Chapter 7
The View *from Within* and the View *from Above*: Looking at van Fraassen's Perrin

Stathis Psillos

Abstract Bas van Fraassen has usefully contrasted two ways to view the relation between theory and measurement: from above and from within. Roughly put, "from above" is the perspective in which we view measurements from the point of view of the finished theory aiming to examine how the measurement is related to the theory. "From within" is the perspective in which we see measurement as a means for the development of the theory. van Fraassen warns us that we need a "synoptic vision," one that combines both perspectives. In this chapter, I argue that this synoptic view can be had *without* forfeiting important conclusions about how theory and experience and observation are related to reality. I make my case by looking in detail into an important episode in which the two views should clearly be in play: Perrin's work on the Brownian motion. This case has been recently studied by van Fraassen too. There are significant elements of disagreement in the ways we look at this case. I argue that Perrin's case shows that it is unreasonable to defend the superiority of the molecular theory *c. 1912* without defending its likely truth. There is an important point of contact with van Fraassen: we both take measurement to be a vehicle of representation. But we disagree on the role of instruments as means for representation. After having presented my own way to bring together the view from within and the view from above in Perrin's case, I take issue with his account of instrument-driven measurement as a case of public hallucinations.

An earlier version of this chapter was presented at the *XVI Jornadas de Filosofía y Metodología actual de la Ciencia*, at the University of A Coruna, Spain in March 2011. Many thanks to Bas van Fraassen and to Valeriano Iranzo for comments and to Wenceslao J. Gonzalez for the kind invitation. Research for this paper has been co-financed by the European Union (European Social Fund – ESF) and Greek national funds through the Operational Program "Education and Lifelong Learning" of the National Strategic Reference Framework (NSRF) – Research Funding Program: THALIS – UOA – APRePoSMA.

S. Psillos (✉)
Department of Philosophy and History of Science, University of Athens,
University Campus, w/n, 15771 Athens, Greece
e-mail: psillos@phs.uoa.gr

W.J. Gonzalez (ed.), *Bas van Fraassen's Approach to Representation and Models in Science*, Synthese Library 368, DOI 10.1007/978-94-007-7838-2_7,

Keywords van Fraassen • Perrin • Brownian motion • Representation • Atomism • Confirmation

7.1 Introduction

Bas van Fraassen has usefully contrasted two ways to view the relation between theory and measurement: from above and from within. Roughly put, "from above" is the perspective in which we view measurements from the point of view of the finished theory aiming to examine how the measurement is related to the theory. "From within" is the perspective in which we see measurement as a means for the development of the theory. The first perspective is ahistorical while the second is historical. van Fraassen (2008, 139) warns us that we need a "synoptic vision," one that combines both perspectives. He claims that this synoptic point of view frees us from the illusory search for "a view from nowhere". It tells us how theory and measurement are related without presupposing "an impossible God-like view in which nature and theory and measurement practice are all accessed independently of each other and compared to see how they are related 'in reality'" (2008, 139).

This "synoptic" vision brings to mind — as van Fraassen is clearly aware of — Sellars's project to relate the manifest image with the scientific image of the world. Sellars urged us to bring the two images together in a "stereoscopic" view, which is achieved when "two differing perspectives on a landscape are fused into one coherent experience." But, the reader may recall, this Sellarsian stereoscopic view was not symmetric; nor did it put the two images on equal footing, as it were. Accommodation of the manifest image there will be, but the scientific image retains its primacy: "in the dimension of describing and explaining the world, science is the measure of all things, of what is that it is, and of what is not that it is not."

The view from above and the view from within should certainly be brought together in a stereoscopic view. What van Fraassen has done in his (2008) to bring them together is really illuminating. But, I will argue, this stereoscopic view can be had *without* forfeiting important conclusions about how theory and experience and observation are related to reality.

I will make my case by looking in detail into an important episode in which the two views should clearly be in play: Perrin's work on the Brownian motion. This has been recently looked at by van Fraassen himself (cf. 2009). There are significant elements of disagreement in the ways we look at this case. I will try to bring them out as clearly as I can. I will argue that Perrin's case shows that it is unreasonable to defend the superiority of the molecular theory *c. 1912* without defending its likely truth.

There is an important point of contact with van Fraassen: we both take measurement to be a vehicle of representation (cf. 2008, 91). But we disagree on the role of instruments as means for representation. After having presented my own way to bring together the view from within and the view from above in Perrin's case, and after having contrasted it with van Fraassen's, I will take issue with his account of instrument-driven measurement as a case of public hallucinations.

7.2 Brownian Motion c. 1905

It is widely accepted that between roughly 1908 and 1912, there was a massive shift in the scientific community on the European continent in favour of the atomic conception of matter. It is also widely accepted that Perrin's theoretical and experimental work on the causes of Brownian motion played a major role in this shift. Brownian movement — so-called because it was first identified as such by the botanist Robert Brown — is the incessant and irregular agitation of small particles suspended in a liquid. 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."

We can see this shift of opinion in its clearest form in Henri Poincaré's writings from 1900 to 1912. In his address to the 1900 International Congress of Physics in Paris, Poincaré claimed that the atomic hypothesis, viz. the hypothesis that matter has a discontinuous structure, is "indifferent," that is, useful as a device of computation or for providing "concrete images" which help scientists fix their ideas (1900, 10). From this, Poincaré noted, there is no reason to conclude "the real existence of atoms." In 1912, in a lecture delivered at the French Society of Physics, Poincaré famously spoke of the experimental proof of the reality of atoms: "the atoms are no longer a convenient fiction; it seems, so to speak, that we can see them since we know how to count them" (1913, 89). What made the difference for Poincaré was Perrin's experiments on the Brownian motion: the shift of the atomic hypothesis from the status of an indifferent hypothesis to the status of a true description of reality was centred around what Poincaré took it to be its experimental verification by Perrin.

Poincaré's change of stance was by no means untypical. Perhaps the most important shift of opinion occurred in Wilhelm Ostwald, who in his (1896) had vehemently attacked "scientific materialism" (basically the "mechanics of atoms"). Already in 1908, in the preface of the third edition of his *Outlines of General Chemistry* he (1912, vi) noted:

> … [T]he agreement of the Brownian movements with the requirements of the kinetic hypothesis, established by many investigators and most conclusively by J. Perrin, justify the most cautious scientist in now speaking of the experimental proof of the atomic nature of matter. The atomic hypothesis is thus raised to the position of a scientifically well-founded theory, and can claim its place in a textbook intended as an introduction to the present state of out knowledge of General Chemistry.

He then went on to present the atomic hypothesis in relation to the Brownian movement and spoke of "the final proof of the grained or atomistic-molecular nature of matter (…) after a fruitless search during a whole century" (1912, 483–484).

The atomic conception of matter went through many twists and turns. It did face significant scientific anomalies (e.g., the specific heats anomaly) as well as important theoretical successes (e.g., the extension of the kinetic theories of gases to liquids). But it did also face important philosophically-motivated objections. Heinz Post (1968) drew a useful distinction between two types of atomic theory: the "essentially atomic theories," which do allow the determination of Avogadro's number *N*, and those that do not. The atomic hypothesis entailed that atoms should be countable; hence, the determination of Avogadro's number *N* (the number of atoms in a gram molecule of a gas) was a key plank in the defence of atomic theory. For, among other things, the determination of this number would allow the determination of other atomic properties, e.g., the size of molecules. There had been many attempts to determine Avogadro's number (cf. Brush 1968), notably by William Thomson (1870), who used four ways to estimate it and who actually declared that the atomic hypothesis thereby received "a high degree of probability." But all this did not really sway the balance in favour of the atomic theory. The opposition's case was still very strong.

Though Brownian movement had received some attention by scientists (cf. Brush 1968), the first to embark on a systematic study of it from the point of view of the atomic conception of matter was the French physicist Léon Gouy. The motion of the suspended granules was "incessant" without being subjected to a visible external cause. In fact, Gouy (1895) showed that a number of initially suggested external causes of the Brownian movement (such as the illumination of the particles by the microscope) could be safely eliminated; and so could a number of initially plausible internal causes (such as convection currents). For Gouy, the kinetic theory of gases (committed as it was to the thesis that the structure of matter is granular) offered a cogent explanation of the phenomenon — the random motion of the Brownian particles was due to the movement of the molecules of the liquid in which they were contained. The extremely active internal agitation of the liquid — which defied its appearance as an immobile body — was the cause of the Brownian motion. This invisible but permanent agitation of the molecules of the liquid explained the incessant and indefinite movements of the Brownian particles. The latter offered us a "feeble image" of the molecular motion (1895, 7). Though Gouy did try to make a case for the claim that the atomic conception of matter — in light of the Brownian motion — did deserve serious attention, he also admitted that there was not yet a way available to develop this explanation in a rigorous and measurable manner.

It might be ironic that Gouy's piece titled "The Brownian movement and the molecular motions" appeared in the same volume of *Revue Générale des Sciences Pures et Appliquées* as Ostwald's "The Failures of Scientific Materialism." Ostwald's attack on atomism was predicated on the fact that the atomic hypothesis was not definite and precise enough. And though it was increasingly accepted that potential explanations of Brownian movement other than that based on the kinetic theory of gases were "untenable," as Poincaré (1906) put it in his address at the St Lewis International Congress of Arts and Science, until the middle of the first decade of the twentieth century there was nothing like a proper theory which unveiled the atomic basis (the quantitative mechanism and the laws) of Brownian movement.

Two important things happened in the first decade of the twentieth century, which as Nye (1976, 266) put it, led to "a completely renovated atomic hypothesis." One was Einstein's (1905) theory of Brownian motion, which provided for an explanatory mechanism of it based on the molecular kinetic theory; the other was Perrin's theoretical and experimental work, which allowed a very accurate determination of Avogadro's number. In Perrin's hand, Avogadro's number became an invariant and indispensable feature in explanations of various phenomena. More importantly, it paved the way for the exact determination and measurement of atomic magnitudes.

I will substantially elaborate on Perrin's achievements looked at both from within and from above. But before this, let me outline the way van Fraassen reads Perrin's achievements.

7.3 van Fraassen's Perrin

The story van Fraassen (2009) narrates aims to render Perrin's achievements accountable from a constructive empiricist point of view, and hence to block the claim that there is a *privileged* realist reading of Perrin's work as being the demonstration of the reality of unobservable molecules. He (2009, 5) takes it that the standard interpretation outlined above constitutes the LORE:

> LORE: until the early 20th century there was insufficient evidence to establish the reality of atoms and molecules, but then Perrin's experimental results on Brownian motion convinced the scientific community to believe that they are real.

He takes it that the LORE is an interpretation of Perrin's achievements and aims to offer an alternative *interpretation*.

When Perrin enters van Fraassen's narrative of the adventures of the atomic conception of matter during the nineteenth century, the stage has already been set in a certain way: the atomic conception of matter was in need of *empirical grounding*. This need was perceived equally forcefully by the friends and foes of the atomic conception and was meant to pose a challenge to the developing theory, viz., to provide (empirical and measurable) links between the theoretical parameters and various empirical phenomena.

van Fraassen renders this idea of empirical grounding more precise by introducing some elements of Hermann Weyl's (1927 [1963]) account of measurement. He takes from Weyl two conditions. Here is how he (2009, 11) puts them:

> *Determinability*: any theoretically significant parameter must be such that there are conditions under which its value can be determined on the basis of measurement.
> *Concordance*, which has two aspects:
>
> – *Theory-Relativity*: this determination can, may, and generally must be made on the basis of the theoretically posited connections.
> – *Uniqueness*: the quantities must be 'uniquely coordinated', there needs to be concordance in the values thus determined by different means.

Roughly put, the two conditions require that for a theory to be empirically grounded, its basic theoretical magnitudes must be amenable to measurement and that the various measurements of the values of these theoretical magnitudes must yield roughly the same result. When Weyl introduced these conditions, he intended them to be conditions of "the correct theory of the course of the world" (1927 [1963], 121). He insisted that the theory becomes empirically testable when theoretical magnitudes are linked "by theoretically posited connections" to empirical data in such a way that the values of these theoretical magnitudes are determined. If there are multiple determinations of these values, they should be concordant with each other, for otherwise the theory would be inconsistent. He added, however, that the demand of concordance "brings the theory in contact with experience."

van Fraassen takes Weyl's conditions to capture the empirical grounding of the theory. In fact, they do a lot more. As Weyl (1927 [1963], 141–142) explains a few pages later, the theory of measurement should be able to address the following question: how is it possible "to determine quantities much more accurately than the differentiating capacity of our senses permits"? What he clearly has in mind is that the theory of measurement should be able to address this question also in the case in which *theoretical* quantities are involved. The theory establishes connections (functional relations) between theoretical magnitude x and various others. Measuring these other magnitudes, the value of x can be determined "more exactly than by direct observation." Insofar as the results of the various measurements are accurate *and* concordant, "the basic theories are confirmed."

It might be thought that Weyl does not take this reference to confirmation seriously. But this would be too quick. A theory whose theoretical parameters can be subjected to the requirements of determinability and concordance is, as Weyl puts it, a well-founded theory. This is not simply an empirically grounded theory; it is also a confirmable theory. A bit further on his text, Weyl (1927 [1963], 185) discussed explicitly the case of atomic theory and — without referring to Perrin specifically but with an explicit mention of the relevance of the Brownian movement — talks about the "golden era of atomic research" and stresses:

> During the last half century it has provided a thorough and brilliant corroboration for the basic tenets of atomism and penetrated into ever deeper layers of the strange atomic world. To begin with, all its methods led with increasing accuracy to the same values of the mass and charge of an electron. Only through this concordance has atomistics become a well-founded physical theory. Gradually indirect methods have been replaced by more and more direct ones. Thus the Brownian motion of small suspended particles demonstrates directly to our senses the presence of a molecular thermic motion. Through ingeniously arranged experiments one has succeeded in isolating the effects of individual atomic events.

There should be little doubt as to how this passage should be interpreted. It might not even be an accident that Weyl uses the very same expression that we have seen Ostwald using, when he came to accept the atomic theory: the theory has been "raised to the position of a scientifically *well-founded theory*" (emphasis added).

It might also be thought that the very fact that the determination of the values of the basic theoretical magnitudes is done *relative* to the theory is problematic. But this need not be so, as van Fraassen himself notes. For one, the relativity of the test to the theory shows, as Weyl in effect noted, the indispensability of theories in making certain measurements available. It is only on the basis of theories and of theoretically posited connections between theoretical magnitudes and empirical magnitudes that "a difference" which is "not manifest to the senses" is established (cf. Weyl 1927 [1963], 142). As we shall see later on, this is achieved masterfully by Perrin when he predicted Avogadro's number based on the assumptions that Brownian particles are, simply, large molecules. The key point here, however, is that the relativity of the test to the theory suggests that the theory is ahead of the measurement in making certain measurements available. For another, it is by no means certain that the test will comply with the theory. Hence, its relativity to the theory does not imply that the test is trivialised. This is already achieved in Clark Glymour (1975) well-known bootstrapping theory of confirmation — which van Fraassen (2009, 12) favourably discusses though he prefers to leave behind confirmation and to keep just the bootstrapping — viz., *the relativity to theory*. In any case, as we shall see later on, Perrin's theoretical model of Avogadro's number left it entirely open whether the actual measurements of the properties of the Brownian particles would confirm the kinetic theory of gases.

Weyl's conditions are very natural constraints on a well-founded theory and they had been introduced already by Ostwald — at least in their essentials.[1] The difficulty with van Fraassen's appropriation of them is not with the conditions themselves. Nor is it with the fact that Perrin did not try to ground empirically the atomic conception of matter — he certainly did. The difficulty is with van Fraassen's contention that the requirement of an empirical grounding of a theory is *an end in itself*. Rather, the empirical grounding — the determination and measurement of the basic theoretical parameters of the theory — is a means for the theory to change cognitive status: from being a mere hypothesis to being (reasonably accepted to be) true. Provided, of course, that one does allow — in principle — this change of status. (I tell this story in detail in my 2011.)

van Fraassen (2009, 19) says:

> but it was [Perrin's] achievement to tie the research that was needed, to complete these efforts at the empirical grounding of the theory, to the study of Brownian motion.

What exactly does that mean? The atomic theory was in the process of theoretical development, aiming to apply it to new domains and phenomena. This development required the addition of further hypotheses, which introduced new theoretical magnitudes related to the properties that molecules would have if they existed. There was then need for the specification of these magnitudes and of finding "stricter and stricter connections" between them and measurable quantities. Given this theoretical development which aims to incorporate new phenomena into the theory, "empirical measurements take on a special significance: their outcomes place

[1] See my (2011). Ostwald (1907, 408) introduced an important criterion concerning the cognitive status of hypotheses — one that was destined to show why the atomic hypothesis (as developed and tested by Perrin) could change cognitive status. Ostwald noted that *definiteness* and *measurability* were conditions such that, once met, they could change the cognitive status of a hypothesis.

constraints on what the values of the molecular parameters can be." Insofar as the outcomes of these measurements are strict and uniquely determined, the theory is empirically grounded. What Perrin achieved then, according to van Fraassen, is the strict and unique specification of a certain theoretical parameter that was required for the empirical grounding of the molecular model, viz., Avogadro's number.

In his (2008) van Fraassen ties this point about empirical grounding to his view about empirical adequacy. To be sure, he speaks of Millikan and not of Perrin, but the point is virtually the same, viz., that measurements are required for the specification of certain theoretical parameters, for measurements only can show what the value of the theoretical parameter must be if the theory is not to end up being empirically *inadequate*. Extending what van Fraassen says of Millikan to Perrin, Perrin's achievement was that he "filled a blank" in the atomic conception of matter, viz., Avogadro's number. Seen from within, Perrin's experiments wrote a number into a theoretical blank:

> What I mean is: in this case the experiment shows that unless a certain number (or a number not outside a certain interval) is written in the blank, the theory will become empirically inadequate. For the experiment has shown by actual example that no other number will do; that is the sense in which it has filled in the blank. So regarded, *experimentation is the continuation of theory construction by other means* (2008, 112).

There are a couple of objections to van Fraassen's narrative, which will be substantiated after we had gone through Perrin's work. But here they are in outline. The first is that Avogadro's number N had been calculated in various ways before Perrin's own theoretical account of the Brownian motion and the experimental specification of N. So, the role of Perrin's work (both theoretical and experimental) was *not* to fill a blank in a theory — this was already filled, as it were. The role of Perrin's work was to show that a certain way to calculate N (based on a certain theoretical prediction of it) could provide a *decisive test* in favour of the atomic conception. Indeed, history does not seem to be on van Fraassen's side. Perrin's work on the Brownian motion and the atomic hypothesis was deemed so important that he was invited to address the French Philosophical Society on the 27th of January 1910. Though there is no doubt that Perrin wanted to render this number determinate and precise (as he put it, "we consider this determination as given or as highly probable, if we get similar numbers by radically different methods"), he was adamant that this determination was meant to undermine a reason offered by various scientists for taking a fictionalist stance towards atoms, that is for arguing that it is "as if atoms exist" (1910b, 268). For Perrin, his own work was not just meant to "fill in" a theoretical parameter; "it's the true existence of atoms that we claim to establish." Secondly, Perrin's experiments did not aim to prove that the atomic theory would be empirically inadequate unless N has had a certain value. This was well-known too. Rather, they aimed to show that the theory-led determination and concordance (to use van Fraassen's terminology) of certain theoretical parameters can become so precise that resistance to accepting these parameters as real was no longer rational. He closed his aforementioned address by stressing that it will be difficult to defend "by reasonable arguments a hostile attitude to the molecular hypothesis" (1910b, 281).

In the sequel, we shall look in detail into Perrin's strategy for proving the reality of molecules, aiming to show that van Fraassen's interpretation of it is unwarranted.

7.4 Perrin Revisited: The View from Within

Perrin's first paper on the atomic conception of matter was published in 1901 in *Revue Scientifique* under the title: "The Molecular Hypotheses." In this, he took it that, by and large, the debate about the molecular hypothesis — that is the debate about whether matter is continuous or discontinuous, has had a "uniquely philosophical character" and as such the choice between the two approaches was a matter of "taste." The issue could not yet be dealt with experimentally. Perrin did favour the atomic conception, but he was adamant that even though numerous of its consequences had been experimentally confirmed and even though these did not follow from the alternative hypothesis (of continuity), still "we will not perhaps have the right to say that the molecular hypothesis is true, but we will know at least that it is useful."[2] The atomic hypothesis remained "one of the more powerful tools of research" invented by human reason. Perrin presented the rudiments and the successes of the kinetic theory of gases and stressed that the law of the corresponding states that was established by van der Waals was a "triumph" of the theory. But he did also claim that for the acceptance of the atomic conception as something more than useful, the determination of the number of molecules and of their diameter was required.

Ten years later, Perrin published another paper (1911) in the same journal as his 1901 paper, this time with the title: "The Reality of Molecules." The conclusion of this article was that "the objective reality of molecules" had been demonstrated. What had happened in between?

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 the key point of his strategy is summed up thus: "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" (1910a, 7).

Perrin takes it that the atomic hypothesis is a plausible hypothesis, its plausibility being grounded in the fact that, in the end of the day, it is the only serious admissible explanation of Brownian movement. Reviewing the work of Gouy and others, Perrin concurs that several potential causes of the movement can be

[2] Perrin did stress, already in 1901, that the molecules of gases are composed of atoms and that the atoms have internal structure.

safely eliminated and that, in particular, it is plausible that the cause of the movement is internal and not external (cf. 1910a, 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 plausible explanation. This is not enough to render it true or probable. In his already noted address to the French Philosophical Society, Perrin (1910b, 273) was clear that having a hypothesis about the *sense*, as he put it, of the Brownian motion was not enough; what was also required was a phenomenon which could allow him to measure directly Avogadro's number. His ingenious strategy was to show 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.

His theoretical construction proceeds as follows. Let us suppose we have a uniform emulsion (all granules are identical) in equilibrium, which 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 permeable to the molecules of water but impermeable to the granules. Each piston is subjected to osmotic pressure by the impact of the granules it stops. This slice of granules does not fall; hence there must be an equilibrium between the force that tends to move it upwards (viz., the difference of the osmotic pressures) and the force that tends to move it downwards (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

$$2/3W\log\left(n_0/n\right)=\varphi\left(\Delta-\delta\right)gh \tag{7.1}$$

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. 1910a, 24).[3]

The equation of distribution describes an exponential law. It shows that the concentration of the granules decreases in an exponential way as a function of the height: the concentration is denser towards the bottom of the cylinder and rarer towards its top. This is exactly what happens with the distribution of the density of air: the barometric pressure decreases exponentially as a function of the height — a fact that was known as Laplace's law. It is then this fact that allows Perrin to justify an important claim he makes, viz., that the mean granular energy W of the particles in Brownian motion is equal to mean molecular energy W'. In other words, he

[3] As Perrin (1910a, 24, note) stresses, the equation of distribution of emulsion was arrived at independently — and by different means — by Einstein (1905) and Smoluchowski. What Perrin observed, and they did not, was that Eq. (7.1) could furnish a crucial experiment for the molecular theory of Brownian movement.

argued that the Brownian particles behave as large molecules and hence obey the laws of the gases (see also 1916, 89 and 92, 1910b, 275–276).[4]

It was known that 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. 1910a, 19). Hence,

$$W' = \left(3\mathrm{R}/2\,\mathrm{N}\right)\mathrm{T}. \tag{7.2}$$

Perrin relied 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. But he took a step further. By a "rational leap," as he put it (1910b, 272) he generalised van't Hoff's law to *all* fluids, including emulsions. This means that Eq. (7.2) will hold for single molecules, as well as for bigger particles including specks of dust formed by many big molecules. To be sure, Perrin's generalisation of van't Hoff's law follows from the theorem of the equipartition of energy. But Perrin did not take this path because of the complexity of the proof of this theorem (cf. 1910a, 21). In any case, given this generalisation, he (1910a, 20) could note that

> not only (…) each particle owes its movement to the impacts of the molecules of the liquid, but further (…) the energy maintained by the impacts is on average equal to that of any one of these molecules.

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 calculating the granular energy in terms of molecular magnitudes. Accordingly, Perrin thought that the road was open 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" (1910a, 21). It is in this precise sense that Perrin's testing of the molecular origin of the Brownian movement was far from trivial, despite the relativity of the tests to the kinetic theory of gases. For, exactly as van Fraassen would require, the tests could *fail* the theory.

Being an extremely skilful experimenter, Perrin managed to prepare suitable emulsions of gamboge and mastic, with spherical granules of radius α. (7.1) thus becomes

$$2/3W\log\left(n_0/n\right) = 4/3\pi\alpha^3\left(\Delta - \delta\right)gh. \tag{7.1'}$$

---

[4] In *Les Atomes*, Perrin simplifies matters by presenting right away the exponential law applying directly the gas laws to emulsions (cf. 1916, 90–93). This way to proceed might be more appetising, but it might well obscure the justification of the application of the exponential law to emulsions. As he (1916, 93–94) noted with emphasis, the strategy he followed was "to use the weight of the [Brownian] particle, which is measurable, as an intermediary or connecting link between masses on our usual scale of magnitude and the masses of molecules."

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.

Perrin was then 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 (7.1′) and (7.2):

$$(\mathrm{RT/N})\log\left(n_0 / n\right) = 4/3\pi\alpha^3\left(\Delta - \delta\right)gh$$

and

$$N = 3RT\log\left(n_0 / n\right)/4\pi\alpha^3\left(\Delta - \delta\right)gh. \qquad (7.1'')$$

This prediction could then be compared with already known calculations of *N* based on the kinetic theory of gases, e.g., that by van der Waals's ($N = 6.10^{23}$). Perrin made a number of experiments and concomitant calculations and the agreement was always impressive. As he (1910a, 46) put it: "[I]t is manifest that these values agree with that which we have foreseen for the molecular energy. The mean departure does not exceed 15 % and the number given by the equation of van der Waals does not allow for this degree of accuracy."

Perrin became immediately convinced that "this agreement can leave no doubt as to the origin of Brownian movement." "[A]t the same time," he said, "*it becomes very difficult to deny the objective reality of molecules*."

Let's be clear on how exactly Perrin argued. Here (1910b, 277) is how he put the matter in his address to the French Philosophical Society:

> To understand how remarkable [this agreement] is, we must consider that before the experiment, they would have certainly not dared certify that the fall of the concentration would not be negligible (…) and that, against it, they would no more have dared to assert that all grains do not gather in the immediate vicinity of the bottom of the tank. The first possibility led to a null value of *N*, and the second to an infinite value of *N*. That, with each emulsion, one is landed, in the immense interval which seemed therefore a priori possible for *N*, precisely on a value adjacent to the expected number, undoubtedly will not appear the result of chance.

Perrin repeated the same point in his more technical work (cf. 1910a, 46, 1916, 105). What does he say? On the negation of atomic hypothesis there are two options available regarding an emulsion suspended in a *continuous* fluid: either all granules stay at the same level or they fall to the bottom of the tank, depending on the viscosities of the fluid and the emulsion. This would lead to calculations of *N* being either

0 or infinite. The exponential distribution of the Brownian particles is enough to discredit the hypothesis that matter is continuous. But it also discredits any other hypothesis which would give N any other value significantly different from the predicted one. Hence, on the hypothesis that the value of N could be anywhere between zero and infinity, the probability that the predicted value of N is the specific one measured would be zero; on the contrary, this probability is very high given the kinetic theory of gases as developed by Perrin and applied to the Brownian movement. It is precisely this concordance, Perrin noted, that "cannot be considered as the result of chance." Note well that in this setting Perrin takes the negation of the atomic theory to be any theory which predicted *any* other value (order of magnitude) of N (infinite or finite, and in the latter case either zero or any value significantly different from that predicted by the theory). As Perrin put it: "if one was not guided by the molecular theory, once could expect any set of values between and including zero and infinity" (1910b, 281).

Before we try to view Perrin's achievements *from above*, let us note that he does take another step. He stresses that the determination of Avogadro's number by (7.1″) 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 (7.1″) 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 $N = 7 \times 10^{23}$. From this, he calculated the weight and the dimensions of molecules. He also reported on a number of other calculations of Avogadro's number N, including from: the measurement of the coefficient of diffusion; the mobility of ions; the blue colour of the sky (the diffraction of the sunlight by the atmospheric molecules); the charge of ions; radioactive bodies; the infra-red 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. 1910a, 90), since they are in considerable agreement, showing that this number is "essentially invariant" (1910a, 74).[5]

Here then 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 (1910a, 91; cf. also b, 281).

In light of this, I very much doubt that Perrin's attitude towards the molecular theory was the one suggested by van Fraassen. There is nothing objectionable *per se* in van Fraassen's (2008, 112) claim that "*experimentation is the continuation of*

[5] A distinct part of Perrin's work on the molecular explanation of Brownian motion was related to his attempt to verify experimentally Einstein's (1905) theory of diffusion. The relation of Perrin's work to Einstein's is discussed in my (2011). Perrin offers a very detail discussion of Einstein's theory and his own experimental verification of it in his 1911 Solvay conference paper (cf. 1912, 189–216).

theory construction by other means." But experimentation is not just that! It is also (surprise!) a vehicle for testing theory and rendering it probable. In the discussion that followed Perrin's address to the French Philosophical Society, Perrin (1910b, 300) made a related point very forcefully:

> In reality this antagonism [between atomism and its opponents] translates two tendencies that oppose each other rather deeply: one that encourages us to make hypotheses in order to go forward, and another which warns off all suppositions that cannot inspire in us an immediate experiment which is immediately feasible. The energeticists classify the experiments and generalise them algebraically; the atomists look for ways to penetrate the mechanism, to go beyond it, and for this they imagine molecules … briefly put, they set themselves the problems that the energeticists consider to be superfluous and purely apparent. Let Brownian movement be h. Our experiments reveal a function of certain measurable quantities: $h = f(R, R')$. And also another function is given to us: $h = \varphi(A, A')$ (energy). If you fear the apparent problems, it suffices not to assume the existence of h and to set $f = \varphi$. The atomists, by contrast, try to guess something behind these functions, that is to say, to specify h.

A somewhat stronger point was made by Louis Couturat (Perrin 1910b, 293), who was present in the meeting alongside many others major French philosophers of the time. After claiming that one could see Perrin's achievements as aiming to ground on controllable and measurable facts a bunch of fictions, he offered the following riposte on Perrin's behalf:

> The fact that my [Perrin's] hypotheses are adapted to reality, and in so many different ways, this is what one calls an experimental verification; this is for us physicists the criterion of truth. Call that language whatever you want, always it is our language framework that 'sticks' to the facts.

7.5 The View from Above

What would be a reasonable way to spell out 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, viz., that matter is continuous.

The way I think Perrin's argument should be reconstructed is as follows. With the reasoning sketched in the previous section, Perrin has made available two important probabilities, viz.

$$\text{Prob}\left(\text{n=N/AH}\right) = \text{very high}$$
$$\text{Prob}\left(\text{n=N/-AH}\right) = \text{very low}$$

That is, the probability that the number *n* of molecules in a gram-molecule of a gas (including an emulsion, which does behave as a gas) is equal to the Avogadro number N given the atomic hypothesis is very high, while the probability that the number of molecules n is equal to the Avogadro number N given the denial of the atomic hypothesis is very low.

To see why Prob(n = N/−AH) = very low, recall Perrin's point (stressed in his address to the French Philosophical Society) that on the negation of atomic hypothesis, the predicted value of N could be *anywhere* between zero and infinity, which means that the probability that the predicted value of n would be *equal* to N would be zero. This does not mean that the negation of the atomic hypothesis implied that matter is discontinuous, with n≠N! Rather the negation of the atomic hypothesis (in the specific form advocated by Perrin, where n = N) is consistent with *any* value of N from 0 to infinity.

These two likelihoods (in the technical sense of the term) can be used to specify the Bayes factor f.

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

Bayes's theorem states

$$\mathrm{prob}\left(\mathrm{AH/n{=}N}\right) = \mathrm{prob}\left(\mathrm{n{=}N/AH}\right)\ \mathrm{prob}\left(\mathrm{AH}\right) / \mathrm{prob}\ \left(\mathrm{n{=}N}\right)$$

where:

$$\mathrm{prob}\left(\mathrm{n{=}N}\right) = \mathrm{prob}\left(\mathrm{n{=}N/AH}\right)\ \mathrm{prob}\left(\mathrm{AH}\right) + \mathrm{prob}\left(\mathrm{n{=}N/\text{-}AH}\right)\ \mathrm{prob}\left(\mathrm{\text{-}AH}\right)$$

Using the Bayes factor, Bayses's theorem becomes this:

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

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 very high.

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 the Avogadro number has the specific value it does (that is, that the number n of molecules in a gram molecule of a gas is equal to Avogadro's number N). 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 had aimed to show that, by eliminating several alternative potential explanations of Brownian movement, the atomic hypothesis had gained at least some initial plausibility which was reflected in its having some prior probability of being true.

This kind of reconstruction would (and does) explain Perrin's own *confidence* that the atomic hypothesis has been "established;" that he has offered "a decisive proof" of it (1916, 104). Admittedly, 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 *really* objective? If not, is the case for the reality of atoms really strong?

It is certainly arguable that prior probabilities express reasonable degrees of belief, which supervene on certain causal and explanatory qualities of a given hypothesis. So, prior probabilities need not be purely subjective or idiosyncratic degrees of belief — though reasonableness need not be determined in any algorithmic way.[6] In any event, the case in hand is quite peculiar for the following reason. It presented to anyone involved (and mainly to working scientists) a case in which the posterior probability of the atomic hypothesis becomes (almost) unity — given, of course, that it is assigned a *non-zero* prior probability, which it seems everybody but Duhem did.[7] This case might well be exceptional, but its role in the establishment and wide acceptance of the atomic conception of matter can hardly be underestimated.

There is another feature of Perrin's strategy that needs to be highlighted. In his (1916, 105) he claims:

> The objective reality of the molecules therefore becomes hard to deny. At the same time, molecular movement has not been made visible. The Brownian movement is a faithful reflection of it, or, better, it is a molecular movement in itself, in the same sense that the infra-red is still light.

Perrin's point here is precisely that size does not matter, but causal role does! Like Pasteur before him, Perrin did place the molecules firmly within the laboratory, grounding their causal role and offering experimental means for their detection and the specification of their properties — even though, the molecules did not become

[6] The whole issue is delicate, of course. But I think the following dilemma should be resisted: *either* prior probabilities should be fixed in a fully objective and logical manner (God-given? based on purely logical or synthetic a priori principles like the Principle of Indifference?) or else they are purely subjective and idiosyncratic and therefore useless in the defence of the rationality of the belief in theories. A priori probabilities can be whimsical, but they need not be. They can be based on judgements of plausibility, on explanatory considerations prior to the collection of fresh evidence and other such factors, which — though not algorithmic — are quite objective in that their employment does and should command rational agreement (As Perrin's case nicely illustrates).

[7] Is there any reason to take the broadly Bayesian reconstruction of Perrin's argument as being Perrin's own? There is some interesting circumstantial evidence, coming mostly from the fact that Émile Borel — who was Perrin's close friend and colleague — was an expert on probability theory and had actually made explicit reference to Bayes's theorem in his (1914). Borel (1914, 99) explicitly associated Bayes's theorem with the case of finding the probability of the causes (given their effects) and stressed that there is need to specify the a priori probability of the cause, though he admitted there was uncertainty as to how a priori probabilities were estimated. Borel did make many references to Perrin's statistical methods in his (1914).

visible. 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. Recall Perrin claim that the movement of the Brownian particles was a "faithful reflection" of the molecular movement, since the Brownian particles *were* large molecules. Perhaps, then it becomes clear why Perrin's argument could persuade almost everyone but Duhem, who took a very hard line on observability and denied the call for explanation-by-postulation.[8]

7.6 van Fraassen's Perrin Evaluated

van Fraassen does not really discuss Perrin's theoretical model in any detail, but he (2009, 20) does mention Perrin's claim that the Brownian particles behave as large molecules and hence obey the laws of gases. He adds:

> Perrin argues for its plausibility, but in terms that clearly appreciate the postulational status of this step in his reasoning. (…) On this basis, the results of measurements made on collections of particles in Brownian motion give direct information about the molecular motions in the fluid, always of course within the kinetic theory model of this situation. But that is just what was needed for empirical grounding of those remaining theoretical parameters.

As noted already, there need not be a tension between the need to ground empirically a theory and its being taken to be a plausible, or even a probable, theory. Perrin did try to ground empirically the atomic theory, but he did not try to do *just this* — at least in the way van Fraassen reads the claim of empirical grounding. It is perfectly consistent to try to ground empirically some theory and to claim that this theory is true, or by and large true (or at least that is highly confirmed by the measurements that ground it empirically). It is precisely this kind of stance that should be attributed to Perrin. Hence, empirical grounding — which turns out to be necessary for the enhanced testability of the theory — is a means to a broader end, viz., the confirmability of the theory. In Perrin's hand, the atomic-theory-based account of the Brownian motion did not just end up being confirmable, but was actually confirmed by a striking prediction of Avogadro's number.

In light of van Fraassen's overall stance, it might be tempting to think that it is enough to say of Perrin's strategy that it was aiming to show that the molecular hypothesis was empirically adequate. Or it might be that it was just aiming to lay to rest "the idea that it might be good for physics to opt for a different way of modelling nature, one that rivalled atomic theories of matter" (2009, 23). But here again, this kind of reading — especially in the latter form — is fully consistent with Perrin

[8] Some more general lessons for scientific realism that can be drawn from this case are discussed in my (2011).

aiming for more — as, in fact, he did. Actually, more can be said. We saw Perrin striving for an articulation of the theoretical mechanism (model) by means of which the all-important exponential law (1) was achieved. He could have started with the exponential law itself, without seeking to explain it. Striving for an explanation/grounding of this law simply does not make sense unless Perrin was aiming establish the causes of the Brownian motion.

There is a seemingly unexpected twist in Perrin's story that could not have escaped van Fraassen's acute attention. Didn't Perrin end up his (1910a) with the strange claim that the reference to molecules was dispensable? Indeed, he (1910a, 91) stressed the following:

> Lastly, although with the existence of molecules or atoms the various realities of number, mass, or charge, of which we have been able to fix the magnitude, obtrude themselves forcibly, it is manifest that we ought always to be in a position to express all the visible realities without making any appeal to elements still invisible. But it is very easy to show how this may be done for all the phenomena referred to in the course of this Memoir.

And then he proceeded to show how the very reference to Avogadro's number can be eliminated. Consider any two laws in which *N* features as a constant (e.g., Einstein's diffusion equation and the law of the distribution of radiation) and take their pure functional form. "The one," Perrin says,

> expresses this constant [Avogadro's number] in terms of certain variables, a, a', a", …,
>
> $$\mathrm{N}=f\left[\mathrm{a},\mathrm{a}',\mathrm{a}'',\ldots\right];$$
>
> the other expresses it in terms of other variables b, b', b", …,
>
> $$\mathrm{N}=g\left[\mathrm{b},\mathrm{b}',\mathrm{b}'',\ldots\right].$$
>
> Equating these two expressions we have a relation
>
> $$f\left[\mathrm{a},\mathrm{a}',\mathrm{a}'',\ldots\right]\equiv \mathrm{g}\left[\mathrm{b},\mathrm{b}',\mathrm{b}'',\ldots\right],$$
>
> where only evident realities enter, and which expresses a profound connection between two phenomena at first sight completely independent, such as the transmutation of radium and the Brownian movement (1910a, 91–92).

This way to proceed might well suggest that, in the end, Perrin wanted to show that the molecular hypothesis is eliminable: a scaffolding that may well be removed after connections between empirical phenomena have been established. van Fraassen does not quite put it like this and he warns us not to read the above passage in a philosophically loaded way. He adds:

> I do not offer this [passage] as a case of an apparent scientific realist contributing grist for the empiricist's mill! Rather, this passage is important because of how it illustrates the factors of *Determinability* and *Concordance* in empirical grounding.

This peace-offering however is unnecessary. Note, for one, that the "evident realities" which enter into the functional relations thus established are not merely observable magnitudes or properties of observable entities. On the contrary, it is evident that they are not. For instance, the diameter of the Brownian particles or the wave-length of emitted light are not observable. They are, however, determinate and measurable and this is what Perrin insisted one. More importantly, however, Perrin did not take it that the possibility of eliminating the constant *N* implied that molecules could be dispensed with. In (1910a, 92) he noted that the discovery of functional relations such as the above — which could not have been established without the atomic hypothesis — mark "the point where the underlying reality of molecules becomes part of our scientific consciousness."

And if this left any doubt to his reader about his commitments, in two subsequent publications in which he also presented *verbatim* the same idea of establishing functional relations among "evident realities" he added:

> But, under the pretence of rigour, we will not make the mistake to throw thus out of our equations the elementary magnitudes that allowed ourselves to obtain them. This would not be to remove a scaffolding that has become useless to the finished structure; it would be to mask the pillars that that have made its skeleton and beauty (1912, 250).

And in his *Les Atomes*, he put the point in a similarly graphical way:

> But, under the pretence of rigour, we will not make the mistake to avoid the operation of the molecular elements in the enunciated laws that we would not have obtained without their assistance. This would not be to uproot a useless stake from a thriving plant; it would be to cut the roots that nourish it and make it grow.[9]

Indeed, it seems it does not make good sense to read Perrin's claim about the role of relations of the form *f*[a, a', a", …] ≡ *g*[b, b', b", …], as van Fraassen does, as illustrating the factors of concordance and determinability. The various ways to specify Avogadro's number lack any kind of concordance unless they are taken to determine *Avogadro's number*; what were concordant were precisely the values of *N*, as they were determined in various ways. Given that the access to the molecules is only indirect — and given Perrin's insistence that their magnitudes should be determined and measurable — it was important to be shown that these magnitudes are essentially invariant irrespective of the observable phenomenon that leads to their calculation. The invariance of Avogadro's number was the key to proving the invariance of the molecular properties.[10]

Insofar as van Fraassen intends to hold on the general view that disbelief in a theoretical hypothesis is *always* a reasonable option, Perrin's case — looked at both from within and from above — shows that it is not. There are cases in which asserting the reality of certain entities is the only reasonable option.

---

[9] My translation from p. 284 of the French edition of *Les Atomes* (Flammarion, 1991). The rendering of this passage in the English translation of the book (1913, 207) is mistaken.

[10] A version of this point is made by Louis de Broglie (1945, 11).

7.7 Brownian Movement Was *not* a Public Hallucination

van Fraassen (2009) takes it that scientific instruments are not "windows on the invisible world" but rather "engines of creation" of new observable phenomena that theories have to save. In the case of microscopes, van Fraassen makes the rather astonishing move to consider the phenomena thus created to be "public hallucinations." Far from giving an image of some unobservable-by-the-naked-eye entities, the image seen in a microscope is just an image. Not an image of anything, but a public hallucination. The rainbow, van Fraassen says, is a public hallucination — there is no *bona fide* object that is a rainbow. It lacks certain invariances; it has no spatio-temporal position etc. But, he says, the images seen under a microscope too are public hallucinations. As he put it, "Nature creates public hallucinations" (2009, 103) and microscopes "imitate the ability of nature to create public hallucinations" (2009, 104). To be more precise, public hallucinations are

> a whole gallery of images which are not things, but are also not purely subjective, because they can be captured on photographs: reflections in the water, mirror images, mirages, rainbows (2009, 105).

Some of them, van Fraassen adds, are "copy qualified" in the sense that they lend themselves to being interpreted as images of real things. Microscope images, unlike rainbows and mirages, are copy-qualified: it makes sense to ask of them whether they represent something real or not.

His view, however, is that

> the microscope *need not* be thought of as a window, but is *most certainly* an engine creating new optical phenomena. It is accurate to say of what we see in the microscope that we are "seeing an image" (like "seeing a reflection," "seeing a rainbow"), and that the image could be *either* a copy of a real thing not visible to the naked eye or a mere public hallucination. I suggest that it is moreover accurate and in fact more illuminating to keep neutrality in this respect and just think of the images themselves as a public hallucination (2009, 109).

Why is keeping neutrality, one may wonder, more accurate and more illuminating? To show that it is not, let us look once more at the case of Brownian motion: the random and incessant motion of microscopic particles suspended in a liquid was observed through a microscope. Think of it as an image on the lens of the microscope. Let us state some of its properties.

First, the image co-varies with something else, viz., the liquid drop which is observed: if the liquid drop is removed, so is the image. So there is a correlation between the image and something else. Actually, there should be no doubt that this something else — call it X — (the liquid with the suspended microscopic particles in our case) *produces* (or at least essentially contributes to the production of) this image. Even if X were not the total cause of the image, it would be a substantial part of the cause since by removing X, the image is removed. It might be that when X is present, something else Y is present too — e.g., a distorting effect of the lens. But even then, Y could not produce the effect on its own. Let's call this *regularity*.

Second, the image is definite enough to be distinguished from other images that have *prima facie* similar causes — e.g., the image of liquid drops with no particles suspended in them. Let's call this *definiteness*.

Third, the image presents a certain temporal fixity. It can be present and be observed on slides that had been conserved for decades under all kinds of external conditions (cf. Nye 1972, 24). Let's call this *resilience*.

Fourth, the image displays certain important invariances. For instance, Robert Brown himself noted that the random movement occurs when pollen of various plants were used. In fact, between 1830 and 1870, physicists and biologists used a great variety of organic and inorganic particles: sulphur, mastic, cinnabar, pulverised coal, India ink and gamboge and they suspended in a variety of fluids (see Nye 1972, 23). Let's call this *invariance*.

Fifth, the image does not go away whenever certain factors are involved in the preparation of the emulsion: sunlight and darkness, electricity and magnetism, temperature variations etc. (cf. Nye, op.cit.) Let's call this *robustness*.

Sixth, the image is manipulable — by manipulating its causes. Perrin was an actual master of this. He used various materials (e.g., gamboge and mastic); he prepared the granules in various meticulous ways; he used various methods to avoid sources of error etc.[11] Let's call this *manipulability*.

I very much doubt that these properties can be had by the rainbow. Or by anything which cannot reasonably be taken to be "copy qualified" image. But let's not argue about this directly. Let's take an indirect route. It was exactly the possession of properties like these that rendered necessary (and desirable) an explanation of the Brownian images. The explanation could proceed at two levels — one intrinsic, the other extrinsic. At the intrinsic level, it would have to be an explanation of the image in terms of the properties of the causes of the image — that is, of the properties of the liquid and of the suspended in it particles. It would require thinking of the image as an image of something — of what is going on *within* the liquid — and would proceed by eliminating various hypotheses as to what is going on within the liquid by eliminating, in effect, various competing images that one would have expected to see in the microscope had the alternative hypotheses of the origin of the image been true. This is more or less the actual course of events, until and during Perrin's experimental work on the Brownian motion. Its very possibility is predicated on taking the image (of Brownian motion) to be the image of whatever it is *within* the liquid that causes it; to be, an image, as Perrin put it, of the internal agitation of the fluid (cf. 1910a, 5). The image did not render visible the molecular movement, but as Perrin himself put it, it was nonetheless a "faithful reflection" of it since the Brownian particles, of which there were copy-qualified images, were large molecules.

[11] Perrin presents in painstaking detail the various ways in which he manipulated the emulsions that he studied and his various attempts to establish concordances between the values of the properties of the Brownian particles (cf. 1910a, §§15–22). At one point he described how he had to wait for 2 or 3 days for various protozoa to die that had developed in an emulsion which had not been rendered aseptic. The bacteria "fell inert to the bottom of the preparation" (1910a, b, 41). For an illuminating discussion of the use of the ultramicroscope from Perrin, cf. Bigg (2008).

The other level at which the explanation of the Brownian images could proceed, as I already said, would be extrinsic. Note that by that I do *not* mean the search for external causes — that is causes of the observed random motion of the Brownian particles that operate outside the liquid, e.g., the road traffic, the effects of which on the microscope a few experimenters tried to shield. This is absolutely fine and intrinsic *in the above sense*. It is an attempt to exclude alternative hypotheses that would have led to alternative images, had they been true. What I mean by "extrinsic explanation" of the image is an explanation of why *this* particular image arises as opposed to anything else. And this would be an explanation of why observers like us see an image like that when they place their eyes in contact with a microscope and a certain liquid is put on the film. This course of action would be absolutely natural, had there been thought that the Brownian image was a public hallucination. Then, the course of the explanation would be very much like the course of the explanation of why we see the rainbow while there is no such thing as the rainbow (or similarly why we see the blue sky though there is no such thing as the blue sky). Differently put, the intrinsic explanation would aim to answer the question of why there is an image like this — and would proceed by examining what it is an image of. The extrinsic explanation would proceed by aiming to answer the question of why we *see* an image like this — and would proceed by examining the causes of our *seeing* it. The intrinsic explanation — the one that was actually pursued — required thinking of the image as a copy-qualified image, while the extrinsic explanation would require thinking of the image as a public hallucination to be explained away *qua* an image of anything.

In the case of Brownian motion, van Fraassen's recommended neutrality is neither illuminating nor accurate. It certainly does not tally with viewing the history of the work on the Brownian motion *from within*.

It might be ironic that when Emile Meyerson (1912 [1930], 90) discussed the shift of opinion in favour of the atomic conception of matter, he noted the following:

> (A)t first sight one is almost tempted to ask whether these investigators [of the Brownian movement] have not been victims of an illusion in this case, if they have not succumbed to an unconscious trick of their own minds.

But he immediately added that looking at the results Perrin had produced entirely frees is from these doubts.

7.8 Merging the Two Views

There is no theory-free perspective on reality. But this does not mean that there is no way to form a reasonable belief about what reality is like. Theories are apt for confirmation and well-confirmed theories do offer good reasons (based on the link between theory and evidence) to think of reality as being in a certain way. van Fraassen is right when he stresses that a God-like view of nature is impossible, but wrong when he takes it that, *because of this*, the deep-structure of reality is impenetrable. Denying the impossible should not make us blind to the possible. When all goes well, theory and experiment — or measurement practices — develop hand in hand. Theory is under constant pressure to render its theoretical parameters determinate and measurable. Measurement both makes theory develop and tests it. When viewed *from within*, theories develop by being in constant interaction with experience. When viewed *from above*, theories are assessed on the basis of experience. The stereoscopic view we have been looking for aims to combine the process and the product. Perrin's case shows that this stereoscopic view need not leave us in the dark as to what the texture of reality is. Indeed, van Fraassen's narrative of Perrin's achievement refuses — ultimately — to view the theory from above; that is, to unravel the general reasoning pattern which made Perrin's achievements so decisive in turning the balance in favour of the atomic conception of matter.

I just want to repeat it: there are cases in which asserting the reality of certain entities is the only reasonable option. This is Perrin's case. Might that be too strong a claim to make? Historically, it has been brought out. Duhem's denial till the bitter end was based on philosophical dogmatism — in essence, in assigning a zero prior probability to the atomic conception of matter, based on the claim that it's primarily an explanatory hypothesis and such hypotheses fall outside science. But even if we leave the actual history out of the picture — as we should *not* — the broader philosophical point is this. What Meyerson aptly called "impartial observers" are precisely those scientists or philosophers who are epistemically open: they can change their minds when decisive evidence in favour of (or against) a hypothesis becomes available — evidence that meets ordinary scientific criteria of relevance and strength. I find it hard to believe that an impartial observer of Perrin's achievements could reasonably resist Perrin's conclusions.

References

Bigg, C. (2008). Evident atoms: Visuality in Jean Perrin's Brownian motion research. *Studies in History and Philosophy of Science, 39*, 312–322.

Borel, E. (1914). *Le Hasard* (2nd ed., 1920). Paris: Librairie Félix Arcan.

Brush, S. (1968). A history of random processes. *Archive for History of Exact Sciences, 51*, 1–36.

de Broglie, L. (1945). *La Réalité Des Molécules et L'Œuvre de Jean Perrin* (Académie des Sciences). Paris: Gauthiers-Villars.

Einstein, A. (1905 [1956]). On the movement of small particles suspended in a stationary liquid demanded by the molecular-kinetic theory of heat. In R. Furth (Ed.), *Investigation on the theory of the Brownian movement* (pp. 1–18). New York: Dover Publications.

Glymour, C. (1975). Relevant evidence. *The Journal of Philosophy, 72*, 403–426.

Gouy, L. (1895). Le Mouvement Brownien et les Mouvement Moléculaires. *Revue Générale des Sciences Pures et Appliquées, 6*, 1–7.

Meyerson, E. (1912 [1930]). *Identity and reality* (K. Loewenberg, Trans.). New York: Macmillan.

Nye, M. J. (1972). *Molecular reality: A perspective on the scientific work of Jean Perrin*. London: MacDonald.

Nye, M. J. (1976). The nineteenth-century atomic debates and the dilemma of an indifferent hypothesis. *Studies in History and Philosophy of Science, 7*, 245–268.

Ostwald, W. (1896). The failure of scientific materialism. *Popular Monthly, 98*, 589–601.

Ostwald, W. (1907). The modern theory of energetics. *The Monist, 17*, 481–515.
Ostwald, W. (1912). *Outlines of general chemistry* (3rd ed., W. W. Taylor, Trans.) London: MacMillan.
Perrin, J. (1901). Les hypothèses moléculaires. *Revue Scientifique, 15*, 449–461.
Perrin, J. (1903). *Traité de Chimie Physique: Les Principes*. Paris: Gauthier-Villars.
Perrin, J. (1910a). *Brownian movement and molecular reality*. (F. Soddy, Trans.). London: Taylor and Francis.
Perrin, J. (1910b). Le Mouvement Brownien. *Bulletin de la Société Française de Philosophie*, *10*(4), 265–302. Séances du 27 Janvier et du 3 Mars 1910. Paris: Vrin.
Perrin, J. (1911). La Réalité des Molécules. *Revue Scientifique*, *25*, 774–784.
Perrin, J. (1912). Les Preuves de la Réalité Moléculaire (Etudes Spécial des Emulsions). In P. Langevin & L. de Broglie (Eds.), *La Théorie Du Rayonnement et les Quanta* (pp. 153–253). Paris: Gauthier-Villars.
Perrin, J. (1916). *Atoms*. (D. L. Hammick, Trans.). London: Constable and Company Ltd.
Poincaré, H. (1900). Les Relations Entre la Physique Expérimentale et la Physique Mathématique. In C.-E. Guillaume & H. Poincaré (Eds.), *Rapports Présentés au Congrés International de Physique de 1900* (Vol. 1, pp. 1–29). Paris: Gauthier-Villars.
Poincaré, H. (1906). The principles of mathematical physics. In H. J. Roberts (Ed.), *St Lewis International Congress of Arts and Science* (pp. 604–622). London: University Alliance.
Poincaré, H. (1913). *Mathematics and science: Last essays*. New York: Dover.
Post, H. R. (1968). Atomism 1900 I & II. *Physics Education*, *3*, 225–232 and 307–312.
Psillos, S. (2011). Moving molecules above the scientific horizon: On Perrin's case for realism. *Journal for General Philosophy of Science, 42*, 339–363.
Thomson, W. (1870). On the size of atoms. *Nature, 1*, 551–553.
van Fraassen, B. (2008). *Scientific representation: Paradoxes of perspective*. Oxford: Clarendon Press.
van Fraassen, B. (2009). The perils of Perrin, in the hands of philosophers. *Philosophical Studies, 143*, 5–24.
Weyl, H. (1927 [1963]). *Philosophy of mathematics and natural science*. New York: Atheneum.