence originally offered in support of the problem of unconceived alternatives can be used to convincingly challenge this very form of inference. Finally, this case itself illustrates why Roush's general strategy will not generally, if ever, be applicable in cases of what I have called 'fundamental theorizing': although we can divide the space of possibilities exhaustively into a hypothesis and its negation, we generally have no idea what empirical consequences to draw simply from the negation of a fundamental theoretical hypothesis, and therefore no way to test among the admittedly exhaustive alternatives in a space of theoretical possibilities formulated in this way. For all these reasons I think we must reject not only the suggestion that the atomic hypothesis serves as an example in which scientific ingenuity has enabled us to formulate and test among an exhaustive space of alternatives in the case of what I have called fundamental theorizing, but also any suggestion that the general strategy at work in the example might render the problem tractable more generally in the sorts of fundamental theorizing on which the central issues turn in the ongoing debate concerning scientific realism.

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