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Comments on Roush, *Tracking Truth*

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In recent decades, analytic epistemology and the philosophy of science have proceeded more or less in regrettable isolation from one another, so perhaps the most important point to make about *Tracking Truth* is that it serves as a model of just the kind of thoughtful and sophisticated engagement from which both fields stand to reap tremendous benefits. Sherri takes the details and distinctive features of scientific knowledge-gathering seriously, but her account also strives for generality, seeking to make robust sense of our tendencies to ascribe and deny knowledge to dogs, theoretical physicists, and everything in between, including the familiar victims of a wide variety of unfortunate Gettier-style predicaments. She bids fair to do that, while offering perceptive contributions to ongoing debates in both epistemology and philosophy of science throughout, including one of the most attractive analyses I have seen of what we should say (and what we should most certainly *not* say) about the possibility of global or Cartesian skepticism. This is just the kind of reengagement that both epistemology and philosophy of science could use a lot more of, and I hope and expect that Sherri's book will inspire many others to take up the challenge.

Having thus celebrated Sherri's shining example, of course, I will now cravenly turn tail and flee in precisely the opposite direction, confining my remarks today within my own narrow relevant expertise concerning disputes over scientific realism.

Fortunately, some of Sherri's most important and exciting claims consist in the philosophical morals she draws from the account of confirmation developed throughout the book for both realist and antirealist positions in the philosophy of science.

As Sherri notes, what matters for the realism dispute is the absolute, rather than merely relative, degree of confirmation enjoyed by our various scientific theories or hypotheses. And the central implication of her likelihood ratio account of confirmation is that to assess the absolute degree of confirmation of an hypothesis we must know not only the likelihood conferred on the evidence we have by that hypothesis, but also the likelihood conferred on that same evidence by the negation of that hypothesis, that is, by the so-called "catch-all" hypothesis collectively representing all the various alternatives to the hypothesis we are trying to confirm, whether or not we have explicitly formulated and considered those alternatives individually.

I fully agree with Sherri's exciting and provocative further claim that we are unable to meet this exacting standard in many cases that scientific realists regard as unproblematic. Moreover, I also fully agree with Sherri's claim that the scope of the problem created by this demand simply cross-classifies distinctions that arguments against scientific realism have traditionally treated as important, like the distinction between observables and unobservables. Her elegant example of pregnancy testing illustrates how we <u>can</u> meet the stringent demands of the Likelihood Ratio even when our hypothesis concerns phenomena or states of the world that are unobservable. When we consider the outcome of a blood test for the presence of hormones indicating the earliest stages of pregnancy, we <u>do</u> know the likelihood conferred on our evidence by the negation of the hypothesis we are investigating. We estimate the likelihood of a positive test result when the test subject is not 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. Sherri is right to insist, then, not only that the problem of estimating the likelihood of the evidence on the catch-all can sometimes be convincingly solved in science, but also that this can indeed be done in cases involving claims about unobservable entities, properties, or processes.

But [and here finally is the “but” you have been waiting for] I also think that Sherri substantially overestimates our ability to convincingly meet this standard in actual scientific cases, including the most important ones. She offers an extended analysis of a single historical case designed to show how scientific ingenuity can put us in a position to know the likelihood conferred on our evidence by the negation of an hypothesis even in the case of what she calls “high theory”. The case she appeals to is Jean Perrin’s famous efforts to confirm the atomic hypothesis near the turn of the century through a careful experimental investigation of the phenomenon of Brownian motion: that is, the constant, erratic motion of particles that are suspended in a liquid and 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. What I hope to show is not simply that Sherri mischaracterizes this case, but also that she does so in an especially revealing way—a way that shows why the need for a responsible evaluation of the likelihood of the evidence on the catch-all places much more significant limits on our ability to confirm scientific hypotheses than she imagines.

Sherri's claim is that Perrin was able to confirm the atomic hypothesis even before the famous step in his argument where he calculates Avogadro's number in a wide variety of independent ways. Perrin's careful experimental investigation of Brownian motion alone suffices, she argues, to substantially confirm the "modest hypothesis that there are atoms and molecules, understood merely as spatially discrete sub-microscopic entities moving independently of one another, i.e., at random" (219).

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 hypothesis:

> "If there are atoms…then the motion of the Brownian particles will be a random walk, that is, the 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" (219).

Thus, Perrin's demonstration that the motion of Brownian particles is indeed a true random walk, Sherri argues, was sufficient to establish what she calls the "modest atomic hypothesis" precisely because it enabled us to satisfy the likelihood ratio's demand to know the likelihood of the evidence on the negation of the atomic hypothesis as well as the likelihood of that same evidence on the presumption that the modest hypothesis was true. But I think it is a mistake to regard Perrin's investigation as having established even such a *minimal* atomism by way of the likelihood ratio, and I think seeing why it is a mistake will help illustrate why the need to responsibly evaluate the likelihood conferred on the evidence by the catch-all negation of an hypothesis poses a much more significant obstacle to theoretical confirmation than Sherri allows.

Now Sherri is by no means the first philosopher of science to invest Brownian motion or Perrin's investigations of it with a special evidential or probative significance. Richard Miller, Peter Achinstein, and my colleague Penelope Maddy, among others, have argued that Perrin's work played an exceptional and distinctive role in confirming the atomic theory. It is worth noting, then, how Sherri's approach recalls the "topic-specific truisms" that Richard Miller used to try to defend realism about individual or particular scientific theories piecemeal in the late 1980's. Indeed, Brownian motion was a flagship case for Miller, too: he argued that the appeal of Einstein's atomic explanation of this phenomenon appealed to a "topic-specific truism"—"Non-living matter does not jump about erratically unless something external is moving it about"—which in turn grounded an inference to a minimal version of the atomic hypothesis: "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". That is, Miller suggested that an appreciation of quite local and homely facts specific to the particular domain of nature under investigation left us in this case without any reasonable alternative to embracing a similarly modest form of the atomic hypothesis.

We might profitably consider, then, the response offered by Arthur Fine to Miller's strategy as applied to this particular case. Of most relevance for us is Fine's claim 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 91-92). 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:

> 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 Browinan 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 1905-1915, 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 91-92)

I remind us of these possibilities because they serve equally well, of course, to undermine Sherri'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 Sherri recognizes between the random walk of the Brownian particles and even the 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.

But what is ultimately most important about these sorts of alternative theoretical possibilities, I think, is *not* the unanticipated conceptual space they illustrate separating the randomness of Brownian motion from the existence of atoms, but instead what they reveal about the *kind of inference* Sherri is making use of when she overlooks it. Sherri 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 I would suggest (as I've argued in considerable detail elsewhere) that the history of science reveals this very form of inference to be demonstrably unreliable when applied in the domain of fundamental scientific theorizing.

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 the 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? <u>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</u>, for energy, as Torricelli remarked, 'is a quintessence of so subtile a nature that <u>it cannot be contained in any vessel except the inmost substance of material things'</u>. Hence all these theories lead to the conception of a medium in which 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 its action, and this has been my constant aim in this treatise (1955 [1873] Vol. II 493, my emphasis).

Here Maxwell reports that he finds it impossible to conceive of how the wavelike propagation of light and electromagnetism could occur without a substantival medium in which those waves are propagated. By Sherri's lights, this would entitle him to conclude that the likelihood conferred on his evidence by the negation of a "modest" ethereal hypothesis was very low. But of course, subsequent scientific history has revealed theoretical possibilities that were simply beyond Maxwell's ability to imagine. And I would suggest that what Sherri finds it easy or even possible to imagine today is no better guide to what the world must be like than what Maxwell was able to imagine in 1873.

Of course, Maxwell's appeal to this form of inference is anything but an isolated incident in the history of theoretical scientific inquiry. 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:

> As the thousands of cells which constitute an organism possess very different properties, <u>the chromatin</u> which controls them <u>cannot be uniform; it must be different in each kind of cell.</u>
>
> The chromatin, moreover, cannot <u>become</u> 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 [1892] 32, all emphases in original).

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 possible way in which the development of such cells could be controlled by this hereditary material and yet allow them to come to possess very different characteristics.

Similarly, in his defense of the caloric theory of heat, 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. In his 1785 "Memoir on Phlogiston," Lavoisier writes,

> One can hardly think about these [thermodynamic] 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, <u>the igneous fluid, the matter of heat and fire.</u> (Lavoisier 1785 as translated in Donovan 1993 171, original emphasis, translation modified)

And in the posthumous <u>Traité de Chimie,</u>

> It is difficult to conceive of these phenomena without admitting that they are the result of a real, material substance, of a very subtile 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 . . . . <u>(Traité de Chimie</u>, in Lavoisier (1965 [1743-1794], vol. I 1-3, my translation)

In all of these cases we find prominent scientists inferring that the natural world must have a certain structure or contain certain entities because they simply cannot conceive of any alternative means or mechanism by which the available evidence could have been produced. There are surely epistemic contexts in which this form of reasoning is fully justified, but these examples at least suggest that theoretical natural science is simply not among them: we have no reason to think that we are managing to exhaust the space of fundamental theoretical possibilities where our predecessors failed to do so. Nonetheless, it is this very form of inference upon which Sherri 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, and thus that Perrin's demonstration that the motion is a true random walk suffices to substantially confirm at least the modest atomic hypothesis. In short, Sherri 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 may confidently treat this as equivalent to or sufficient to confirm even the most modest version of the atomic hypothesis simply because she cannot imagine how else the phenomena might be produced. Sherri's reliance on this eliminative form of reasoning in the context of fundamental scientific theorizing places her in extremely distinguished company, but company that the historical record reveals to be engaged in a systematic inferential error nonetheless.

Of course, to see the problem in these terms is to see both how difficult it will be to overcome and what a serious obstacle the likelihood ratio poses for scientific realism. We are able to responsibly evaluate the likelihood conferred on the evidence by both an hypothesis and its negation (pregnant or not, random motion or not) only in very special and well-behaved kinds of circumstances. It turns out that we are characteristically <u>unable</u> to do this in the case of what Sherri calls "high theory", not even in the one case she has explicitly singled out as showing that sometimes we can. Thus, because I think Sherri is absolutely right to insist that absolute confirmation requires responsible estimation of the likelihood conferred on our evidence by the negation of an hypothesis, I also think she should join me in finding the prospects for realism in the case of “high theory” or fundamental scientific theorizing exceedingly dim.

Possible Further Issues and/or Items for Discussion (NOT in my prepared remarks):

“Modest” Atomism:

[Perhaps Sherri would want to insist that such further possibilities do not actually conflict with the atomic hypothesis in the appropriately modest form she has given it. When she introduces the modest atomic hypothesis, for example, she does after all 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 that there are atoms and molecules, and was not intended to be excluded by the more modest atomic hypothesis” (219).

I am not sure quite what to 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 I see no evidence that early 20th 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. In any case, even generously qualifying the "modest" atomic hypothesis in this way still leaves us neglecting the further alternatives that were taken seriously by the scientific community at the time and that equally well predict that the Brownian motion will be a random walk. Sherri's claim that confirming the truly random character of the Brownian motion was tantamount to confirming even the modest atomic hypothesis can be defended only if the modest hypothesis is construed so broadly as to include electrostatic fields and/or uncaused random motion of the Brownian particles as potential referents for the "atoms" it postulates, and if we do this it is hard to see what the modest atomic hypothesis excludes, or, therefore, why it's supposedly hard-won should be treated as any sort of epistemic accomplishment.]

Constructive Empiricism:

[[Now, I should say that I remain unconvinced by Sherri's creative argument for the claim that Bas Van Fraassen's Constructive Empiricism is an untenable, as opposed to simply unmotivated, position. Her fundamental claim is that the attractiveness of Constructive Empiricism depends upon embracing the ratio measure of confirmation, but even if we do so, Constructive Empiricists must still undertake *some* commitments regarding unobservables—in particular she shows them to be committed to a claim we might call (following Sherri) “No Exotics”, which holds in essence that there are no unobservable transient and local influences unaccounted for that are affecting the relationships under investigation. But this is simply a smaller part of an assumption that virtually any scientific investigation requires: that there are no unaccounted-for exotic influences of *any* kind, observable or unobservable, disrupting the experimental investigation in unanticipated ways. The Constructive Empiricist, it seems to me, is well within her rights to simply concede Sherri’s point, and admit that she must assume any particular experimental outcome or investigation of a relationship to be uncontaminated by exotics (including unobservable exotics), but still insist that the spirit of her position is preserved by refusing to grant us any *substantive* knowledge of facts about particular unobservable entities or processes of the sort she was inclined to deny in the first place. Van Fraassen may simply move his half-way house a few inches down the road, I think, without much damage to the foundations, and since our embrace of Constructive Empiricism already depended upon epistemic voluntarism, we are equally free to choose to take up residence in this new dwelling that so closely resembles our old familiar surroundings.]]