**Towards a Mutually Beneficial Integration of History and Philosophy of Science: The Case of Jean Perrin**

**Klodian Coko**

1. **Introduction**

Since the 1960s, there have been many efforts to defend the relevance of History of Science to Philosophy of Science, and *vice versa.* For the most part, these efforts have been limited to providing an abstract rationale for a closer integration between the two fields, as opposed to showing: (a) how such an integrated work is to be produced concretely, and (b) how it can lead us to a better understanding of past and/or present science than if historical and philosophical perspectives are employed separately.[[1]](#footnote-1) In this chapter, I argue that one of the most promising ways to integrate history and philosophy of science is the historicist-hermeneutic approach to iHPS. I will present the main features of the historicist-hermeneutic approach and will show, concretely, how it can provide a mutually beneficial integration of History and Philosophy of Science. More specifically, I will employ the historicist-hermeneutic approach to elucidate one of the most problematic historical case studies in philosophy of science: Jean Perrin’s argument for molecular reality at the beginning of the twentieth century.

Jean Baptiste Perrin (1870-1942) was a French physicist who is widely credited – by historians of science - with providing the conclusive evidence for the existence of atoms and molecules,[[2]](#footnote-2) and thus ending the long nineteenth century debates over the existence of these unobservable entities.[[3]](#endnote-1) The most famous part of Perrin’s argument was his description of thirteen different ways to determine Avogadro’s number (*N*): the number of atoms, ions, and molecules contained in a gram-atom, gram-ion, and gram-mole of a substance, respectively. The different determinations included Perrin’s own three, which were based on the experimental study of the height distribution, mean displacement, and mean rotation of Brownian particles, respectively.[[4]](#footnote-3)

As well as being of interest to historians of science, Perrin’s argument has also been the focus of much interest from philosophers of science. This interest is justified by the ability of Perrin’s argument to quickly and successfully end the nineteenth century atomic debates. We can discern two, relatively independent, philosophical treatments. On the one hand, Perrin’s argument is often presented as a case of *multiple determination*. Multiple determination is the epistemic strategy of using multiple and independent procedures to establish the same result.[[5]](#endnote-2) It is widely regarded as a very important strategy by both working scientists and philosophers of science.[[6]](#footnote-4) One contrived example that is used to illustrate the multiple determination strategy is that of independent witnesses: if several witnesses testify that an event occurred, and we can be certain that the witnesses’ testimonies are independent (that is, if we can be certain that the witnesses did not base their testimonies on one another, that they were not coached, that they were not bribed by some villain, and so on), we can safely conclude that the event did occur. It would be an improbable coincidence for multiple witnesses, independently of one another, to fabricate the exact same story. In the context of discussing the merits of multiple determination, Perrin’s argument is presented as the paradigmatic case demonstrating the epistemic force of the strategy. When it comes to providing the specific grounds underlying Perrin’s argument, however, not much it is said besides – what we may call – ‘the blunt rationale’ for multiple determination; namely, that it would have been a highly improbable coincidence for thirteen independent procedures to point at the same value for the number of molecules contained in a unit of substance, and yet for there not to be any such things as molecules. Not much analysis is provided regarding the role that multiple determination played in Perrin’s experimental work and in convincing the scientific community. What makes matters more perplexing is that, on the other hand, philosophers who have looked more closely at Perrin’s argument, have reached different (and often contradictory) conclusions, not only regarding the structure of Perrin’s argument, but also regarding the role that the multiple determination of *N* played in it.[[7]](#endnote-3)

I argue that the integration of historical and philosophical perspectives provided by historicist-hermeneutic approach is necessary for understanding the structure of Perrin’s argument, in particular, and for developing a conceptual framework for understanding the structure and the epistemic force of the multiple determination strategy, in general. By following the historicist-hermeneutic approach, I emphasize both the historical context and the temporal development of Perrin’s argument (as opposed to only looking at its final and finished form). I locate the main elements responsible for the argument’s success. I argue that Perrin’s argument was the result of his clear understanding of the philosophical and scientific challenges facing the empirical verification of claims regarding the existence of unobservable entities, such as atoms and molecules, at the beginning of the twentieth century. Perrin’s efforts were influenced by the late nineteenth century recognition that an experiment in physics, in general, and the experimental investigation of unobservable entities, in particular, required the use of complex instruments and experimental procedures as well as the employment of many theoretical and other auxiliary assumptions. Lacking direct observable evidence for the existence of atoms and molecules, Perrin’s efforts were concentrated on what he considered to be the next best thing: the determination of the various (hypothetical) molecular magnitudes *via* independently theoretically-dependent routes. The extremely remarkable agreement on the numerical values for the molecular magnitudes determined by independently theoretically-dependent determinations gave rise to a strong no-coincidence argument that was used to argue both for the correctness of the values determined and the validity of the theoretical and other auxiliary assumptions underlying the different determinations. The argument’s structure, however, was more complex than the one encountered in the blunt rationale. There were structural elements of Perrin’s argument which, although are neglected in the various philosophical accounts, were responsible for its strength and, ultimately, for its success. They were, namely: (a) the numerical or quantitative nature of the agreement, (b) the close agreement on the numerical values calculated (especially if one considered the *a priori* improbability of such an agreement), (c) the theoretical independence of the determination procedures, (d) the genetic independence of both the determination procedures and of the agreement achieved, (f) the high quality and reliability of some of the determinations, (g) the final lack of discordant results, (h) the ability to conclusively explain away any objections and discordant results when they eventually emerged, and (i) the relatively high number of determinations.

The chapter is structured as follows. In section 2, I present some alternative approaches to iHPS and I indicate their shortcomings. In the following sections I employ the historicist hermeneutic approach to elucidate the reasoning underlying Perrin’s work on Brownian movement and his argument for molecular reality. In section 3, I present Perrin’s early views on scientific methodology and which were very influential in shaping his experimental reasoning. In sections 4 and 5, I describe Perrin’s early experimental work on the phenomenon of Brownian movement. In section 6, I present the importance of this experimental work for Perrin’s argument for molecular reality. In section 7, I show the epistemic import of the independent determinations of Avogadro’s Number. In section 8, I present Perrin’s later experimental work on Brownian movement and its importance for his argument for molecular reality. In section 9, I conclude by showing the necessity of the historicist-hermeneutic approach for understanding the structure and epistemic import of the multiple determination strategy.

1. **Jean Perrin and Traditional Approaches to HPS**

Perrin’s argument for atomism was successful in ending nearly one hundred years of debates over the existence of unobservable atoms and molecules. It is not a surprise that it has been the focus of much philosophical interest. We can discern two, relatively independent, philosophical treatments. Firstly, Perrin’s argument is often presented as a case of multiple determination. There are philosophers who claim that the ability to establish the same result by means of independent procedures is an important epistemic strategy. To support this claim, these philosophers refer to Perrin and to his thirteen determinations of Avogadro’s number.[[8]](#footnote-5) Perrin’s argument is presented as a no-coincidence argument and its underlying rationale as being something along these lines: it would have been an improbable coincidence for thirteen different determination procedures to arrive at the same value for the number of molecules contained in a unit of substance and yet for such things as molecules not to exist. Other than the offering of this rationale, however, not much analysis is devoted to the actual role that the multiple determination of *N* played in Perrin’s experimental work, or in convincing the scientific community.

Secondly, Perrin’s argument has been the subject of detailed case-studies by other philosophers of science. The proclaimed goal of these case-studies is to provide the exact reasoning underlying Perrin’s argument for molecular reality. This enormous philosophical interest in Perrin’s argument is explained by the fact that, just in a matter of few years (roughly 1908-1913), Perrin was able to end nearly one hundred years of debates over the existence of the unobservable atoms and molecules. The authors of these case-studies have tried to appropriate Perrin’s argument for their own purposes. Unsurprisingly, these philosophers have arrived at contradictory conclusions, not only regarding the reasoning underlying Perrin’s argument, but also regarding the role that the multiple determination of *N* played in it. For example, Perrin’s argument has been interpreted as an ‘inference to the best explanation’.[[9]](#footnote-6) Clark Glymour, on the other hand, regarded Perrin’s case as an instance of his account of ‘bootstrapping confirmation’.[[10]](#footnote-7) Wesley Salmon claimed that Perrin’s argument has the structure of a ‘conjunctive common cause argument.[[11]](#footnote-8) Nancy Cartwright, building on Salmon’s ‘common cause’ interpretation, has argued that Perrin made an ‘inference to the most probable cause’.[[12]](#footnote-9) Cartwright’s interpretation was in turn challenged by Deborah Mayo. For Mayo, Perrin’s argument constituted a ‘severe testing’ of the molecular explanation of the phenomenon Brownian movement.[[13]](#footnote-10) Peter Achinstein has claimed that his account of evidence offers the best interpretation of Perrin’s reasoning.[[14]](#footnote-11) Achinstein’s interpretation has been criticized by Stathis Psillos, who argues that Perrin used Bayesian reasoning to provide a crucial experiment for the reality of atoms.[[15]](#footnote-12) Bas van Fraassen has argued that Perrin did not intend to, and thus did not establish the real existence of atoms and molecules. He only provided ‘empirical grounding’ for one of the most important parameters of the kinetic theory: Avogadro’s Number.[[16]](#footnote-13) Robert Hudson has claimed that Perrin’s was not a case of multiple determination (or ‘robustness’, as he calls it), but a case of ‘reliable process reasoning’. Perrin possessed a very reliable process for determining *N:* the one based on his experimental study of the height distribution of Brownian particles. What is usually interpreted as a multiple determination of *N* was in fact a process of calibration. Perrin used his most reliable determination process to test the accuracy of the other determinations.[[17]](#footnote-14) We are thus left with a number of different interpretations regarding both Perrin’s reasoning for molecular reality and the role that the multiple determination of *N* played in it. It is not difficult to locate the source of the problem. All the above interpretations belong to a flawed way of integrating History and Philosophy of Science. Attention now will be turned to considering the weaknesses of this and other efforts to integrate History and Philosophy of Science.

**2.1 The Illustrative Model of iHPS**

All the interpretations of Perrin’s argument mentioned belong to the illustrative use of history (or ‘illustrative model of iHPS’). The illustrative model of iHPS is explained in the following way: Philosophers of science have pre-established conceptions - regarding, for instance, the nature of scientific explanation, the nature of the relationship between theory and evidence, the nature of theory, - and they often use the historical material to illustrate or offer support for these pre-established conceptions. There is, therefore, the danger that the philosophers might have misunderstood, or even intentionally distorted, the argumentative reasoning of the scientists they study. The following quote from Ernan McMullin identifies the major shortcomings and pitfalls of the illustrative approach:

It makes use of the great scientists of the past as lay figures in what seems to be a historical analysis but really is not. They are manipulated to make a philosophical point which, however valid it may be in itself, was really not theirs, or at least is not really shown using the proper methods of the historian to have been theirs.[[18]](#footnote-15)

The problem with the illustrative use of history is an old complaint that historians have against philosophers. There have been several attempts to deal with this problem. In the next two subsections, I describe the two most influential approaches.

**2.2 The Confrontation Model of iHPS (The VPI Program)**

One way to deal with the problems generated by the illustrative model of iHPS is the so-called ‘confrontation model of iHPS’.[[19]](#footnote-16) This approach was supported by philosopher of science Larry Laudan and his group at the Virginia Polytechnic Institute in the 1980s.[[20]](#footnote-17) Their proposal was that instead of letting philosophical pre-conceptions influence the interpretations of historical facts, we should use the historical material to test philosophical theses, more or less in the same way that scientists use empirical evidence to test theoretical hypotheses. Although it was in fashion during the 1980s, this approach to iHPS is now considered outdated.

**2.3 The Methodology of Historiographical Research Programmes**

Another way to address the problems with the illustrative use of history was proposed by philosopher of science Imre Lakatos and was put into fruition by his disciples.[[21]](#footnote-18) This approach is known as the ‘Methodology of Historiographical Research Programmes.’ It suggests that scholars: (a) should embrace the fact that historical material is always influenced by philosophical interpretations, (b) should try to impose different philosophical interpretations on the historical material (this process is known as a ‘rational reconstruction’ of the historical material), and (c) choose the philosophical interpretation that offers the most consistent account of the historical material. Although popular in the late 1970s and the 1980s, this approach was also eventually abandoned.

One can easily find in the literature a compendium of the problems with each one of these three approaches to iHPS, as well as analyses of why they all lead straight to ‘disaster’.[[22]](#footnote-19) I will only mention what I think is the greatest weakness which they all have in common; this is the fact that there is nothing historical about them. The philosophical conceptions that are illustrated, tested, or used to rationally reconstruct the historical material, are presented as fixed and eternal; as applicable to all times and all places, rather than to a specific time, place, and context. These approaches, therefore, fail to pay attention both to the historical context and to the historical development of knowledge.

**2.4 The Historicist-Hermeneutic Approach to iHPS**

I suggest a more promising way to integrate historical and philosophical accounts of scientific practice: the ‘Historicist-Hermeneutic Approach to iHPS’.[[23]](#footnote-20) Aspects of this approach have been supported by various scholars during the second half of the twentieth century.[[24]](#footnote-21) As its names indicates, this approach has two main parts: historicism and hermeneutics. The term ‘historicism’ has been used in many ways in the history of philosophy. In this context it refers strictly to the very simple and intuitive, and yet often neglected, idea that the best way to understand something is to know how it came about. Philosopher of science Dudley Shapere described concisely the historicist approach for understanding science:

The question of why science today believes the peculiar things it does about the universe, and why it is willing to consider the alternatives it does, requires attention to the question of how science has come to think in those ways.[[25]](#footnote-22)

For Shapere, science is a self-sustainable and self-generating enterprise which has an intrinsically temporal dimension. If the philosopher’s job is to understand science, paying attention to this temporal dimension is essential. It is not enough to consider a ‘slice’ of scientific work at a moment in time.

The ‘hermeneutic’ part of the approach can also be understood by using another of Shapere’s dictums: ‘We learn how to learn as we learn’.[[26]](#footnote-23) This dictum also reflects the idea that science is a self-transforming enterprise with no concepts and methodological precepts that are applicable to all times and all places. Supporters of the historicist-hermeneutic approach argue that this dictum applies also at the meta-level: to the philosophical efforts for understanding science. We are continuously bettering our philosophical understanding of science as we study science. There are no fixed and eternal things here either.

According to the historicist-hermeneutic approach, History of Science and Philosophy of Science are not separate endeavors, but fields of inquiry which are already closely intertwined with one another. The use of philosophical notions, tools, and concepts, such as *theory*, *experiment*, *evidence*, *hypothesis*, *confirmation*, *multiple determination*, and so on, is necessary for understanding even a single episode in the history of science (this is the case even if this use of philosophy may go unnoticed by the historians themselves). The insight offered by the historicist-hermeneutic approach is that we can use this necessary intertwinement for the betterment of both philosophy of science and history of science. For instance, we can improve our understanding of philosophical notions such as the ones mentioned above, by studying concrete historical episodes where such notions are employed and by studying their historical development through time. We can then use these improved and more precise philosophical notions to achieve a better understanding of other concrete episodes from past and/or present science, and then use the conclusions arrived at to achieve further elaboration and refinement of the said philosophical notions and of their historical development, and so on and so forth.[[27]](#footnote-24)

In the rest of the chapter, I will use the historicist-hermeneutic approach to understand the reasoning underlying Perrin’s argument for molecular reality, which, as we saw in this section, is one of the most problematic historical case studies in philosophy of science. My approach is historicist because I will argue for the importance of the temporal development of Perrin’s thought for understanding the reasoning underlying his argument, and especially for understanding the role that the independent determinations of *N* played in it. I argue that one important reason why the various philosophers have arrived at such different assessments of Perrin’s argument is because (insofar as they make use of Perrin’s own writings at all) their accounts are based on final versions of Perrin’s argument. Almost all the philosophical interpretation of Perrin’s argument mentioned above are based on the English translations of Perrin’s influential 1909 paper ‘Brownian Movement and Molecular Reality’ and of his 1913 book *The Atoms*.[[28]](#endnote-4) From these writings, the philosophers cherry-pick the elements of Perrin’s argument that are most beneficial to their philosophical positions and disregard the elements that do not fit. My approach is also hermeneutic, because I do not have as a starting point some preconceived philosophical position which I will try to illustrate, test, or use to rationally reconstruct the historical material. My starting point is the blunt rationale for multiple determination, which will be further clarified and elucidated in the end by its ‘friction’ with the historical material.

1. **Perrin’s Early Methodological Views**

Jean Baptiste Perrin was born in Lille, on September 30, 1870.[[29]](#footnote-25) He was raised in Lyon, where he also received his early education. He moved to Paris to enter a class of special mathematics at the *lycée Janson de Sailly*. Studying under Émile Lacour, young Perrin was encouraged to prepare for the *École Normale Supérieure*. He entered the prestigious Parisian school in 1891. He was immediately attracted to experimental physics and studied under Marcel Brillouin, one of the few French scholars who supported the kinetic theory of gases at the time. In 1895, after refusing a teaching position in secondary education, Perrin was appointed *agrégé-préparateur* at the École Normale. At the same time, he began his experimental work, first on cathode and then on Röntgen rays. This early experimental work aimed to provide experimental evidence for the existence of atoms. It resulted in his doctorate thesis of 1897.[[30]](#footnote-26)

Perrin’s earliest thoughts on scientific methodology can be found in his book *Traité de Chimie Physique: Les Principes*, published in 1903.[[31]](#footnote-27)From the book’s general approach it is clear that, for Perrin, what was at stake in the contemporary atomic debates was not simply the question of whether there was enough evidence to warrant belief in the existence of atoms and molecules, but even more fundamentally, whether it was necessary for physical science to postulate the existence of such unobservable entities, and what could count as confirmatory evidence for their existence.

In the book’s preface, Perrin discusses the two fundamental methods of physical science. The first is the inductive method, which is characterized by a sure and slow march, from the recording of particular empirical facts, to the formulation of general principles. It is a method characterized by the defiance of all mystery and metaphysics and the disdain for everything that cannot be reduced to perceivable empirical facts.[[32]](#footnote-28) Opposite the inductive method stands the deductive method, which mostly provides “explanations of the visible by the invisible”. More specifically, the deductive method “consists in imagining for matter a structure the direct perception of which still escapes our imperfect senses, and such that its knowledge would allow to predict in a deductive manner the visible properties of the universe.”[[33]](#footnote-29) Contrary to the prevailing philosophical atmosphere of the *fin de siècle* in France, Perrin’s argues that, rather than being incompatible, the two methods can be fruitfully combined to investigate the properties of matter which escaped empirical detection. This could be achieved without abandoning the inductive principle that physical science is fundamentally based on empirical facts, and without rescinding into metaphysics. One simply had to accept the intuitive idea that what is empirically detectable is not limited to what is currently detectable, but that it can be extended with the development of new methods and the invention of more advanced scientific instruments. For Perrin, the atomic-molecular hypotheses had proved their fruitfulness and legitimacy by being able to deductively predict a variety of facts, which were then empirically confirmed. What was still needed, was direct empirical observation that would transform these hypotheses into a confirmed reality.

*Les Principes* was a textbook aiming to present the fundamental principles of physical chemistry. It did not do much for expounding the existing evidence in favor of the molecular hypothesis. In an article he had published in 1901 in the journal *Revue Scientifique* – which was addressed to a wider audience – Perrin had presented this evidence in a detailed manner and had explained the nature of support it provided for the molecular hypothesis.[[34]](#footnote-30) Most of the evidential support came from the molecular hypotheses made in the context of the kinetic theory of gases.[[35]](#footnote-31) These hypotheses not only offered explanations for the visible properties of gases and liquids*,* but also offered numerical approximations for various molecular magnitudes such as the velocity and the diameter of molecules, and Avogadro’s number (*N*). Perrin recognized that, in itself, this was not a strong argument for the existence of molecules, or for the validity of the molecular values calculated.[[36]](#footnote-32) There was nothing remarkable about the ability of molecular-kinetic hypotheses to explain known facts or provide theoretical values for the molecular magnitudes; these hypotheses were constructed in the first place exactly in order to explain the observable facts. The importance of these first numerical approximations consisted in that they could be compared with values for the same magnitudes derived independently from the investigation of other phenomena. The atomic chemical theory, for example, was another hypothesis which was invoked to explain the empirical evidence from the chemical combination of substances. The explanations of the phenomena of electrolysis and of the newly discovered phenomena of cathode and X-rays had also given rise to yet other hypotheses regarding the discontinuous structure of matter - although at a level deeper than that of molecules.[[37]](#endnote-5) If one could derive similar values for the molecular magnitudes from the consideration of such diverse phenomena, then one could put forward a very strong argument for molecular reality, which was the next best thing in lack of direct empirical observation.[[38]](#footnote-33)

1. **The Phenomenon of Brownian Movement and the Qualitative Triangulation of Molecular Reality: The Molecular Hypothesis as a *Logical Induction***

Beginning in 1901, and while studying the properties of colloids, Perrin became fully acquainted with the phenomenon of Brownian movement: the incessant and completely irregular movement of microscopic particles when suspended in liquids.[[39]](#footnote-34) Although it was known for the most part of the nineteenth century, it was only during the 1870s that the importance of the phenomenon for the kinetic-molecular hypothesis was recognized.[[40]](#footnote-35) Perrin’s main source on the topic was the work of the French physicist Léon Gouy who, in the end of the nineteenth century, had experimentally established the basic properties of the phenomenon and had demonstrated its independence from all imaginable external influences.[[41]](#footnote-36)

From his earliest writings on the topic, Perrin argued that the phenomenon of Brownian movement offered a different kind of evidence for molecular reality from the *a priori* considerations made in the kinetic theory of gases.[[42]](#footnote-37) Whereas in the kinetic theory one postulated *a priori* a molecular structure for matter from which to *deduce* the observable facts, the phenomenon of Brownian movement moved in the opposite direction; it provided directly observable evidence that could be used to *inductively* infer a molecular structure for matter. It provided, what Perrin calls, a *logical induction*.[[43]](#footnote-38)

In sum, Perrin’s argument was the following. The basic characteristics of Brownian movement, as established by Gouy and other nineteenth century investigators were: unceasingness, complete irregularity, dependence on the size of the suspended particles, dependence on the temperature of the suspending liquid, independence from the nature of the particles, and independence from any external influences. These characteristics led naturally to the conclusion that the phenomenon was caused by the internal movements of the liquid itself. There is, therefore, a continuous movement of the internal parts of the liquid. The distribution of motion in a fluid does not de-coordinate indefinitely. Therefore, the liquid ought to be composed of elastic granules which are in permanent motion. If such granules have no existence, it is not apparent why there is a limit to the de-coordination of motion, and how a phenomenon such as that of Brownian Movement is possible. For Perrin, the empirical examination of Brownian Movement alone, independently of any kinetic considerations, was sufficient to logically suggest that every fluid is composed of elastic granules, animated by a perpetual motion.[[44]](#footnote-39)

Now, we only need to call these granules *molecules*, in order to recognize an old hypothesis, glimpsed by the intuition of Epicurus and Lucretius, revived and clarified by Bernoulli, and developed by Clausius and Maxwell. Only that, this hypothesis is no longer in our eyes *a priori*: it ranks as a logical induction, inspired from the observation of phenomena, in the same way that, for example, the undulatory theory of light is inspired, but not imposed, by the known properties of light.[[45]](#footnote-40)

The phenomenon of Brownian movement provided thus an inductive argument for the existence of unobservable molecules, independent from the deductive argument provided by the kinetic theory of gases. The logical induction fell short of establishing the kinetic-molecular explanation of Brownian movement, because it did not prove that the discontinuous parts of the liquid causing the phenomenon were the same (or even were of the same order of magnitude) with the molecules postulated by the kinetic theory of gases. To establish this identity, what was needed was *an independent quantitative determination*. That is to say, one would have to use the observable properties of Brownian movement to calculate the quantities of the magnitudes causing them, and then compare the results with the values for the molecular magnitudes provided by the kinetic theory of gases. This is exactly the experimental path that Perrin followed, beginning in 1908.

1. **The Quantitative Triangulation of Molecular Magnitudes**

Perrin argued that the best way to connect the observable characteristics of Brownian movement with the kinetic-molecular movements (supposedly) causing them, was to consider the suspended particles as giant molecules (for example, like molecules of sugar in a solution of sugar water).[[46]](#footnote-41) To establish the identity of the granular movements causing the Brownian movement with the molecular movements postulated by the kinetic theory, one had to triangulate: one had to calculate the kinetic energy of a Brownian particle and compare it with the kinetic energy that the kinetic theory had deduced for an isolated molecule at the same temperature. The ingenuity of Perrin’s experimental approach consisted in finding a way to calculate the kinetic energy of a Brownian particle that did not require the calculation of its velocity, which, by this time, it was realized that it was impossible to measure.[[47]](#footnote-42) Perrin developed the hypothesis that the Brownian particles of a homogeneous emulsion, because of their irregular movements, ought to distribute themselves in the same way as the (hypothetical) air molecules under the influence of gravity. It was known since the eighteenth century that the density of a gas in equilibrium decreases with altitude according to an exponential law.[[48]](#footnote-43) Perrin’s idea was that, if he could establish that the height distribution of Brownian particles obeyed the exponential law, he would have confirmed the hypothesis that the gas laws extended to Brownian particles and, thus, have in the behavior of the suspended Brownian particles a magnification, in a visible scale, of the behavior of the unobservable and hypothetical) molecules. In early 1908, Perrin conducted his famous *height distribution experiments* which established that the height distribution of Brownian particles was indeed exponential.[[49]](#footnote-44) Further, Perrin claimed that he could explain this exponential height distribution in a way that allowed the determination of the osmotic pressure (*k*) of a single Brownian particle. Perrin devised his height distribution equation, which allowed the calculation of *k,* if one could determine: the mass of a Brownian particle in a homogeneous emulsion (*m*), the density of the Brownian particle (*p*), and the ratio of the concentration of Brownian particles at two different levels of the emulsion $(\frac{n\_{0}}{n})$.

$2,3log\frac{n\_{0}}{n}= \frac{1}{k}mgh(1-\frac{1}{p})$ (Logarithm to base 10)[[50]](#endnote-6)

Perrin attempted the experimental determination of the osmotic pressure of a Brownian particle, in early 1908 and published his results in the *Comptes Rendus de l’Académie des Sciences*. His aim was to prove ‘that molecular agitation is an actual cause, and cause unique, of Brownian movement.’[[51]](#footnote-45) Leaving aside the ingenious ways that Perrin invented to circumvent all the difficulties that surrounded the experimental calculation of the magnitudes appearing in the height distribution equation and, making a long story short, Perrin found the osmotic pressure (*k*) exerted by a Brownian particle to be equal to $360.10^{-16}$.[[52]](#footnote-46) Perrin compared the osmotic pressure exerted by $n$ Brownian particles with the pressure that, according to the kinetic theory, was exerted by $n$molecules of a gas. This pressure would be equal to $n \frac{RT}{N}$ (with $R$ being the constant of perfect gases, $T $the absolute temperature, and $N$ the number of molecules contained in one gram-molecule - which theoretical considerations from the viscosity of gases placed it around 7.1023). After making all the calculations Perrin found that the pressure exerted by$ n$ molecules of a gas was equal to *n*$ × $343.10-16; almost equal to the osmotic pressure of n Brownian particles, assuming the validity of the value for *N.*[[53]](#footnote-47) The conclusion of Perrin’s first experimental paper from 1908, reads:

*The mean kinetic energy of a colloid granule is therefore equal to that of a molecule*…At the same time, the kinetic theory of fluids seems a little more fortified, and the molecules a little more tangible. Their number *N* in a gram-mole, deduced from the previous equality, assumed to be correct, is 6,7.1023.[[54]](#footnote-48)

1. **The Structure and Epistemic Import of Perrin’s Height Distribution Experiments**

In section 2, I showed how different philosophers, starting from different pre-conceived philosophical positions, have offered different interpretations of Perrin’s experimental work. In sections 3 to 5, by following the historicist approach, I placed Perrin’s experiments on the height distribution of Brownian particles in their temporal dimension. We are now able to tackle the important question: What was the structure and epistemic import of Perrin’s height distribution experiments? The historicist approach shows that Perrin’s height distribution experiments:

1. Were part of a case of multiple determination (or triangulation), and not simply the confirmation of a theoretical prediction made by the kinetic theory of gases. Perrin’s experimental work was a continuation of his early methodological views. The height distribution experiments offered an independent, experimental (or inductive) determination of *k* and, subsequently,of values for other molecular magnitudes which were first determined in a deductive manner in the kinetic theory of gases. The precise quantitative agreement between the two determinations established that the molecules, hypothesized in the kinetic theory of gases, had a real existence and that they were identical with the ‘granules’ that, based on inductive reasoning from empirical observations, ought to be the cause of the phenomenon of Brownian movement.[[55]](#footnote-49)
2. The two determinations of *k* were theoretically independent. They were based on different reasoning processes (inductive *vs.* deductive), on the consideration of different phenomena (Brownian movement *vs*. viscosity of gases) and, most importantly, on theoretically independent auxiliary assumptions.[[56]](#endnote-7)
3. The numerical agreement achieved was very striking, especially if one considered the possible numerical values for *k* that could be the result of the height distribution experiments. According to Perrin, the range of possible values extended from zero to infinity.[[57]](#footnote-50)
4. Perrin used the agreement between the numerical value for *k* obtained in his height distribution experiments with the value for *k* theoretically inferred in the kinetic theory of gases to argue, not only about the validity of the result, but – perhaps even more importantly – about the validity of the central theoretical and experimental auxiliary assumptions upon which his height distribution experiments were based.[[58]](#footnote-51) The theoretical auxiliary assumptions included: the theorem of the equipartition of energy (which was central for the claim that Brownian particles behaved just like the molecules postulated by the kinetic theory of gases), the claim that the laws of perfect gases extended to uniform emulsions (with the particles of a uniform dilute emulsion behaving like the molecules of a gas or liquid in equilibrium), the extension of Stokes’ law to the order of magnitude of Brownian movement, and the claim that molecular movement was the (unique) cause of Brownian movement. The numerical concordance was also used to argue about the validity of the experimental methods employed to measure the magnitudes that appeared in the height distribution equation. These included the methods used: to prepare a uniform emulsion with spherical granules of equal diameter, to calculate the mass of the granules, and to determine the ratio $\frac{n\_{0}}{n}$. Perrin’s underlying reasoning was that it would be a remarkable coincidence for independent determination procedures to arrive at almost identical numerical values for the values of the molecular magnitudes measured, and yet for the (theoretical and experimental) auxiliary assumptions underlying them to be essentially flawed.[[59]](#endnote-8) This form of reasoning is crucial. Because of the large number and the precarious nature of the auxiliary assumptions required to determine the magnitudes appearing in the height distribution equation, no theoretical or experimental determination, *by itself*, could ever be sufficient to establish both the validity of the result and the validity of the determination procedure. Only the strong no-coincidence argument that emerged when independently theoretically-dependent procedures converged on the same value for *k*, could be used to argue both for the validity of the result and the validity of the determination procedures.
5. Perrin thought that one important feature of the height distribution experiments, which distinguished it from other efforts of determining the molecular magnitudes, was that they allowed an unlimited precision in determining the values for $k$ and$ N$.[[60]](#footnote-52) As Perrin would often repeat in his writings, providing a precise value for $N$ from the height distribution experiments was simply a question of conducting very careful experiments and making precise calculations of the magnitudes appearing in the height distribution equation.[[61]](#footnote-53)
6. **The Epistemic Import of the Independent Determinations of Avogadro’s Number**

To recapitulate, after conducting the height distribution experiments, Perrin claimed that the numerical concordance he had achieved: (a) established without a doubt the kinetic-molecular explanation of Brownian movement, (b) justified his theoretical and experimental approach, and (c) provided a first determination of the various molecular magnitudes. It is only after these initial experiments that providing a precise value for Avogadro’s number (*N*) became central to Perrin’s experimental work.[[62]](#footnote-54) Avogadro’s number, because of its direct connection with other molecular magnitudes, could serve as a sort of common ground for coordinating between the determinations of the various molecular magnitudes coming from the consideration of different phenomena.[[63]](#footnote-55) At the time when Perrin concluded his height distribution experiments (end of 1908), besides the value for Avogadro’s numbercalculated from the theoretical considerations made in the context of the kinetic theory of gases, there emerged four other determinations*.* All of them (largely) agreed with Perrin’s. Calculations of the charge of the electron (*e*), which were conducted at the Cavendish laboratory, placed *N* between 43.1022 and 96.1022. Max Planck’s and H.A. Lorentz’s calculations, which were based on the theory of black-body radiation, gave for *N* the values 61.1022 and 77.1022, respectively. Finally, Rutherford’s calculations of *e*, which were based on the study of radioactivity, placed *N* between 62.1022 and 77.1022. To these values, Perrin added his own determination by ‘a method which seems to me *direct and susceptible to an unlimited precision*.’[[64]](#footnote-56) By October 1908, Perrin had conducted three series of experiments with Brownian particles of different sizes. They involved calculations for 13000 particles and 16000 readings. Despite the variation of the different parameters, all the experiment series gave – within the limit of experimental error – the same invariant value for *N*: 70,5.1022.

In section 2, I showed how the various philosophers disagree not only regarding the structure of Perrin’s argument for molecular reality, but also regarding the role that the independent determinations of *N* played in it. Again, the historicist approach allows us to tackle this issue conclusively. To recapitulate, the question we are faced with is: what the importance of these additional concordant determinations of N for Perrin?- especially given that (as showed in section 6) he had already claimed that his experimental work had already established both the kinetic-molecular explanation of Brownian movement and the value of *N*. By reading Perrin’s writing from this period, we can infer several reasons why he thought these additional concordant determinations were important.

1. *Lack of Discordance*. Given that Perrin had already established the validity of his experimental approach and was certain that it could be used to provide precise values for $N$, the fact that it was not contradicted by the other determinations was, we could say, a huge relief. A discordant result could have raised doubts about the molecular theory of Brownian movement and required further experimental investigation to determine the source of disagreement. In fact, this is exactly what happened when the first experimental efforts to verify Einstein’s mathematical theory on the molecular origin of Brownian movement led to results that were discordant with Perrin’s (see section 8 below). The lack of discordance was extremely striking if one considered the *a priori* possibilities for the values for Avogadro’s number that were possible in each one of the different determinations.
2. *The Variety of the Phenomena Considered*. The different determinations were based on the theoretical consideration or experimental investigation of different phenomena: viscosity of gases, Brownian movement, black body radiation, radioactivity, and the electric charge of ions.[[65]](#footnote-57) This variety of phenomena, not only provided the required theoretical independence of the different determinations, but was to be expected (or even required) given that one was trying to determine the value of a fundamental magnitude concerning the ‘building blocks’ of observable phenomena.
3. *The Genetic Independence of the Determinations*. Besides being theoretically independent, the different determinations of *N* were also genetically independent.[[66]](#endnote-9) What is meant by genetic independence is simply the fact that the different determinations were conducted independently of one another.Although Perrin did not use the term, he was fully aware of the possible objection that the achieved agreement could be construed as a case of experimental calibration or mutual adjustment of the experimental results. Perrin explicitly stated how lucky he was that most of the experimental determinations of *N* were conducted concurrently with his, without the different researchers having knowledge of each other’s results. This precluded the possibility that they had (deliberately or even subconsciously) calibrated their results in order to achieve agreement.[[67]](#footnote-58)
4. *The Offering of Mutual Support.* The theoretical independence of the determinations, the genetic independence of determinations, the number of determinations, the quantitative nature of the agreement, the variety of phenomena considered, the lack of discordant results, were elements which were used to construe a very strong no-coincidence argument to support the validity of the auxiliary assumptions underlying the different determinations; especially the determinations which were based on the investigation of new phenomena (like black-body radiation and radioactivity) and which were thought to be based on more speculative and untested auxiliary assumptions. The rationale of this no-coincidence argument was that it would be an improbable coincidence for the various determinations to arrive at the same value for Avogadro’s number, and yet for the auxiliary assumptions underlying them to be essentially flawed.
5. **The Emergence of Discordance: Mathematical Theories of Brownian Movement**

Perrin’s experimental work on Brownian movement did not stop there. Perrin was able to provide two additional, concordant values for *N* *via* his experimental study of the mean horizontal displacement and mean rotation of Brownian particles, respectively. What did Perrin think was the importance of these additional determinations of *N*? Continuing with the historicist approach and looking at the events from a temporal perspective – as opposed to looking only at the final form of Perrin’s argument – shows that Perrin’s aim in conducting this additional experimental research was not to offer another determination of *N*, but to use the concordance in order to remove the doubts regarding the molecular-kinetic explanation of Brownian movement that had emerged when the first efforts to experimentally verify Albert Einstein’s mathematical work on Brownian motion failed to do so.

In 1905, without even knowing that the phenomenon of Brownian movement had been already observed and studied for around eighty years, Einstein produced a mathematical formula which described the average horizontal displacement that, according to the kinetic theory of heat, the (hypothetical) molecular movement ought to be causing on microscopic particles suspended in a liquid:

 $λ\_{x}= \sqrt{t }\sqrt{\frac{RT}{N}\frac{1}{3πkP}}$

Where, $λ\_{x} $is the mean horizontal displacement of a suspended (Brownian) particle, *t* is the time interval during which the displacement is measured, *T* is the absolute temperature, *N* is Avogadro’s number, *k* is the coefficient of viscosity of the liquid, and *P* the radius of the particle.[[68]](#footnote-59)

The important thing about Einstein’s formula was that it defined the horizontal displacement of a suspended particle without involving its real velocity, which could not be calculated because of the extremely complicated path the particle ought to be describing during a specific time interval. This opened the way for an experimental confirmation, given that all the magnitudes could (theoretically, at least) be experimentally determined. After presenting this equation, Einstein concluded his 1905 paper by hoping “that some enquirer may succeed shortly in solving the problem suggested here.”[[69]](#footnote-60) The experimental testing of the displacement formula, however, was not a straightforward issue. Assuming that the formula was exact, and that it was relatively unproblematic to measure the radius of the suspended particles and the viscosity of the intergranular liquid, one still had to assume a value for $N$ in order to provide a prediction for the mean displacement. That is, the precision of the prediction depended heavily on the exactness of the numerical value attributed to $N$. On the other hand, one could attempt to experimentally measure the radius, the viscosity, and the mean displacement, and thus provide a numerical value for $N$. In this case, however, besides the veracity of the equation, one had to assume the validity of the procedures employed to measure these magnitudes, especially the procedures employed to measure the mean displacement.

The difficulties surrounding the experimental testing of Einstein’s formula became apparent from the very first attempts.The publication of Einstein’s theoretical work on Brownian motion was followed by three independent verification attempts. They were by The Svedberg in Sweden, Max Seddig in Germany, and Victor Henri in France. They all, and independently of one another, failed to verify the formula. Svedberg argued that his experimental results offered a rough verification of the formula, but his claims were rejected by his contemporaries, including Perrin and Einstein.[[70]](#footnote-61) Seddig accepted the failure, but blamed his experimental method.[[71]](#footnote-62) The verification failure that came from the quantitative, cinematographic study of Brownian movement, undertaken by Victor Henri at the *College de Fran*ce, in the beginning of 1908, was the failure that had the most impact on Perrin and on the community of French physicists at the time. Contrary to Svedberg and Seddig, Henri concluded that Einstein’s displacement formula did not apply to the Brownian movement of the particles he had experimentally studied.[[72]](#footnote-63) Henri’s results were interpreted as a failure to establish the kinetic-molecular movement as the unique cause of the phenomenon of Brownian movement. They were consistent with the position, still defended by many French physicists at the time, that the electric actions exerted from the ions of the liquid on electrically charged suspended particles were an additional cause of the phenomenon.[[73]](#footnote-64)

Perrin believed that the height distribution experiments had established beyond any reasonable doubt the kinetic-molecular explanation of Brownian movement. The failure to experimentally verify Einstein’s displacement formula put in front of him the choice between the inexactness of molecular explanation and the inexactness of the formula. Perrin chose the latter option, believing that some unjustified assumption had entered in Einstein’s reasoning. Nevertheless, after suggestions made by Aime Cotton and Paul Langevin, he attempted a verification of the displacement formula by using Brownian particles of exactly known radius which he had used in his height distribution experiments.[[74]](#footnote-65)

Perrin conducted the first measurements with the help of his doctoral students.[[75]](#footnote-66) Surprisingly, the initial displacement measurements offered a satisfactory agreement with the value for *N* calculated in the height distribution measurements of the same particles. In 1909, Perrin announced a mean value for *N*, calculated by around 3000 displacement recordings, equal to 70,5x1022. This value was identical with the value for *N* determined in the height distribution experiments and remained relatively invariant to changes of the various experimental parameters. Perrin used the numerical agreement to support the validity of both the experimental procedures employed to determine the magnitudes appearing in Einstein’s formula, and the theoretical assumptions underlying Einstein’s mathematical derivation.[[76]](#footnote-67)

After verifying Einstein’s displacement formula, Perrin saw the possibility of an experimental test of Einstein’s equation for the rotational Brownian movement. Einstein had theoretically demonstrated that the molecular impacts, besides a translational movement, imparted on the suspended microscopic particles also a rotational movement. At the basis of Einstein’s equation of mean rotation was the equipartition of energy theorem, which claimed that, at the same temperature, the mean kinetic energy of rotation of a suspended Brownian particle was equal to its mean kinetic energy of translation, and both equal to the mean kinetic energy of an isolated molecule (and all this independently of the size of the granule).Perrin’s stated aim behind this experimental effort was not another confirmation of the molecular theory of Brownian movement, or another determination of *N*, but the confirmation of the theoretical assumptions underlying Einstein’s rotation equation;[[77]](#footnote-68) in particular, the invariance of the equipartition of energy theorem to changes of the various parameters (especially to changes on the size of Brownian particles).[[78]](#footnote-69)

1. **Conclusion: Towards a Two-Way, Mutually Beneficial, Integration of History and Philosophy of Science**

In this chapter, I argued for the necessity of the historicist-hermeneutic approach for achieving a mutually beneficial integration of History of Science and Philosophy of Science. Aspects of the historicist-hermeneutic approach have been supported by various scholars during the last fifty years. I demonstrated how this approach can be applied concretely to solve one of the most problematic case-studies in philosophy of science: the reasoning underlying Jean Perrin’s argument for molecular reality. I argued that Perrin’s was a case of multiple determination. Perrin put forward a no-coincidence argument for the existence of molecules, which was based on the agreement between multiple, independent determinations of Avogadro’s number (and consequently, other molecular magnitudes). The blunt rationale of the argument was the following: it would be a highly improbable coincidence for multiple, independent determinations of molecular magnitudes to achieve concordant results, and yet for there not to be any molecules. The careful application of the historicist-hermeneutic approach, however, shows that there were additional structural elements of Perrin’s argument that were responsible for its exceptional strength and, ultimately, for its success. They were the following:

1. The argument was based on a *quantitative* multiple determination. That is, the independent determinations concerned specific numerical values of the molecular magnitudes.
2. There was a close agreement between the independent determinations. This agreement became even more striking if one considered the possible values for the molecular magnitudes that could have been the result of each one of the determinations.
3. There was a (relatively) large number of determinations that converged on the same result.
4. The different determinations were theoretically independent; that is, they were based on independent theoretical assumptions.
5. The different determinations were genetically independent, and no effort was made to mutually adjust the numerical values calculated by theoretically independent procedures.
6. The different determinations were based on the investigation of unrelated phenomena.
7. The high quality and reliability of some of the determinations.
8. There was not even one discordant result, despite the large number of determinations.
9. When objections and discordant results which challenged Perrin’s determination of molecular magnitudes emerged, Perrin conclusively resolved the discordance.

Following the historicist-hermeneutic approach, it is possible to develop a conceptual framework for dealing with the structure and epistemic importance of the multiple determination strategy in scientific practice. The historicist-hermeneutic approach, as employed in Perrin’s case, shows the existence of several structural elements upon which the strength of the no-coincidence argument – the defining feature of the multiple determination strategy - depends. These elements are:

1. The number of determinations: the more the determinations that produce the same result, the stronger the no-coincidence argument.
2. The theoretical independence of the determinations: the more theoretically independent the determination procedures that establish the same result are, the stronger the no-coincidence argument.
3. The genetic independence of the determinations: the more genetically independent the determinations procedures that establish the same result are, the stronger the no-coincidence argument.
4. The reliability of the determinations: the more reliable the determination procedures that establish the same result are, the stronger the no-coincidence argument.
5. The quality (or clarity) of the result established by independent determinations; the clearer or more precise is the result upon which the independent determinations agree, the stronger the no-coincidence argument.
6. The quality of the convergence: the more the determination procedures are judged to have established the same result, the stronger the no-coincidence argument.
7. The complexity of the independently established result: the more complex is the result that is established by independent determinations, the stronger the no-coincidence argument.
8. The existence of discordant results and/or conflict with accepted knowledge: the less the discordant results and/or the conflict with accepted knowledge, the stronger the no-coincidence argument.

Continuing with the historicist-hermeneutic approach, we can use this preliminary conceptual framework to understand and evaluate the epistemic force of other cases of multiple determination, from past or current science. For example, we can use it to understand why in some cases of multiple determination the no-coincidence argument succeeds, whereas in other cases fails. The implementation of this step will demonstrate the relevance of Philosophy of Science to History of Science. This step is different from the traditional use of philosophical pre-conceptions to interpret the historical material. And this because from the interaction of the initial conceptual framework with the historical material it is possible to further sharpen and elucidate our initial framework. This could be done, for example, by noticing other structural elements that influence the strength of the no-coincidence argument underlying the multiple determination strategy. The implementation of this step will demonstrate the relevance of History of Science to Philosophy of Science. We can use this more developed framework to elucidate and evaluate other (or even the same) cases of multiple determination. And so on, and so forth. Our efforts to understand science in its historical dimension are themselves open-ended.

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1. Jutta Schickore, ‘More Thoughts on HPS: Another 20 Years Later’, *Perspectives on Science,* 19 (2011), pp. 453-81, offers a historical account of these efforts. [↑](#footnote-ref-1)
2. Stephen G. Brush, ‘A History of Random Processes: I. From Brown to Perrin’, *Archive for History of Exact Sciences,* 5.1 (1968), 1-36; Mary J. Nye, *Molecular Reality: A Perspective on the Scientific Work of Jean Perrin* (New York: American Elsevier Company, 1972); Alan Chalmers, *The Scientist’s Atom and the Philosopher’s Stone: How Science Succeeded and Philosophy Failed to Gain Knowledge of Atoms*, *Boston Studies in the Philosophy of Science 279* (Springer, 2009). [↑](#footnote-ref-2)
3. Rocke; Nye; and Chalmers, ‘The Scientist’s Atom’, offer comprehensible and accessible historical accounts of the nineteenth century atomic debates. [↑](#endnote-ref-1)
4. Jean Perrin, ‘Mouvement Brownien et Réalité Moléculaire’, Annales de Chimie et de Physique, 18 (1909), pp. 1-114; Jean Perrin, Les Atomes (Paris: Libraire Félix Alcan, 1913). [↑](#footnote-ref-3)
5. This very same strategy is referred to by different terms in the literature. Some of these terms are: ‘robustness’, ‘experimental robustness’, ‘independent confirmation’, and ‘triangulation’. I use the term ‘multiple determination’ because it is more transparent and less technical than the other terms. In addition, it is the one closest to the wording of the scientific practitioners themselves. We should always keep in mind, however, that the term ‘multiple determination’ is a tool used to help the analysis of the structure and epistemic import of a particular epistemic strategy. The latter is not a matter of terminology. [↑](#endnote-ref-2)
6. William Wimsatt, ‘Robustness, Reliability, and Overdetermination’, in *Scientific Inquiry and the Social Sciences*, ed. by M.B. Brewer and B.E. Collins (San Francisco: Jossey-Bass, 1981), pp. 124-63; Ian Hacking, ‘Do We See Through a Microscope?’, *Pacific Philosophical Quarterly*, 63 (1981), pp. 305-22; Allan Franklin, *The Neglect of Experiment* (Cambridge University Press, 1986); Nancy Cartwright, ‘Replicability, Reproducibility and Robustness: Comments on Harry Collins’, *History of Political Economy*, 23 (1991), pp. 143-55; Sylvia Culp, ‘Defending Robustness: The Bacterial Mesosome as a Test Case’, *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association,* 1 (1994), pp. 46-57; Alan Chalmers, ‘The Theory-Dependence of the Use of Instruments in Science’, *Philosophy of Science*, 70.3 (2003), pp. 493-509; Marcel Weber, *Philosophy of Experimental Biology* (Cambridge: Cambridge University Press, 2005). [↑](#footnote-ref-4)
7. The structure and epistemic import of Perrin’s argument has not been a topic of interest to historians of science, who are mostly limited to providing descriptive accounts of Perrin’s experimental work. [↑](#endnote-ref-3)
8. Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983), pp. 186-209; Peter Kosso, ‘Dimensions of Observability’, *The British Journal for the Philosophy of Science*, 39.4 (1988), pp. 449-67; Sylvia Culp, ‘Objectivity in Experimental Inquiry: Breaking Data-Technique Circles’, *Philosophy of Science*, 62.3 (1995), pp. 438-58; James Woodward, ‘Some Varieties of Robustness’, *Journal of Economic Methodology*, 13.2 (2006), pp. 219-40; Jacob Stegenga, ‘Robustness, Discordance, and Relevance’, *Philosophy of Science*, 76.5 (2009), pp. 650-61. [↑](#footnote-ref-5)
9. Gilbert Harman, ‘The Inference to the Best Explanation’, *The Philosophical Review*, 74.1 (1965), 88-95 (p. 89). [↑](#footnote-ref-6)
10. Clark Glymour, ‘Relevant Evidence’, *The Journal of Philosophy*, 72.14 (1975), 403-26 (p. 403). [↑](#footnote-ref-7)
11. Wesley Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton University Press, 1984), pp. 213-26. [↑](#footnote-ref-8)
12. Nancy Cartwright, *How the Laws of Physics Lie* (Oxford University Press, 1983), p. 83. [↑](#footnote-ref-9)
13. Deborah Mayo, ‘Cartwright, Causality, and Coincidence’, PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, 1 (1986), 42-58; Deborah Mayo, *Error and the Growth of Experimental Knowledge* (Chicago: University of Chicago Press, 1996), pp. 214-50. [↑](#footnote-ref-10)
14. Peter Achinstein, *The Book of Evidence* (Oxford University Press, 2001), pp. 243-65. [↑](#footnote-ref-11)
15. Stathis Psillos, ‘Making Contact with Molecules: On Achinstein and Perrin’, in *Philosophy of Science Matters: The Philosophy of Peter Achinstein*, ed. by Gregory J. Morgan (Oxford University Press, 2011), pp. 177-90; Stathis Psillos, ‘Moving Molecules Above the Scientific Horizon: On Perrin’s Case for Realism’*, Journal for General Philosophy of Science*, 42 (2011), pp. 339-63; Stathis Psillos, ‘The View from Within and the View from Above: Looking at Van Fraassen’s Perrin’, in *Bas van Fraassen’s Approach to Models and Representation in Science*, ed. by Wenceslao J. Gonzalez (Springer, 2014), pp. 143-68. [↑](#footnote-ref-12)
16. Bas van Fraassen, ‘The Perils of Perrin, in the Hands of Philosophers,’ *Philosophical Studies*, 143 (2009), pp. 5-24. [↑](#footnote-ref-13)
17. Robert Hudson, *Seeing Things: The Philosophy of Reliable Observation* (Oxford University Press, 2013), pp. 103-38. [↑](#footnote-ref-14)
18. Ernan McMullin, ‘The History and Philosophy of Science: A Taxonomy’, in *Minnesota Studies in the Philosophy of Science*, ed. by Roger H. Stuewer (Minneapolis: University of Minnesota Press, 1970), pp. 12-67, p. 18. [↑](#footnote-ref-15)
19. Schickore, 2011, p. 456. [↑](#footnote-ref-16)
20. Larry Laudan et al., ‘Scientific Change: Philosophical Models and Historical Research’, *Synthese*, 79.2 (1986), pp. 141-223. [↑](#footnote-ref-17)
21. Imre Lakatos, ‘History of Science and its Rational Reconstructions’, *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, (1970), pp. 91-136. [↑](#footnote-ref-18)
22. Thomas Nickles, ‘Remarks on the Use of History as Evidence’, *Synthese*, 69.2 (1986), pp. 253-66; Nickles, ‘Philosophy of Science and History of Science’; Hans Radder, ‘Philosophy and History of Science: Beyond the Kuhnian Paradigm’, *Studies in History and Philosophy of Science*, 28.4 (1997), pp. 633-55; and Schickore, provide accessible and detailed accounts of these problems. [↑](#footnote-ref-19)
23. Schickore introduces the term and also provides an account of this approach. [↑](#footnote-ref-20)
24. Ernan McMullin, ‘History and Philosophy of Science: A Marriage of Convenience?’, *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, (1974), 585-561; Dudley Shapere, ‘What Can the Theory of Knowledge Learn from the History of Knowledge?’, *The Monist,* 60.1 (1977), 488-508; Richard M. Burian, ‘More than A Marriage of Convenience: On the Inextricability of History and Philosophy of Science’, *Philosophy of Science*, 44.1 (1977), 1-42; Richard M. Burian, ‘The Dilemma of Case Studies Resolved: The Virtues of Using Case Studies in the History and Philosophy of Science’, *Perspectives on Science*, 9.4 (2002), 383-404; Thomas Nickles, ‘Philosophy of Science and History of Science’, *Osiris*, 10 (1995), 138-63; Jutta Schickore, ‘More Thoughts on HPS: Another 20 Years Later’, *Perspectives on Science,* 19 (2011), pp. 453-81; Hasok Chang, ‘Beyond Case Studies: History as Philosophy’, in *Integrating History and Philosophy of Science*, ed. by Seymour Mauskopf and Tad Schmaltz (Springer 2012), pp. 109-24. [↑](#footnote-ref-21)
25. Shapere, ‘What Can the Theory of Knowledge Learn from the History of Knowledge?’, p. 496. [↑](#footnote-ref-22)
26. Dudley Shapere, *Reason and the Search for Knowled*ge, *Boston Studies in the Philosophy of Science*, 78, (Dordrecht: D. Riedel Publishing Company, 1984), p. 185. [↑](#footnote-ref-23)
27. Schickore, pp. 471-74. [↑](#footnote-ref-24)
28. Psillos is a notable exception. [↑](#endnote-ref-4)
29. For more biographical information on Perrin see: Louis de Broglie, *La Réalité des Molécules et L’Œuvre de Jean Perrin* (Paris: Gauthier-Villars, 1945); and Nye. [↑](#footnote-ref-25)
30. Jean Perrin, *Rayons Cathodiques et Rayons de Röntgen*: *Étude Expérimentale* (Paris: Gauthier-Villars et fils, 1897). [↑](#footnote-ref-26)
31. Jean Perrin, *Traité de Chimie Physique: Les Principes* (Paris: Gauthier-Villars, 1903) [↑](#footnote-ref-27)
32. Perrin, *Traité de Chimie Physique*, p. vii. [↑](#footnote-ref-28)
33. Perrin, *Traité de Chimie Physique*, p. vii. All translations are by me. [↑](#footnote-ref-29)
34. Jean Perrin, ‘Les Hypothèses Moléculaires’, *Revue Scientifique*, 15.15 (1901), pp. 449-61. [↑](#footnote-ref-30)
35. Perrin, ‘Les Hypothèses Moléculaires’, pp. 451-55. [↑](#footnote-ref-31)
36. Perrin, ‘Les Hypothèses Moléculaires’, p. 449. [↑](#footnote-ref-32)
37. See also Perrin, ‘Rayons Cathodiques et Rayons de Röntgen’ [↑](#endnote-ref-5)
38. Perrin, ‘Les Hypothèses Moléculaires’, pp. 456-459. [↑](#footnote-ref-33)
39. Jean Perrin, *Notice sur les Travaux Scientifiques de M. Jean Perrin* (Toulouse: Édouard Privat, 1923), pp. 22-28. [↑](#footnote-ref-34)
40. Brush, pp. 7-14; Nye, Chapter 1; Roberto Maiocchi, ‘The Case of Brownian Motion’, The British Journal for the History of Science, 23.3 (1990), pp. 257-83, (pp. 257-263). [↑](#footnote-ref-35)
41. Léon Gouy, **‘**Le Mouvement Brownien et les Mouvements Moléculaires’, *Revue Générale des Sciences*, 6 (1895), pp. 1-7. [↑](#footnote-ref-36)
42. Jean Perrin, ‘Le Contenu Essentiel des Principes de la Thermodynamique’, *Bullétin de la Société de Philosophie*, 6 (1906), pp. 81-111; Jean Perrin, ‘La discontinuité de la matière’, *Revue de Mois*, 1 (1906), pp. 323-44. [↑](#footnote-ref-37)
43. Perrin, ‘La discontinuité de la matière’, p. 338. [↑](#footnote-ref-38)
44. Perrin, ‘La discontinuité de la matière’, pp. 335-36. [↑](#footnote-ref-39)
45. Perrin, ‘La discontinuité de la matière’, p. 338. [↑](#footnote-ref-40)
46. Jean Perrin, ‘Mécanisme de l’électrisation de contact et solutions colloïdales’, *Journal de Chimie Physique*, 3 (1905), pp. 50-110 (p. 58). [↑](#footnote-ref-41)
47. Nye, p. 101. [↑](#footnote-ref-42)
48. Jean Perrin, ‘Mécanisme de l’électrisation de contact et solutions colloïdales’, p. 60. [↑](#footnote-ref-43)
49. Jean Perrin, ‘L’agitation moléculaire et le mouvement brownien’, *Comptes Rendus,*147 (1908), pp. 967-970. [↑](#footnote-ref-44)
50. In his later writings, and depending on his argumentative goals, Perrin produced different versions of the height distribution equation. This, however, is the original version of the equation which clearly shows that Perrin’s initial goal was the quantitative multiple determination of the value for *k.* [↑](#endnote-ref-6)
51. Jean Perrin, ‘L’agitation moléculaire et le mouvement brownien’, p. 968. [↑](#footnote-ref-45)
52. Jean Perrin, ‘L’agitation moléculaire et le mouvement brownien’, p. 969. [↑](#footnote-ref-46)
53. Jean Perrin, ‘L’agitation moléculaire et le mouvement brownien’, p. 969. [↑](#footnote-ref-47)
54. Jean Perrin, ‘L’agitation moléculaire et le mouvement brownien’, p. 970. Italics in the original. [↑](#footnote-ref-48)
55. Perrin, ‘Peut-on peser un atome avec précision ?’, *La Revue du Mois*, 6 (1908), pp. 513-538, (pp. 514-515). [↑](#footnote-ref-49)
56. See also Chalmers 2011, pp. 10-13. [↑](#endnote-ref-7)
57. Jean Perrin, ‘L’origine du mouvement brownien’, *Comptes Rendus*, 147 (1908), pp. 530-532, (p. 531). [↑](#footnote-ref-50)
58. Perrin, ‘L’origine du mouvement brownien’, p. 531; Perrin, ‘Peut-on peser un atome avec précision?’, p. 528. [↑](#footnote-ref-51)
59. Viewed in this manner, Perrin experimental work seems to provide a direct response to Pierre Duhem’s famous critique of experimental method from the 1890s. This way of reasoning is not necessary troublesome or vicious. Since the emergence of the problem of the experimenters’ regress – which can be interpreted as a more general version of the *Duhem thesis* - philosophers have been aware of the interdependency existing between the correctness of an experimental result and the reliability of the experimental procedure used to establish that result: a correct experimental result is generally considered to be one produced with a reliable experimental procedure; but a reliable experimental procedure is the one that produces the correct result (Collins 1984). In other words, we don’t know if we have obtained the correct result unless we have used a reliable procedure, but we don’t know if we have used a reliable procedure unless we have obtained the correct result.Insofar as the no coincidence argument from the multiple determination strategy helps to break the regress by arguing about the validity of the result, it can also be used to argue about the rough reliability of the procedures. [↑](#endnote-ref-8)
60. Perrin, ‘Peut-on peser un atome avec précision ?’, pp. 529-32. [↑](#footnote-ref-52)
61. Perrin, ‘Peut-on peser un atome avec précision ?’ ; Jean Perrin, ‘Mouvement Brownien et Molécules’, Journal de Physique Théorique et Appliquée, 9 (1909), pp. 5-39 ; Perrin, Jean, *Les Atomes* (Paris: Libraire Félix Alcan, 1913) [↑](#footnote-ref-53)
62. Perrin ‘L’origine du mouvement brownien’, p. 532. [↑](#footnote-ref-54)
63. Perrin, Jean, ‘Grandeur des molécules et charge de l’électron’, Comptes Rendus, 147 (1908), pp. 594-96 (p. 594). [↑](#footnote-ref-55)
64. Perrin, ‘Grandeur des molécules et charge de l’électron’, p. 595. Italics in the original. [↑](#footnote-ref-56)
65. Perrin, Jean, ‘Mouvement Brownien et Réalité Moléculaire’, Annales de Chimie et de Physique,
18 (1909), pp. 1-114, (pp. 93-113). [↑](#footnote-ref-57)
66. See Soler for a discussion. [↑](#endnote-ref-9)
67. Perrin, Jean, ‘Mouvement Brownien et Réalité Moléculaire’, p. 108. [↑](#footnote-ref-58)
68. Albert Einstein, ‘On the Movement of Small Particles Suspended in a Stationary Liquid Demanded by the Molecular-Kinetic Theory of Heat’, in Investigations on the Theory of theBrownian Movement by Albert Einstein, ed. by R. Fürth, transl. by A. D. Cowper, (Dover Publications, 1956) 1-18, p. 18. [↑](#footnote-ref-59)
69. Einstein, ‘On the Movement of Small Particles’, p. 18. [↑](#footnote-ref-60)
70. Perrin, ‘Mouvement Brownien et Réalité Moléculaire’, p. 74; Albert Einstein, ‘Theoretical Observation on the Brownian Motion’, in Investigations on theTheory of the Brownian Motion, pp. 63-67. [↑](#footnote-ref-61)
71. Nye, p. 125. [↑](#footnote-ref-62)
72. Victor Henri, ‘Étude cinématographique des mouvement brownien’, Comptes Rendus, 146 (1908), 1024-26, (p.1026). [↑](#footnote-ref-63)
73. Aimé Cotton, ‘Recherches récentes sur les mouvements browniens’, La Revue du Mois, 5 (1908), pp. 737-41, (p. 739) ; Jacques Duxlaux, ‘Pression osmotique et mouvement brownien’, Comptes Rendus, 147 (1908), pp.131-34. [↑](#footnote-ref-64)
74. Perrin ‘Mouvement Brownien et Molécules’, p. 32. [↑](#footnote-ref-65)
75. Chaudesaigues ‘Le mouvement brownien et le formule d’Einstein’, Comptes Rendus, 147 (1908), 1044-1046 ; Jean Perrin and Dabrowski, ‘Mouvement brownien et constants moléculaires’, Comptes Rendus, 149 (1909), 477-79. [↑](#footnote-ref-66)
76. Chaudesaigues, p.1045; Perrin, ‘Mouvement Brownien et Molécules’, p. 33. [↑](#footnote-ref-67)
77. Jean Perrin, ‘Le Mouvement Brownien de Rotation’, Comptes Rendus, 149 (1909), pp. 549-51, (p. 550). [↑](#footnote-ref-68)
78. Perrin, ‘Mouvement Brownien et Réalité Moléculaire’, p. 92. [↑](#footnote-ref-69)