Cartwright, Causality, and Coincidence[1]

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1. Introduction

In How the Laws of Physics Lie (1983)[2] Cartwright argues for being a realist about theoretical entities but non-realist about theoretical laws. Her reason for this distinction is that only the former involves causal explanation, and accepting causal explanations commits us to the existence of the causal entity invoked. "What is special about explanation by theoretical entity is that it is causal explanation, and existence is an internal characteristic of causal claims. There is nothing similar for theoretical laws." (p. 93). For, according to Cartwright, the acceptability of a theoretical explanation is a matter of its ability to satisfy such criteria as organizing and simplifying, and in her view, "success at organizing, predicting, and classifying is never an argument for truth." (p. 91). In contrast, Cartwright claims, "When I infer from an effect to a cause, I am asking what made the effect occur, what brought it about. No explanation of that sort explains at all unless it does present a cause; and in accepting such an explanation, I am accepting not only that it explains in the sense of organizing and making plain, but also that it presents me with a cause." (p. 91). She considers explaining the yellowing of leaves on her lemon tree by the accumulation of water. "There must be such water for the explanation to be correct." (p. 91). The same, she claims, is true when the causal entity is a theoretical one.

This logical point is uncontroversial: If it is correct to accept that entity X caused observed effect Y then X must exist. But this logical point hardly settles the matter. It only shifts the problem to questions about when a causal explanation is correct. Cartwright may be right to suggest that such questions "do not bear on what kind of inferences you can make once you have accepted that explanation." (p. 93). But one could equally define a good theoretical explanation so that accepting it entailed the truth of the theory. Cartwright goes beyond this logical point, and argues that methods are available for warranting causal explanations that are lacking for theoretical ones.

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According to Cartwright, theoretical explanations can be justified only by a version of Gilbert Harman's (1965) inference to the best explanation. Such inferences are said to be warranted by an argument from coincidence. It is argued that "It would be an absurd coincidence if a wide variety of different kinds of phenomena were all explained by a particular law, and yet were not in reality consequent from the law." (p. 75). The main objection to such arguments is the redundancy objection: alternatives that explain the phenomena equally well cannot be ruled out. For Cartwright, "We can infer the truth of an explanation only if there are no alternatives[3] that account in an equally satisfactory way for the phenomena. In physics nowadays, I shall argue, an acceptable causal story is supposed to satisfy this requirement. But exactly the opposite is the case with the specific equations and models that make up our theoretical explanations." (p. 76). Aligning herself with Duhem and van Fraassen on this point, Cartwright rejects the inference to the best theoretical explanation. If there is always more than one equally acceptable but incompatible way of explaining a phenomena, acceptability as an explanation does not warrant acceptance as true.

But Cartwright maintains that "arguments against inference to the best explanation do not work against the explanations that theoretical entities provide." (p. 89). "[U]nlike theoretical accounts, which can be justified only by an inference to the best explanation, causal accounts have an independent test of their truth; we can perform controlled experiments to find out if our causal stories are right or wrong." (p. 82). Indeed such experiments, Cartwright claims, play a major role in the very example cited by Harman, Salmon and others as "a paradigm of inference to the best explanation" (p. 83) -- namely Jean Perrin's arguments for "the existence of atoms and for the truth of Avogadro's hypothesis that there are a fixed number [N] of molecules in any gram mole of a fluid." (pp. 82-83). On her view "Perrin does not make an inference to the best explanation, where explanation includes anything from theoretical laws to a detailed description of how the explanandum was brought about. He makes rather a more restricted inference -- an inference to the most probable cause." (p. 83, emphasis added). Nor, according to Cartwright, is the molecule or other theoretical entity inferred as the cause the theoretical entity of any particular theory.[4] What we are allowed to infer about molecules or other theoretical entities, Cartwright asserts, are "highly specific claims about just what behavior they lead to in just this situation." (p. 92).

Focusing on the Perrin example, I shall sketch the argument which Cartwright claims leads to his inferring the most probable cause. I shall argue that either the inference she describes fails to be a genuinely causal one, or else it too is open to the redundancy objection (raised against theoretical explanation). However, I think there is a way to sustain Cartwright's main insight: that in certain cases of causal inference from experiments (e.g., Perrin) the redundancy objection may be avoided. But, contrary to Cartwright, I argue that in those cases one is able to infer causes only by inferring some theoretical laws about how they produce experimental effects.

2. Rejecting Coincidence vs. A Causal Argument from Coincidence

Brownian motion (discovered by the botonist Brown in 1827) refers to the irregular motion of small suspended particles in various fluids, observed through high-powered microscopes, which keeps the particles from sinking due to gravitation. Perrin sought to determine if the cause of Brownian motion is molecular collisions according to the kinetic theory of gases. Doing so was regarded as a test of the kinetic theory against the classical theory of thermodynamics. If Brownian motion could be explained as caused by something either outside the liquid medium or within the particles themselves, then it would not be in conflict with the classical theory. If, alternatively, the cause of Brownian motion was shown to be a molecular motion in the liquid medium, as given in the kinetic theory, it would be in conflict.[5]

The evidence typically alluded to in viewing the Perrin case as illustrative of an inference to the best explanation is the fact that experiments on numerous distinct phenomena (e.g., gases, Brownian motion, blue of the sky, etc.) yielded estimates for Avogadro's number, N, of a similar order of magnitude (around 70×10^{22}).[6] We have a theoretical prediction from the kinetic theory for the value of N, and diverse experiments show agreement with that value. Cartwright also alludes to this agreement of the estimates of N from a variety of phenomena: "The convergence of results provides reason for thinking that the various models used in Perrin's diverse calculations were each good enough. ... that those models can legitimately be used to infer the nature of the cause from the character of the effects." (p. 85). But rather than taking such agreement to rule out other theoretical explanations (which, according to the redundancy argument, it cannot), Cartwright suggests Perrin can take it as ruling out *experimental artefacts* by means of an "argument from coincidence". Her argument is interesting:

> *If we are mistaken about the processes that link cause and effect in our experiment, what we observe may not result in the way we think from the cause under study. Our results may be a mere artefact of the experiment, and our conclusions will be worthless.* ... Frequently we are not sure enough; we want further assurance that we are observing *genuine results and not experimental artefacts*. This is the case with Perrin. He lacks confidence in some of the models on which his calculations are based. *But he can appeal to coincidence. Would it not be a coincidence if each of the observations was an artefact and yet all agreed so closely about Avogadro's number*? (p. 84, emphasis added).

The basic outline of Cartwright's argument is this:

(1) "If we are mistaken about the processes that link cause and effect in our experiment" then "[o]ur results may be a mere artefact of the experiment... ."

(2) By an argument from coincidence, the convergence of estimates of N lets us rule out the experimental artefact explanation (negating the consequent of (1)).

So Cartwright must be saying this: While it is not an absurd coincidence to suppose this convergence of estimates of Avogadro's number N comes about without also supposing a specific theory H about molecular motion is true, it would be an absurd coincidence to suppose this convergence comes about and yet the estimates of N merely artefacts of the experiments. So, while not ruling out the alternative theoretical explanations to H, Cartwright is saying, the convergence does warrant ruling out (as an absurd coincidence) the experimental artefact explanation. Thus, we can negate the antecedent of (1), and conclude we are not "mistaken about the processes that link cause and effect in our experiment". And if we can infer we are correct about the process causing the effect, on Cartwright's view, we can infer the causal entity is real. So in this way the pieces of Cartwright's interesting argument fit together. But is the argument sound? I claim it is not.

The problem, as I see it, is that merely ruling out the experimental artefact explanation does not let us pinpoint a genuine causal explanation. The convergence of estimates does allow one to argue that it is unlikely that all the agreement is a mere coincidence, and that the effect (the convergence) is of a systematic or non-chance variety. But this allows only one type of "artefact" to be ruled out -- that of chance or experimental error. That is, it permits inferring to the "non-chance" hypothesis NC:

> NC: The convergence of estimates is a non-chance result of experimental artefacts.

But it does not indicate it is due to any specific cause, such as random collisions of molecules with the Brownian particles, which is what Perrin needs to infer. There are many other explanations still not ruled out, (e.g., temperature differences, convection currents, and others; see Brush 1968). Nor did Perrin take the convergence over the 13 phenomena to show more than the existence of a "real connection" between the phenomena.[7] Similar estimates of molecular magnitudes were available long before Perrin's work. Far from being taken to show Brownian motion is caused by random molecular collisions, some took them to show that attractive and repulsive forces between molecules, not mere random motion, must be responsible.[8]

It might be countered, however, that by inferring to a cause Cartwright only means that we infer to specific causal behavior of some entities or other; namely, that the cause (whatever it is) must be that which reliably yields such and such experimental estimates. And it is enough for this sort of inference to be able to infer the existence of a real (non-chance) effect. Granted, if the only claims under consideration are the chance or experimental artefact explanation, and the non-chance one (NC), then it is true that in ruling out the former one can sustain an argument from coincidence to the second (avoiding the redundancy objection). But this is not to make a genuine causal inference to a specific entity or process. It may allow inferring the existence of a correlation between a certain type of experiment and a range of results.[9] This would really only affirm the existence of real (non-artefactual) effects, and the only properties of the "causal entities" that would be inferred are in terms of the properties regularly exhibited in experiments -- their performance characteristics.[10] One could say, for example, that Perrin inferred that something reliably manifests itself in the form of a given range of estimates of N. We know there is a cause only in the sense that we know we have real experimental effects -- not artefacts. But this is not sufficient for Perrin's inference to a process of molecular motion as the cause of the reliable experimental effects.

For an analogy[11] consider a photograph that reveals a halo of light around the head of a photographed subject. The persistence of this halo, despite varying cameras, labs, and deliberately monitoring against fraud, might allow ruling out its being caused by experimental artefacts, e.g., pin-holes in the film box (which would have to repeatedly occur in precisely the same place), vaseline on the lens, etc. But the fact that this "halo effect" is real, I am claiming, does not select from among possible causal explanations that might be considered, e.g., a specified ESP ability of the subject, the reflections of a suitably positioned mirror, light from a stained glass window, etc.

3. Summary of the Problem and Proposed Criteria for its Avoidance

My argument so far is this. If the only artefact that could be ruled out was that of a chance coincidence or the experimental artefact explanation, then the first premise of Cartwright's argument would be false. It is very possible to be mistaken about the processes that link cause and effect and yet have our results (e.g., convergence of estimates of N) not be merely a chance coincidence or due to experimental artefacts. In other words, the truth of premise (1) of the above argument depends on how "artefact of the experiment" is understood. Typically, in experimental design, effects are due to experimental artefacts when they are due to extraneous factors introduced by the experimental apparatus, e.g., a faulty microscope, a pin-hole in the film box. For (1) to be true, however, "experimental artefacts" would have to include all factors other than the causal factor to be inferred, which we can call the primary factor. But if we suppose, in order to render premise (1) true, that "artefacts of the experiment" means "all non-primary factors", then premise (2) would be false. The reason is this: The convergence of estimates Cartwright cites does sustain an argument from coincidence to a hypothesis. But the hypothesis says only that the effect is "real" in the sense of not due to chance or experimental artefacts. Non-redundancy is satisfied because the only alternative here would be that the convergence was due to chance. But it is false that this argument from coincidence "allows us to rule out the experimental artefact explanation" in the sense of ruling out all non-primary factors that might be responsible. So premise (2) is false; we are not able to negate the consequent of (1) as claimed. Yet, if Cartwright intends her analysis to sustain inferences to a causal hypothesis such as Perrin's inference to a statistical distribution of molecular collisions, or, as she sometimes suggests, to the claim that "Avogadro is right" (p. 84), then it appears Cartwright is describing an inference from converging results to the truth of some theoretical claims after all. And such an inference, by her own insistence, is open to the redundancy objection.

So it appears that Cartwright is caught on the horns of the following dilemma: either the only inference being made is to "non-chance" and is not genuinely causal, or it is intended to be genuinely causal (denying alternative non-primary factors) but then it is open to the redundancy objection. I think Cartwright can escape both horns, however. Although (contrary to Cartwright's claim) the causal inference she wants does seem to require inferring to some theoretical claims, such an inference, when it is warranted, is not based on an appeal to mere convergence of experimental results (e.g., estimates of N across phenomena). We can say that such convergence would be highly likely if in fact a causal hypothesis H were true. That is:

(i) Prob $(e|H)$ is high.

All such assertions are (after-trial) measures of explanatory goodness, admittedly open to the redundancy objection. The additional requirement needed is that it is practically impossible (or an absurd coincidence) for the effects e to have been caused by the factors except the (primary) one hypothesized in H. I suggest construing this second requirement as

(ii) Prob $(e|\text{not } H)$ is very low

over alternatives to H[12]. Satisfying (i) and (ii) sufficiently, I suggest, satisfies non-redundancy, and at the same time non-primary causal factors may be ruled out. I shall sketch one illustration from the reasoning Perrin uses to learn the cause of Brownian motion.

4. Perrin's Statistical Argument from Coincidence

If Brownian motions were not due to any source external to the liquid medium, their motions would be expected to be unsystematic and entirely irregular. Even without being able to list all possible external factors, it can be argued that were Brownian motion the effect of such a factor, then neighboring particles would be expected to move in approximately the same direction; the movement of a particle's neighbors would not be independent of its own. Thus, the object of Perrin's inquiry was a matter of determining whether the movement of Brownian particles exemplifies a specific type of random phenomenon that was well known from simple games of chance.

Using a microscope it is possible to observe the total distance traveled by a Brownian particle in any direction as it weaves its zig-zag path from some starting point: its displacement. The displacement of Brownian particles (along the x-axis) after t minutes was the magnitude chosen by Einstein and Smoluchowski as characteristic of the agitation; for in the mean, this line will be longer the more active the agitation. Although the irregularity of its motion precludes predicting what each particle's displacement will be, it is possible to generalize about the pattern of irregularity it follows by means of its statistical distribution law. This distribution specifies how frequently Brownian particles would be expected to be displaced along the x-axis by certain amounts over a given time. If molecular agitation (as described by the kinetic theory of gases) causes Brownian movement then, as Einstein showed, the displacement of a Brownian particle is Gaussian distributed about its mean (which by symmetry is 0) with variance equal to 2 Dt (where D is the coefficient of diffusion and t is the time).[13] Einstein (1905) states, "The probable distribution of the resulting displacements in a given time t is therefore the same as that of fortuitous error, which was to be expected." (p. 16 emphasis added).

Since Avogadro's number N is a function of the diffusion coefficient D, the prediction of the kinetic theory (for a given type of particle) can be stated as a predicted standard deviation. Once D is obtained, Avogadro's number N can be calculated; so the formal aspect of the causal inquiry is to estimate the parameter D and then test how well it agrees with the value hypothesized by the kinetic theory. However, the sample data can be used to estimate or check the parameter value D only if it can be seen (i.e., modelled) as the results of observing (500) displacements from the hypothesized Gaussian process M. So, evaluating the causal hypothesis "The displacement distribution (in Brownian motion experiments) is caused by molecular collisions according to the kinetic theory" largely involves evaluating the statistical hypothesis:

> H: The experimental displacement distribution is from a population distributed according to Gaussian distribution M with parameter value a function of N^*,

for N^* the (probable) value for N hypothesized by the kinetic theory. Perrin's inquiry into H consists of two broad steps: (I) checking whether the results of the experiment actually performed follow the given statistical distribution M and (II) using estimates of D to estimate (or test) values of N. The question: is Perrin's model adequate for the causal inference? corresponds to accomplishing step (I). Formally, it involves using experimental data to "test" the claim:

> (j): The data approximates a (random) sample from the (hypothesized) Gaussian process M.

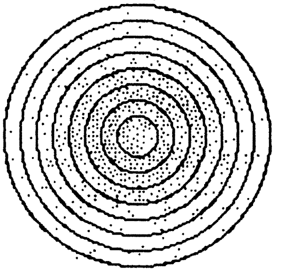
Equivalently, statistical hypothesis j asserts the denial of j':

> (j'): The assumptions of the experimental model M are violated; the sample displacements are characteristic of systematic, non-fortuitous effects.

Being able to rule out j' indicates that violations are sufficiently negligible for the purpose of estimating the parameter D, (and so j passes the test).

Regardless of the statistical method chosen the aim is the same. What is wanted is a question that can be put to the data such that if the model is inadequate (for the experimental analysis) then one set of "answers" is expected with a high probability; while if it is adequate, another set is. A "good" statistical test from the standard (Neyman-Pearson) theory provides such a question. It provides a test that rarely rejects j erroneously (i.e., low size), but has a good chance of correctly detecting departures from j, and so often rejects j correctly (i.e., high power). Note that these critieria of low size and high power are just those needed to satisfy criteria (i) and (ii) for arguing from coincidence to j. One example must suffice:

The data in the experiment I shall consider are the measured displacements of 500 gamboge grains (with approximately the same radius[14]). Their positions are measured every 30 seconds on paper with grids of squares, and then shifted to a common origin. "The extremities of the vectors obtained this way should distribute themselves about that origin as the shots fired at a target distribute themselves about the bull's-eye." (Perrin 1913, p. 118).



Observed Distribution of Displacements of 500 Gamboge Grains

"Here again we have a quantitative check upon the theory; the laws of chance enable us to calculate how many points should occur in each successive ring." (Perrin 1913, p. 118). The number observed in each ring is close to the hypothesized number. That is, the observed displacements are not statistically significantly different from what is typical under j. This indicates that j holds, but only because in addition there are grounds for claiming that were the model inadequate (i.e., were j' to hold), then we would almost always get differences that were statistically significant from what is typical under j.

The displacement of a particle after time t may be seen as the result of k steps, where at each step the particle has an equal chance of being displaced by a given amount in either a positive or negative direction. (This is called a simple random walk. See Note 13.) Were it incorrect to assume j, there would not be an equal chance of being displaced by a given amount in either direction for each particle; there would be some dependencies. Were we observing such dependencies we would easily (frequently) generate statistically significant differences using the various analyses Perrin applied to the 500 recorded displacements. So we can argue: were j', and not j, the case, it is extremely improbable that none or even very few of the experiments would have indicated this (by means of differences statistically significant from what is expected under j.) On such grounds, j' was ruled out by Perrin and others.[15] "In short, the irregular nature of the movement is quantitatively rigorous. Incidentally we have in this one of the most striking applications of the laws of chance." (Perrin 1913, p. 119, emphasis added).

Checking assumptions in step (I) is a question of the experiment's internal validity. Contrary to what Cartwright had suggested, it was not answered by finding converging estimates -- even over a variety of experimental phenomena. Indeed, a number of experiments yielded estimates for N that were close to the predicted value, and yet were shown by Perrin and others not to have been adequate.[16] (i.e., Despite agreement in estimating N from the model, assumptions of the model were seen to be violated.) Moreover, even within the single phenomenon of Brownian motion it is not mere agreement that warrants claiming one's model is adequate for causal inference (i.e., for ruling out j'). The concordance of results does indicate Perrin's model is satisfactory, but only because they result from experiments deliberately designed so that if one misses a bias (and erroneously gives a good fit with M) another is likely to find it. This is the additional requirement that I claim sustains an argument from coincidence, avoiding the redundancy objection. Here the bias one needs to rule out is that some regularity or coordination of molecular motion has been concealed. Such a regularity was the main alternative to Perrin's causal hypothesis H. By finding that the Brownian motion is totally irregular (as described in M), such coordinated motion is ruled out.

Having checked that the data satisfy the assumptions of the experimental model (i.e., having ruled out j' in step (I)) Perrin remarks, "To verify Einstein's diffusion equation, it only remains to see whether the number [obtained for N by substituting the estimate of D into the equation $N = RT/D(1/6\ \Pi ac)$] is near 70×10^{22}." (Perrin 1913, p. 132).[17] Nearly all of the estimates for N obtained from several Brownian motion experiments were insignificantly different from the number predicted by the kinetic theory, N^*. "It cannot be supposed that, out of the enormous number of values a priori possible, values so near to the predicted number have been obtained by chance for every emulsion and under the most varied experimental conditions." (Perrin 1913, p. 105, emphasis added).

But this sounds like an argument from coincidence to the non-chance hypothesis that I earlier argued cannot sustain Perrin's causal inference to H. The difference, I claim, is that having accomplished step (I) it is now possible to say the cause of this reliable effect is random molecular collisions as described in model M. Having checked that assumptions of experimental distribution M hold to a given approximation (i.e., affirming j), we know how to design experiments that make it very difficult to generate results close to N^* unless we really are sampling from a population where N is approximately N^*. Given j, the falsity of hypothesis H indicates the existence of a real discord between the hypothesized value, N^*, and the "true" value for N (where the "true" value is the mean value for N in a population of experiments[18]). If there is such a discord (the extent of which can be made precise) then our experiment has given it a good chance (often) to manifest itself in terms of a discrepancy between the hypothesized and estimated N. That is, the experimental analysis had a high chance of detecting a genuine discordancy (of a specifiable amount) if one existed, i.e., it had high power. But our experimental result e did not indicate such a discordance; instead we find we can generate at will (frequently) estimates of N near the hypothesized value N^*. Therefore,

$$\text{Prob}(e \mid \text{not } H) = \text{very low}$$

and the criterion (ii) for arguing from coincidence to H is met. From the statistical hypothesis H, it may be possible to infer substantively to "the nature of the cause".

5. What is Learned About the Cause of Experimental Effects

Estimates of the diffusion coefficient D indicate the approximate rate at which a particle is moving, from which we ascertain the average number of collisions to which these Brownian particles must be subject to have caused such diffusion. This indicates approximately how many molecules per unit area there must be, i.e., N. In showing Brownian motion is a Gaussian process, Perrin showed the existence of a process due to a random agitation not attributable to the particles, or to external energy sources. Such spontaneous agitation requires that the second law of classical thermodynamics be seen as only statistically valid. In fact, Perrin describes his experiments as means for generating such violations of the second law. In Perrin (1906, p. 68) for example, (cited in Brush 1968, p. 377) he states: "[W]e are going to show that sufficiently careful observation reveals that at every instant, in a mass of fluid, there is an irregular spontaneous agitation which cannot be reconciled with Carnot's principle except just on the condition of admitting that his principle has the probabilitistic character[19] suggested to us by molecular hypotheses." (emphasis added).

Here Perrin gives an argument from coincidence for inferring the statistical nature of the second law of thermodynamics. He can rule out the alternative non-statistical theory by affirming the (approximate) truth of the statistical one given in Einstein's theory of Brownian motion. In saying it is approximately true, we are acknowledging that it includes approximations of certain aspects of molecules, e.g., the theory holds only if time t is not taken as too small.[20] We do not infer all of the properties of molecules given in the kinetic theory of gases; nor need we infer the truth of the full kinetic theory to infer the cause of Brownian motion is random molecular collisions. Cartwright is correct about this. However, on my account, in contrast to Cartwright's, this causal inference requires knowing at least some of the properties of the entities from substantive theories. For Perrin, it requires inferring the (approximate) truth of such theoretical distribution laws as those given in Einstein's theory.

But in suggesting we infer the truth or the near truth of such theoretical distribution laws, am I not simply inferring to the best theoretical explanation, opening myself to the redundancy objection? I claim the answer is no. For, if my characterization (in Section 3) of an argument from coincidence that avoids redundancy is correct, the experimental reasoning to the statistical distribution law is not open to this objection. Arguments fail to avoid the redundancy objection when they show only that hypothesis H renders the convergent results e highly probable[21] (i.e., my criterion (i) is satisfied), but are unable to show that e would be very improbable unless H were (approximately) true (i.e., my criterion (ii) is not satisfied). Perrin's inference to the statistical distribution law satisfies both criteria.

My argument, if correct, justifies Cartwright's central point: in experiments like Perrin's, reasoning from effects to causes avoids the redundancy objection. Statistics allows us to learn about the probabilistic relationship between characteristics of experimental results (i.e., statistics) and parameters of the population from which the results may be seen as random samples. Step I teaches Perrin that the experimental results on Brownian motion approximate a random sample from a specified Gaussian distribution M. Step II indicates the values of parameters of this distribution law governing the motion. Cartwright's two premises become:

(1) If we are incorrect about H then either the experimental assumptions are not sufficiently met (i.e., the experiments do not approximate a random sample from Gaussian distribution M) or else N is far from the hypothesized value N*.

(2) Arguments from coincidence (satisfying criteria (i) and (ii)) enable us to reject the consequent of (1).

This allows us to accept H, that the statistical distribution of molecular motion given in M correctly describes the causal process responsible for Brownian motion.[22] Accepting the causal explanation in the Perrin case, then, requires accepting at least the approximate correctness of a specific statistical distribution of molecular motion. But Cartwright, in her discussion of models, denies that any of the statistical distributions used to describe molecular motion can be true. She deems the distribution functions of statistical mechanics, fictional, not real. "What is the distribution function for the molecules in this room? ... They [these questions] are queer because they are questions with no answers. They ask about properties that only <u>objects</u> <u>in</u> <u>models</u> have, and not real objects in real places. ... The distribution functions play primarily an organizing role. They cannot be seen; they cause nothing;... we have no idea how to apply them outside the controlled conditions of the laboratory... ." (p. 156, emphasis added).[23]

But no one would claim that it is the distribution function that causes the effect. It is the molecules moving according to the distribution law that does the causing. Accepting Perrin's causal explanation involves accepting that molecules truly are in this sort of random motion, causing the particles to display that distribution as well. That is, the <u>statistical</u> <u>motion</u> <u>of</u> <u>the</u> <u>particles</u> (the effect) indicates the <u>statistical</u> <u>motion</u> <u>of</u> <u>the</u> <u>molecules</u> (as cause). As Perrin states: "The Brownian motion is a faithful reflection of [molecular movement], or, better, it is a molecular movement in itself, ... From the point of view of agitation, there is no distinction between nitrogen molecules and the visible molecules realized in the grains of an emulsion [with a given N]." (Perrin 1913, p. 105).

My argument shows Cartwright's fundamental intuition to be correct: in certain cases one can learn about the nature of the cause on the basis of its experimental effects. In one such case (e.g., Perrin), one learns about a statistical population distribution from which the experimental results may be seen as a random sample. Admittedly, my argument does not show that the truth of such theoretical claims cannot be construed instrumentally.[24] What it does show is that one does not infer the cause of Brownian motion without inferring such (theoretical) distribution laws of the molecular motion. But given Cartwright's attempt to drive a wedge between inferring entities and laws, and given her denial of the truth of such statistical laws, it is hard to see how she can suppose such a causal inference is made.

Notes

[1]I am greatly indebted to Ronald Giere and Alan Musgrave for many helpful discussions and comments on early drafts of this paper. I am also grateful to Larry Laudan for very valuable criticisms, and to Norman Gilinsky and Joseph Pitt for useful suggestions.

[2]All references to Cartwright in this paper will be from this book.

[3]Cartwright restricts "no alternatives" to those available (see for example p.76). The justification for considering only practically available alternatives is that the redundancy objection which Cartwright is concerned to avoid is the one raised by Duhem and van Fraassen, and the basis of their objection is not that in the future other alternatives might arise--nor even that there always exists others in the present. For this would also be true for the inductive methods used to infer empirical laws which they themselves countenance. As Cartwright remarks, "Duhem is not, for example, opposed to phenomenological laws, which arise by inductive generalization. It is a familiar fact that it is possible to construct different inductive rules which give rise to different generalizations from the same evidence. _Here too there will always be more than one incompatible law which appears equally true so far as we can tell_." (p. 90, emphasis added). For further discussion of Cartwright's reading of Duhem see pp. 87-90.

[4]She claims, for example, that "the electron [that is inferred in explaining the rate of fall of a light droplet] is not an entity of any particular theory. [e.g., Bohr electron, Rutherford electron, etc.] ...it is the electron, about which we have a large number of incomplete and sometimes conflicting theories." (p. 92).

[5]Moreover it would show a statistical process was responsible. See, for example, Einstein (1905, pp. 1-2).

[6]Typically the 13 phenomena in Perrin's (1913) _Atoms_ are cited. A good illustration is found in Salmon (1984). He argues, for example, that "the 'remarkable agreement' constitutes strong _evidence_ that these experiments are not fully independent--that they reveal the existence of such entities [as atoms, molecules, and ions]." (p. 220). But having strong evidence for the hypothesis "that these experiments are not fully independent", is not the same as having evidence for the hypothesis "that they reveal the existence of such entities" as Salmon suggests. Although Cartwright distinguishes her account of the Perrin case as an inference to the most probable cause from Salmon's account of it as an inference to the best explanation, it too, I shall argue,

appears open to this criticism.

[7]Perrin himself claimed:

> Yet however strongly we may feel impelled to accept the existence of molecules and atoms, we ought always to be able to express visible reality without appealing to elements that are still invisible. And indeed it is not very difficult to do so. We have but to eliminate the constant N between the 13 equations that have been used to determine it to obtain 12 equations in which only realities directly perceptible occur. These equations express fundamental connections between the phenomena, at first sight completely independent, of gaseous viscosity, the Brownian movement, the blueness of the sky, black body spectra, and radioactivity. (1913, p. 216).

The value he sees in this is its enabling observations on one sort of phenomena to "check an error in the observation" (p. 216) of some very different measurement.

[8]Brush (1968, pp. 357-358) cites similar arguments given by Ramsay (1882) and later by Gouy (1888). They argued that the existence of Brownian motion indicated the existence of <u>coordinated</u> molecular movements in liquids, denying they could be due to the random molecular motions that Perrin later showed to be the cause.

[9]One could infer the <u>experimenter</u> causes the experimental outcome to result but this is not to infer to the causal entity or process of interest.

[10]Cartwright suggests in some places (e.g., p. 98) that it is such an inference to mere "performance characteristics" that she has in mind in speaking of experimental inferences to causes.

[11]I owe the details of this analogy to Mark Mayo.

[12]With Cartwright, I restrict these to practically available alternatives, or those under consideration. For the justification of this see Note 3. Moreover, the probability in (ii) does not require the availability of an exhaustive list of alternative hypotheses about non-primary factors. As I mention in Section 4, the source I have in mind for these probabilities are the error probabilities provided by standard Neyman-Pearson statistical methods. For example, a Neyman-Pearson test with <u>high</u> <u>power</u> over specified alternatives to H provides a way of satisfying criterion (ii), since the probability in (ii) is one minus the power over the given alternative (or discrepancy from H). This is in contrast with the requirments for assigning after-trial measures of the probability or "support" evidence e affords hypothesis H, such as a Bayesian posterior probability assignment to H. For such a posterior requires not only a complete set of alternative hypotheses, but a prior probability assignment to each as well. (A discussion of how Neyman-Pearson methods may be used to satisfy (i) and (ii), and how I suggest they be interpreted in learning from experiments occurs in Mayo (1985).)

[13]The standard deviation (square root of the variance) is the displacement in the direction of the x-axis which a particle experiences on average (root mean square of displacement). The

importance of this statement of variance, for the experimental determination of D, is that it shows the mean square displacement of a Brownian particle to be proportional to the time t. This suggests, for example, that a model for Brownian motion is provided by viewing a particle as taking a simple random walk: it has the same chance of being displaced a given amount X in either a positive or negative direction. Thus, after k steps, on average, the displacement would be 0. Occasionally, more steps will be to one direction than to the other, yielding a non-zero total displacement. That the variance is proportional to the time (the number of steps) corresponds to the fact that the more steps taken, the larger the possible value of this non-zero total displacement. Another model that was also used analogizes the distribution of the total displacement with the winnings of a gambler who stands to win or lose a fixed amount X with equal probability on each trial. As with the displacement, the largest possible value won or lost is proportional to the number of trials played. Also see the "bull's eye" model in this Section. Clear derivations of this statistical distribution occur in Chandrasekhar (1943), Einstein (1908), and Parzen (1960, pp. 374-376).

[14]Checking this assumption required a separate, but similar, battery of statistical analyses. For a discussion of Perrin's methods for ensuring approximate uniformity of gamboge grains, see Perrin (1913, pp. 94-99).

[15]See for example Chandrasekhar (1943, pp. 27-28).

[16]An example of such an error is acknowledged in Perrin (1923, p. 124).

[17]c is the viscosity of the fluid, T, its absolute temperature, R, the gas constant, a, the radius of the particles. This substitution also required a separate check of the applicability of Stokes formula. See for example Perrin (1913, p. 129).

[18]That is, N is itself a parameter (a mean) of a population distribution from which observed estimates of N are random samples. Though actual experiments do not give us the precise value of this parameter, they do enable us to learn if estimates are discrepant from the true value by certain amounts. They do this by ruling out those discrepancies which, with overwhelming probability, could not have produced observed estimates.

[19]Its being "probabilistic" has an entirely frequentist interpretation: Perrin means that it is violated (i.e., there are decreases in entropy) very rarely. To illustrate, Perrin calculates "the time we would have to wait before we had an even chance of seeing the brick [weighing a kilogramme suspended in the air by a rope] rise to a second level by virtue of its Brownian movement." (Perrin 1913, p. 87). It is considerably more than 10^{10} billion years.

[20]As Chandrasekhar (1943, p. 89) notes: "In Einstein's and Smoluchowski's treatment of the problem, Brownian motion is idealized as a problem in random flights; but as we have seen, this idealization is valid only when we ignore effects which occur in time intervals of [specifiably small order]."

[21]Equivalently, when they show only that e renders H highly likely, well-confirmed, or that other "explanationist" measures hold.

[22]It might be objected at this point, that I have not ruled out all possible alternatives, and hence have not satisfied the redundancy requirement after all. But as I explain in Notes 3 and 12, the redundancy objection to which Cartwright is responding justifies my trying to show only that the inference to the statistical law of molecular motion in the case of Brownian motion is on par with causal or other empirical inferences that are countenanced. And no such inference rules out all possible hypotheses that might ever be conceived. However, I would even go further. Whatever else is learned about molecules, Brownian motion, etc., I think the statistical distribution of molecular motion given in model M will still be a correct account of the causal process responsible for Brownian motion. The possible alternatives are complete with respect to the parameter space of that statistical model, and the approximate values for a parameter like the diffusion coefficient D (or Avogadro's, N) that have been affirmed, will, with very high probability, not be found to deviate far from future estimates. (Present estimates of N are, explicably, somewhat lower than in Perrin's day.)

[23]One reason Cartwright reaches this conclusion is that several different probability distributions may be employed for different purposes in statistical mechanics, suggesting that there is no correct one (e.g., p. 154). This multiplicity of distributions is not a problem for my account. For all of them are similar on the aspects inferred by means of the argument from coincidence that I recommend, and these are the ones needed for the causal inference.

[24]The point of this qualification is not that I consider there to be no argument that shows this, but rather that the one given here would have to be supplemented to provide such an argument.

References

Brown, R. (1828). "A Brief Account of Microscopical Observations Made in the Months of June, July and August, 1827, on the Particles Contained in the Pollen of Plants; and on the General Existence of Active Molecules in Organic Bodies." Philosophical Magazine 4: 161-173.

Brush, S. (1968). "A History of Random Processes. I. Brownian Movement from Brown to Perrin." Archive for the History of the Exact Sciences 5: 1-36. (As reprinted in Studies in the History of Statistics and Probability. Volume II. Edited by Sir M. Kendall and R.L. Plackett. New York: MacMillan, 1977. Pages 347-382.)

Cartwright, N. (1983). How the Laws of Physics Lie. Oxford: Clarendon Press.

Chandrasekhar, S. (1943). "Stochastic Problems in Physics and Astronomy." Reviews of Modern Physics 15: 1-89. (As reprinted in Wax (1954). Pages 3-91.)

Duhem, P. (1908). Σώζειν τα φαινόμενα: Essai sur la notion de théorie physique de Platon a Galilée. Paris: A. Hermann. (As reprinted as To Save the Phenomena: An Essay on the Idea of Physical Theory from Plato to Galileo. (trans.) E. Doland and C. Maschler. Chicago: University of Chicago Press, 1969.)

Einstein, A. (1905). "Die von der molekularkinetischen Theorie der Wärme geforderte Bewegung von in ruhenden Flüssigkeiten suspendierten Teilchen." Annalen der Physik, ser. 4, 17: 549-560. (As reprinted as "On the Movement of Small Particles Suspended in a Stationary Liquid Demanded by the Molecular-Kinetic Theory of Heat." In Einstein (1926). Pages 1-18.)

-----------. (1908). "Elementare Theorie der Brownschen Bewegung." Zeitschrift für Elektrochemie 14: 235-239. (As reprinted as "Elementary Theory of Brownian Motion." In Einstein (1926). Pages 68-85.)

-----------. (1926). Investigations on the Theory of the Brownian Movement. (ed.) R. Fürth, (trans.) A.D. Cowper. London: Methuen and Company. (As reprinted New York: Dover Publications, 1956.)

Gardner, M. (1979). "Realism and Instrumentalism in 19th Century Atomism." Philosophy of Science 46: 1-34.

Gouy, L. (1888). "Note sur le mouvement brownien." Journal de Physique 7: 561-564.

Hacking, I. (1983). Representing and Intervening. Cambridge: Cambridge University Press.

Harman, G.H. (1965). "Inference to the Best Explanation." The Philosophical Review 74: 88-95.

Mayo, D. (1985). "Behavioristic, Evidentialist, and Learning Models of Statistical Testing." Philosophy of Science 52: 493-516.

Nye, M.J. (1972). Molecular Reality. London: Macdonald and Company.

Parzen, E. (1960). Modern Probability Theory and Its Applications. New York: John Wiley.

Perrin, J. (1906). "Le contenu essential des principes de la thermodynamique." Bull. Soc. Philosophie 6: 81-111. (As reprinted in Perrin (1950). Pages 57-80.)

---------. (1913). Les Atomes. Paris: Alcan. (As reprinted as Atoms. (trans.) D.L. Hammick. London: Constable and Company, 1923.)

---------. (1950). Oeuvres Scientifiques de Jean Perrin. Paris: Centre National de la Recherche Scientifique.

Ramsay, W. (1882). "On Brownian or Pedetic Motion." Proceedings of the Bristol Naturalists' Society 3: 299-302.

Salmon, W.C. (1984). Scientific Explanation and the Causal Structure of the World. Princeton: Princeton University Press.

Uhlenbeck, G.E. and Ornstein, L.S. (1930). "On the Theory of the Brownian Motion." Physical Review 36: 823-841. (As reprinted in Wax (1954). Pages 93-111.)

van Fraassen, B.C. (1980). The Scientific Image. Oxford: Clarendon Press.

Wax, N. (ed.). (1954). Selected Papers on Noise and Stochastic Processes. New York: Dover Publications.