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Making Contact with Molecules: On Perrin and Achinstein

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1. INTRODUCTION

In his essay, "Philosophy in France in 1912," André Lalande made the following observation.

> M. Perrin, professor of physics at the Sorbonne, has described in *Les Atomes*, with his usual lucidity and vigor, the recent experiments (in which he has taken so considerable a part) which prove conclusively that the atoms are physical realities and not symbolical conceptions as people have for a long time been fond of calling them. By giving precise and concordant measures for their weights and dimensions, it is proved that bodies actually exist which, though invisible, are analogous at all points to those which we see and touch. An old philosophical question thus receives a positive solution. (Lalande 1913, 366–7)

This brief and matter-of-fact announcement expressed a rather widely shared sentiment on the European continent that Jean Perrin's experimental work had clinched the issue of the reality of atoms. Indeed, it is now obvious that between roughly 1908 and 1912, there was a massive shift in the scientific community in favor of the atomic hypothesis. It is also obvious that Perrin's experimental work on the causes of Brownian motion played a major role in this shift. When Perrin received the Nobel Prize for physics in 1926, it was noted in the presentation speech by Professor C. W. Oseen that he "put a definite end to the long struggle regarding the real existence of molecules."

Peter Achinstein has offered one of the most systematic expositions and reconstructions of Perrin's argument, aiming (a) to show how his own theory of evidence best accounts for the significance of Perrin's results; and (b) how Perrin has offered a local and experimental argument for scientific realism. After some detailed presentation of Perrin's argument, I will offer my own reconstruction of it and will show why it is superior to Achinstein's. Finally, I will try to draw some lessons for scientific realism.[1]

2. ENTER PERRIN

Over time, Perrin seems to have shifted from a neutral position, with friendly gestures to atomism, to a full endorsement of the atomic theory. In his textbook of 1903, he contrasted two methods of doing science: the inductive, which proceeds by analogy and generalization, and the intuitive-deductive, which consists "in imagining *a priori* matter to have a structure that still escapes our imperfect senses, such that its knowledge permits *deductive* predictions about the sensible properties of the universe" (Perrin 1903, viii). The latter method fosters "the illusion of a satisfactory explanation . . . [of] the visible in terms of the invisible, even when [it does not] lead to the discovery of new facts" (viii). Though he notes that in that book he will adopt the inductive method, he nonetheless claims he will *not* aim to condemn "en bloc" the molecular theories, but rather to submit them to a critical scrutiny in such a way that their essential elements are preserved.

Perrin was sensitive to the fact that for many (notably Duhem and perhaps Ostwald and Mach), the atomic hypothesis was a venture into metaphysics. Surprisingly, he added: "I do not forget that the sensation is the only reality." This would seem to condemn the atomic hypothesis from the start. Yet, Perrin added two important caveats. The first is that "[sensation] is the only reality, on the condition that to the actual sensations all *possible* sensations are added." This is important because he thought that the atomic hypothesis could, in the end, be rooted in sensations. How this could be is illustrated by his second caveat, in which he drew an analogy between molecules and microbes—the latter did become the object of "possible sensation" via the microscope. Here is how he put it.

> One would certainly have been able, without the aid of the microscope, to arrive at the thought that contagious diseases were due to the multiplication of very small living beings. One, guided by these ideas a priori, would have been able to discover almost all of the Pasteurian technique. One would have thus followed deductive science and cured the contagious diseases, but following a way condemned by the supporters solely of the inductive method, until the very day in which the microscope had proved that the microbe hypothesis expressed several possible sensations. Here then is an indisputable example of a structure which could escape our senses and the knowledge of which allows anticipation of certain properties which are [to our senses] directly accessible. (Perrin 1903, ix–x)

The point is that a hypothetico-deductive account of scientific method won't provide strong grounds for accepting the reality of the explanatory posits—more is needed, and this more comes, in the end, from experimental confirmation and, in particular, from placing the hypothesized entities into a causal network that ends up in certain observational trails.

By the time he wrote his *Les Atomes*, he had become an ardent defender of the intuitive-deductive method. In the preface, he noted,

> To divine in this way the existence and properties of objects that still lie outside our ken, *to explain the complications of the visible in terms of invisible simplicity* is the function of the intuitive intelligence which, thanks to men such as Dalton and Boltzmann, has given us the doctrine of Atoms. This book aims at giving an exposition of that doctrine. (Perrin 1916, vii)

However, even then, he very much hoped that there would be some day in which atoms would be "as easy to observe as are microbes today," though for him the use of a microscope is within the "realm of experimental reality" (1916, x). The point that needs to be appreciated is that for Perrin science should proceed by refusing to limit itself "to the part of the universe we actually see," and that the best way to achieve this is to aim at explanation-by-postulation, that is by aiming to explain the visible in terms of the invisible (1916, xii).

Perrin's more technical work is collected in his *Brownian Movement and Molecular Reality*, which appeared in French in 1909 and was translated into English in 1910. In this book, Perrin makes almost no methodological remarks, but I shall attempt to reconstruct the structure of his argument for the reality of molecules in a way that his methodology is clearly brought out. The key point of his strategy is this: "Instead of taking this hypothesis [the atomic hypothesis] ready made and seeing how it renders account of the Brownian movement, it appears preferable to me to show that, possibly, it is logically suggested by this phenomenon alone, and this is what I propose to try" (1910, 7).

Perrin takes it that the atomic hypothesis is an already given plausible hypothesis, its plausibility being grounded in the fact that it remains the only serious admissible explanation of Brownian movement. Reviewing the work of Léon Gouy and others, Perrin suggests that several potential causes of the movement can be safely eliminated and that, in particular, the cause of the movement is internal and not external (cf. 1901, 6). This kind of eliminative approach paves the way for rendering the standard atomic explanation of Brownian movement "by the incessant movements of the molecules of the fluid" the only serious admissible explanation. This is not enough to render it true or probable; and yet, by the end of his reasoning, Perrin does think that it is probable *and* true. This happens because Perrin succeeds in showing that Brownian movement *is* itself an instance of molecular movement and hence that it obeys the laws of the molecular movement. Hence, it can be used to (a) determine Avogadro's number and (b) to specify the individuating properties of atoms. To see all this, let us follow his steps in some detail.

Perrin's theoretical schema proceeds as follows. Let us suppose we have a uniform emulsion (all granules are identical) in equilibrium that fills a vertical cylinder of cross section *s*. Consider a horizontal slice contained between the levels <h, h+dh>, where this slice is enclosed between two semi-permeable pistons—they are permeable to the molecules of water but impermeable to the granules. Each piston is subjected to osmotic pressure. This slice of granules does not fall; hence there must be an equilibrium between the force that tends to move it upward (viz., the difference of the osmotic pressures) and the force that tends to move it downward (viz., the total weight of the granules less the buoyancy of the liquid). Having estimated both forces, Perrin arrives at the equation of the distribution of the emulsion

$$2/3W\log(n_0/n) = \varphi(\Delta - \delta)gh \qquad (1)$$

where W is the mean granular energy, φ the volume of each granule, Δ its density, δ the density of the intergranular liquid and n and n_0 respectively the concentrations of the granules at the two levels separated by height h. The task then is to measure all magnitudes other than W; hence, to determine W (cf. 1910, 24).

The important assumption that Perrin makes is that the mean granular energy W of the particles in Brownian motion is equal to mean molecular energy W'. In other words, he argues that the Brownian particles behave as large molecules and hence obey the laws of the gases (see also 1916, 89, 92). Indeed, the first few sections of his 1910 work aim to motivate this claim. The mean kinetic energy W' of the molecules of a gram-molecule of a gas is a function of Avogadro's number N. It is equal to (3R/2N)T, where T is the absolute temperature of the gas and R is the constant of the perfect gases (cf. 1910, 19). Hence,

$$w' = (3R/2N)T \qquad (2)$$

Perrin relies on van't Hoff's proof that the invariability of energy (viz., that the mean kinetic energy is the same for all gases at the same temperature) holds *also* for the molecules of dilute solutions and generalized it further to all *fluids*, including emulsions.

The claim that "the mean energy of translation of a molecule [is] equal to that possessed by the granules of an emulsion"—that is that $W = W'$—is crucial. It paved the way for an *experimentum crucis*: either $W = W'$ or W

$\neq W'$ and given that both W and W' could be calculated, we might have "the right to regard the molecular theory of this movement as established" (1910, 21).

Being an extremely skillful experimenter, Perrin managed to prepare suitable emulsions of gamboge and mastic, with spherical granules of radius α. (1) thus becomes

$$2/3W\log(n0/n) = 4/3\pi\alpha^3(\Delta-\delta\ )gh. \tag{1'}$$

Here again, all magnitudes but W are measurable. Determining the ratio (n_0/n) was quite demanding, but Perrin used the microscope to take instantaneous snapshots of the emulsion. Determining the value α of the radius was even more demanding, but Perrin used three distinct methods to achieve this, one relying on Stokes's equation (capturing the movement of a sphere in a viscous fluid), and two without applying this equation (using, instead, a *camera lucida*). These calculations were in impressive agreement, which led Perrin to conclude, among other things, that the otherwise controversial application of Stokes's equation (because it was meant to apply to continuous fluids) was indeed legitimate.

When all was said and done, Perrin was able to calculate the granular energy W (which is independent of the emulsion chosen). If $W = W'$ (if, that is, the Brownian particles do behave as heavy molecules and hence if the laws of the gases do hold for them too), there is a direct prediction of Avogadro's number N from (1′) and (2), that is,

$$(RT/N)\log(n0/n) = 4/3\pi\alpha^3(\Delta-\delta)gh$$

and

$$N = 3RT\log(n_0/n)/4\pi\alpha^3(\Delta-\delta)gh. \tag{1''}$$

This prediction could then be compared with known calculations of N based on the kinetic theory of gases, for example, that by van der Waals's ($N = 6\times10^{23}$) (cf. 1910, 44). Perrin made a number of experiments and concomitant calculations and the agreement was always impressive. As he put it, "It is manifest that these values agree with that which we have foreseen for the molecular energy. The mean departure does not exceed 15 percent and the number given by the equation of van der Waals does not allow for this degree of accuracy" (Perrin 1910, 46).

Perrin became immediately convinced that "this agreement can leave no doubt as to the origin of Brownian movement" (1910, 46). "[A]t the same time," he said, "*it becomes very difficult to deny the objective reality of molecules*" (1916, 105). What convinced him, he says, was that on any other hypothesis (better, on the negation of the atomic hypothesis), the expected value of *N* from the study of the movement of granules suspended in a liquid would be either infinite or zero—it would be infinite if all granules actually fell to the bottom of the vessel, and zero if the fall of the granules was negligible. Hence, on the hypothesis that matter has not molecular structure, the probability that the predicted value of N would be the specific one observed would be zero; on the contrary, this probability is high given the atomic hypothesis. This, Perrin noted, "cannot be considered as the result of chance."

Perrin takes another step. He stresses that the determination of Avogadro's number by (1″) affords a determination of the properties of molecules that can be calculated on its basis. Moreover, this determination of N is now "*capable of unlimited precision*," since all magnitudes in (1″) other than N can be determined "to whatever degree of precision desired." Hence, Perrin went on to calculate N and to conclude that its value is $N=7\times10^{23}$. From this, he calculated the weight and the dimensions of molecules. He also reported on a number of other calculations of Avogadro's number, including: the measurement of the coefficient of diffusion; the mobility of ions; the blue color of the sky (the diffraction of the sunlight by the atmospheric molecules); the charge of ions; radioactive bodies; and the infrared part of the spectrum of the black-body radiation. Though all these calculations were less accurate than his own, Perrin took them to prove molecular reality (cf. 1910, 90), since they are in considerable agreement, showing that this number is "essentially invariant" (1910, 74).

Here is his conclusion.

> I think it impossible that a mind, free from all preconception, can reflect upon the extreme diversity of the phenomena which thus converge to the same result, without experiencing a very strong impression, and I think it will henceforth be difficult to defend by rational arguments a hostile attitude to molecular hypotheses, which, one after another, carry conviction, and to which at least as much confidence will be accorded as to the principles of energetics (1910, 91).

What then is the logical structure of Perrin's argument? Recall his claim that he was after a crucial experiment for the reality of atoms. Of course, there are no crucial experiments in the strict sense of the expression, viz., in the sense of disproving a hypothesis or of proving a hypothesis. But as Poincaré has put it, an experiment can condemn a hypothesis, even if it does not—strictly speaking—falsify it. Perrin's argument was precisely meant to condemn the denial of the atomic hypothesis—which, of course, is not to say that he intended to condemn energetics. As we have just seen, he did think (and he had already noted this in his 1903 work) that energetics need *not* imply the denial of the atomic hypothesis, namely, that matter is continuous.

The way, then, I think Perrin's argument should be reconstructed is as follows. With the argument sketched above, Perrin has made available two important probabilities, namely,

Prob (n=N/AH) = very high
Prob(n=N/-AH) = very low

That is, the probability that the number of molecules in a gram-molecule of a gas (including an emulsion, which does behave as a gas) is equal to Avogadro's number given the atomic hypothesis is very high, while the probability that the number of molecules is equal to Avogadro's number given the denial of the atomic hypothesis is very low. These two likelihoods can be used to specify the so called Bayes factor *f*.

$$f = \text{prob}(n=N/\text{-}AH)/\text{prob}(n=N/AH)$$

Bayes's theorem states

$$\text{prob}(AH/n=N) = \text{prob}(n=N/AH)\text{prob}(AH)/\text{prob}(n=N)$$

where:

$$\text{prob}(n=N) = \text{prob}(n=N/AH)\text{prob}(AH)+\text{prob}(n=N/-AH)\text{prob}(-AH).$$

Using the Bayes factor, Bayses's theorem becomes:

$$\text{prob}(AH/n=N) = \text{prob}(AH)/(\text{prob}(AH) + f\,\text{prob}(\text{-}AH)).$$

Perrin's argument then can be put thus:

1. *f* is very small.
2. N = n is the case.
3. prob(AH) is not very low.

Therefore, prob(AH/n=N) is high.

Now, premise 1 (that *f* is very small) is established by the body of Perrin's demonstration, which shows that given the denial of the atomic hypothesis, it is extremely unlikely that Avogadro's number has the specific value it does. Premise 2 is established by a series of experiments involving different methods and different domains. Premise 3 is crucial, since it is required for the probabilistic validity of Perrin's argument. It specifies the prior probability of the atomic hypothesis and without the prior probability the argument noted above would commit the base-rate fallacy. Perrin's preparatory eliminative work has aimed to show that, by eliminating several alternative potential explanations of Brownian movement, the atomic hypothesis has gained at least some initial plausibility which is reflected in its having some prior probability of being true.

Actually, the following might be added. There is a rare case in which the prior probability of a hypothesis does not matter, and this is when the Bayes factor is zero. This happens when just one theory can explain the evidence. Then, we can dispense with the priors. If the Bayes factor is zero, no matter what prob(AH) is, the posterior probability prob(AH/n=N) is unity. And the Bayes factor is zero if prob(n=N/-AH) is zero. Recall Perrin's wording: "That, in the immense interval [0, infinity] which *a priori* seems possible for N, the number should fall precisely on a value so near to the value predicted, certainly cannot be considered as the result of chance" (1910, 46; cf. 1916, 105). This is *almost* tantamount to saying that his experiments established that prob(n=N/-AH) = 0.

This kind of claim would (and does) explain Perrin's confidence that the atomic hypothesis has been "established"; that he has offered "a decisive proof" of it (1916, 104). Admittedly, there is room for manoeuver here, since it *might* be argued that prob(n=N/-AH) has, after all, a small finite value. In that case, some reliance on the prior probability prob(AH) is inevitable and the usual philosophical dialogue would kick off: How are the priors fixed? Are they objective? If not, is the case for the reality of atoms strong?

I do not want to follow this dialogue now (except to note that I agree with Achinstein that prior probabilities need not be subjective or idiosyncratic degrees of belief). I want to stress, however, that it seems to *me* that the major role Perrin's work has had in persuading scientists to adopt the atomic hypothesis lies mostly in its presenting a rare but very important case in which the posterior probability of the atomic hypothesis becomes (almost) unity—given, of course, that it is assigned a *non-zero* prior, which it seems everybody but Duhem did.

A chief point that Perrin makes is precisely that size does not matter, but causal role does! Like microbes, molecules do end up being the objects of possible sensation—in the broad sense in which Perrin understands this, namely, to include detection through the microscope. Hence Perrin, like Pasteur before him, places the atoms firmly within the laboratory, grounding their causal role and offering experimental means for their detection and the specification of their properties. This is of great significance because it becomes clear that Perrin's argument should be compelling for anyone who does not take it that strict naked-eye observability is a necessary condition for accepting the reality of an entity. It should also be compelling for anyone who thinks that continuity of causal role is a criterion for accepting the reality of an entity—irrespective of whether some instances of this entity are observable, while others are not.

By the same token, it becomes clear that the real issue about the so-called theoretical entities is not their unobservability, but rather their accessibility. In this sense, what Ostwald aptly called "the scientific horizon" is not fixed and immovable; claims that are once below it can move above it. What facilitates this change is not that some suspicious entities become observable, but rather that some suspicious entities enhance their explanatory role: claims about them are highly confirmed by ordinary scientific methods; their causal role is established experimentally; they become the locus of unification of disparate phenomena. Perrin's case is instructive because it shows vividly that there are points after which resistance to accepting the reality of certain entities becomes dogmatic and mostly motivated by philosophical prejudice (cf. Krips 1986).

3. ENTER ACHINSTEIN

The core of Achinstein's claim is that the calculation of Avogadro's number by Perrin's experiments using (a notational variant of) equation (1″) above confirmed Perrin's core hypothesis, namely that molecules exist and that Avogadro's number is 6×10^{23}. More specifically, Achinstein takes proposition T to express the core hypothesis of the atomic theory:

> T = Chemical substances are composed of molecules, the number N of which in a gram molecular weight of any substance is (approximately) 6×10^{23}.

He takes it that this proposition already has had some support from background knowledge *b* and other evidence. In particular, he rightly claims that T's plausibility (and in fact its non-zero probability) was based on the application of "causal eliminative" reasoning (Achinstein 2001, 255). Actually, the initial probability that Achinstein assigns (or claims that Perrin assigned) to T, given background knowledge, is Prob(T/b)>1/2. He then claims that Perrin's experimental result led him to accept the following proposition:

> C = The calculation of N done by means of Perrin's experiments on Brownian particles using equation [(1″)] is 6×10^{23}, and this number remains constant [when several parameters in equation (1″) are varied].

Achinstein goes on to claim that C is more probable given the truth of T than without T and to make his crucial point that C confirms T. This is because given

(i) prob(C/T&b) > prob(C/b)
(ii) prob(T/b)>0
(iii) prob(C/b)>0

it follows from an application of Bayes's theorem that

(iv) prob(T/C&b)>prob(T/b).

Moreover, since he has assumed that prob(T/b)>1/2, it follows that

(v) prob(T/C&b)>1/2.

Achinstein put this in the service of his own theory of evidence. In broad outline, two statements are the main features of Achinstein's theory. The first is that for something e to be evidence for a hypothesis H, it must be the case that the probability of H given e should be higher than ½. That is, prob(H/e)>1/2. So, Achinstein works with an absolute concept of evidence: e is evidence for H only if e is not evidence for the denial of H. This is meant to capture the view that evidence should provide a *good reason* to believe. But, second, this absolute conception of evidence (though necessary) is not sufficient for reasonable belief. What is added is that the probability that there is an explanatory connection between H and e, given H and the evidence e, should be more than ½. Call E(H/e) the claim that there is an explanatory connection between H and e. Achinstein's second feature is that prob(E(H/e)/H&e)>1/2. More accurately, e is evidence (a good reason) for H only if the *product* of prob(E(H/e)/e&H) with prob(H/e) should be greater than ½.

Given this conception, (v) is far more important than (iv) above. Besides, the foregoing requirement that there is an explanatory connection between the hypothesis and the evidence is satisfied in Perrin's case, and Achinstein argues that

(vi) prob(E(T/C&b)/T&C&b)>1/2.

Of course, there is no guarantee that the *product* of prob(E(T/C&b)/T&C&b) with prob(T/C&b) is great than ½. The values of the two factors should be chosen by hand such that their product is greater than ½. Achinstein argues that they can be plausibly taken to be sufficiently high in Perrin's case. But this is certainly an extra and defeasible assumption. In any case, Achinstein's conclusion is that not only did Perrin provide evidence for the reality of molecules, but also that this is best captured by his own theory of evidence.

It seems to me this is not right. Achinstein's reconstruction leads to a weak conclusion vis-à-vis the case at hand. If Perrin just succeeded in establishing that prob(T/C&b)>1/2, it seems there is no explanation of why his achievement was taken (by himself and almost everybody else) to be decisive in establishing the reality of atoms. On Achinstein's reconstruction, all Perrin achieved was to show that the atomic hypothesis is more likely than not. This is not a mean feat, of course. But it is hardly sufficient to sway the balance in favor of the atomic hypothesis in the way it actually did. There is no natural way to increase the posterior probability of T in Achinstein's account, unless T is given a very high prior probability and the product prob(E(*T*/C&b)/T&C&b) × prob(T/C&b) is fiddled with.

My account, on the contrary, does show that AH (which is roughly equivalent to Achinstein's T) becomes very probable, given Perrin's experimental results. Besides, my account, unlike Achinstein's, captures the strength of the evidence. More specifically, Achinstein notes that his own account of evidence cannot easily explain why some qualities of some piece of evidence (e.g., precision and directness) provide stronger support for a hypothesis than pieces of evidence that lack these qualities (Achinstein 2001, 262). (Achinstein ends up adding these qualities by hand into his theory.) In my account, the precision of the determination of Avogadro's number and the diversity of means by which this precise determination was achieved makes it all the more improbable that this will be the right number (that is, that n will be equal to N) given the negation of AH.

Defending his own theory of evidence against other attempts to reconstruct and explain Perrin's achievements, Achinstein (2001, 259) notes that his own account (i) is better than Salmon's (which was based on the common cause principle) because on his own account the molecular hypothesis does get confirmed by the evidence; and (ii) is better than an ordinary hypothetico-deductive reconstruction, since it does not suppose a deductive link between the molecular hypothesis and Perrin's results. It's patently the case that my own account fares at least as well as Achinstein's vis-à-vis the other two stories.

Achinstein is very sensitive to the charge that Perrin's reasoning might be circular, since Perrin seems to assume the reality of the molecules before he tries to prove it (Achinstein 2001, 259). His answer to this charge is that Perrin does not start with an unquestioned assumption that molecules exist, but that he does take this assumption to have some initial probability, based on the causal-eliminative reasoning that preceded his own strategy. I think this is right and is actually brought out by my own account too.

Hence, my account offered in the previous section has all the strengths and none of the weaknesses of Achinstein's.

4. LESSONS FOR SCIENTIFIC REALISM

Perrin's case highlights a claim that lately I tend to find all the more forceful, namely, that commitment to the reality of specific explanatory posits is a matter that depends on the context. This is so because, as I have argued in *Knowing the Structure of Nature* (Psillos 2009), there are two types of evidence that are brought to bear on the truth of scientific hypotheses (and which inform judgments of prior probability and of confirmation). The first type is *first-order evidence* and is related to whatever evidence scientists have in favor of a hypothesis. In Perrin's case, this evidence includes the several methods of determination of Avogadro's number, the evidence that goes into fixing a non-zero prior probability to the atomic hypothesis (e.g., the evidence that the cause of Brownian movement is internal to the fluid), and so on. The second type of evidence, what I call *second-order evidence*, comes from the track record of scientific theories and/or meta-theoretical (philosophical) considerations that have to do with the reliability of scientific methodology. This, for instance, is evidence that many past explanatory hypotheses have been abandoned, that there have been alternative potential explanations of some phenomena that came to be accepted later on, and so on. This kind of (historical-philosophical) evidence does not concern particular scientific theories but science as a whole. It is the kind of evidence that, for instance, motivates the pessimistic induction. I have argued that the proper philosophical task is to balance these two kinds of evidence and that this balancing is context-dependent (Psillos 2009).

Perrin's case is very instructive precisely because it shows that the context can settle issues of balance. For instance, it is clear that Perrin's case is so strong that the first-order evidence for the reality of molecules takes precedent over the second-order evidence there might be for being skeptical about explanatory posits. The fact that other explanatory hypotheses have failed in the past is trumped—in this context—by the strength of the first-order evidence. It would be folly, however, to think that considerations concerning the second-order evidence should be totally wiped out—or worse, that these are considerations to which working scientists are blind. These are meta-theoretical or philosophical considerations that do get into the evidential balance sheet nonetheless. Achinstein seems to imply that these considerations are almost irrelevant to the issue of the reality of explanatory posits (Achinstein 2002, 495). They are not.

Achinstein (2002) is right in stressing that the proper battleground for scientific realism is made of specific arguments in favor of the reality of specific unobservable entities. Given the key role that explanatory considerations play in specifying the prior probabilities of certain hypotheses (e.g., the atomic hypothesis), it is an exaggeration to call the proper argument for realism "experimental." Better put, the proper argument for realism is explanatory-experimental, the latter component meaning to stress that causal contact with the explanatory posits enhances the claim to their reality. But Achinstein (2002) seems to want to draw the further conclusion that the realism debate as it has been conducted so far is independent of the kind of argument for realism you get from Perrin. This is wrong.

I will confine myself to two points. First, Perrin already works within what I have elsewhere (Psillos forthcoming [a]) called "the realist framework." Simply put, Perrin already works *within* the framework that seeks explanation of the manifest behavior of bodies while positing typically unobservable entities; hence he adopts a framework that allows for the assignment of prior probabilities to "invisible" entities. This is not something that evidence or a priori reasoning forces on anyone. To see this, just think of die-hard opponents of realism (Duhem? van Fraassen?) who refuse to adopt this framework; and hence, who refuse to assign non-zero (Duhem) or anything-but-vague (van Fraassen) prior probabilities to specific explanatory posits—such as the molecules. To put the point somewhat crudely, Perrin's argument does not amount to an argument for scientific realism in *general* (as opposed to an argument for the reality of certain entities) because it is launched *within* the realist framework. Hence, the debate about the realist framework itself is alive and well. My second point concerns the relation between Perrin's argument and the so-called "no miracles" argument (NMA) for realism. Achinstein intends to distance Perrin's argument from NMA (Achinstein 2002, 486). But he does not have to do so. The relation between Perrin's argument and NMA is precisely the one anticipated by realists like Boyd and myself (Psillos 1999), namely, Perrin's argument is one of the very many first-order instances of inference to the best explanation (IBE), which feed the premises of the realist argument that IBE is reliable. And this is precisely what the NMA aims to do, namely, to defend the reliability of IBE. I have defended all this in my forthcoming paper (Psillos forthcoming [b]). What Perrin's case has taught me, among other things, is that the first-order IBE-type of reasoning that leads to commitment to the reality of certain explanatory posits has a fine structure and a strength that is shaped, by and large, by the context.

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NOTES

1. I dedicate this essay to Peter, who has taught me (and us, I hope) how important it is to combine philosophical rigor with historical sensitivity.