A DIFFERENT CONCEPTION OF SCIENTIFIC REALISM: THE CASE OF THE MISSING *EXPLANANDUM*

by

J.K. Derden Jr.
Department of Philosophy
Humboldt State University
Arcata, CA 95521
email: jkd2@axe.humboldt.edu

ABSTRACT

Given the ideal conditions used in constructing empirical mathematical laws, one finds and should expect that the values deduced from such laws (C−values) will not agree with the measured values for the same variable (O−values). These conditions are constitutive of such laws, and the latter are acceptable only if their O and C−value differences can be explained in terms of the factors idealized. A conception of realism emerges where the laws are true but misrepresent reality, have false C−value predictions, are not accepted because of their truth or approximate truth, and are acceptable only if the differences are explained.

1. This paper is concerned with the mathematical laws/theories of empirical science. We begin with van Fraassen's portrayal of scientific realism: *"Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves belief that it is true."* (1980, 8) Of this portrayal, van Fraassen says that "it is quite minimal, and can be agreed to by anyone who considers himself a scientific realist." (1980, 8) I am one scientific realist who cannot agree with this portrayal. But before we proceed, we must settle an issue that might derail the project before it gets a fair hearing.

The realist position developed in this paper comes about through the recognition of the role of ideal conditions in scientific theorizing. This is best seen through an example in which I make a distinction between mathematical laws and their equations.

Thus, the ideal gas **equation** is: PV =RT.[1] But the ideal gas **law** is more than this equation. Many **idealizing assumptions known to be false** are used in constructing or deriving the ideal gas law (and hence its equation). Some of the assumptions for the molecules of a gas in a given container are: (G1) The molecules are perfectly spherical; (G2) The molecules are perfectly elastic in their collisions with each other and the sides of the container; (G3) There are no attractive forces exerted by the molecules or by the container; (G4) The volumes of the molecules are "negligible". As a first approximation, the ideal gas **law** is the following: (1) If G1 & G2 & G3 & G4, then PV = RT. I say "approximation" because the (almost) final analysis of the ideal gas law is the following: (2) (u)(z)(x)(y)(w){{[u is a gas & z is a vessel & u is contained in z & u is composed of molecules all of which are perfectly elastic & u is composed of molecules all of which have negligible volumes & u is composed of molecules all of which are perfectly spherical & u is composed of molecules all of which exert no forces on one another or with the sides of the container except during collisions] & [x=the numerical value of the pressure of u & y=the numerical value of the volume of z & b=the numerical value of the universal gas constant & w=the numerical value of the absolute temperature of u]} −−−> (xy=bw)}.[2]

It should be clear from (2) that I take the idealizing conditions to be **constitutive** of the laws and theories for which they are invoked. Such conditions, when expressed in the form of singular statements, are known to be false. For example, the statement that the molecules in this container here and now are perfectly spherical is false. Also, the

1 Throughout I am taking the number of moles of the gas to be one.

2 With (2) the analysis is still not complete, since we have not introduced the units for pressure, temperature, volume, and the gas constant , and since there are additional ideal conditions other than the four above. See Sears (1956), 201−204, for additional assumptions.

idealizing predicates occurring in the antecedent of the generalized conditional are unsatisfied, e.g., ‘x is a perfectly spherical molecule’. Because of the material conditional, a generalized conditional with at least one idealizing predicate in its antecedent is true in elementary logic. Thus, mathematical laws of empirical science on this account are generalized conditionals **that are true and known to be so prior to any testing**.[3]

And so the potentially derailing issue here is whether it is appropriate to take the mathematical laws, as opposed to the equations, to be (vacuously) true. Some will prefer to pursue a relevance logic here, while others will want to pursue theories for counterfactual conditionals. I have chosen neither of the latter approaches, and instead have decided to take as the project one of determining when to accept and when to reject the vacuously true mathematical laws. I suspect that the task of finding criteria for accepting or rejecting the vacuously true laws is an easier one than that of building a general relevance logic or a general counterfactual theory because of the restriction in scope. I also suspect that any criteria I might offer in my project will very likely have to be respected by either of the other two attempts at theories. For example, in the absence of a counterfactual theory, I am betting that my acceptable vacuous mathematical laws will serve as a basis for intuitively acceptable counterfactuals.

Scientists reason from false assumptions in conjunction with asserted premises to derive the false equations. When they are done with their derivations, the conclusions that they validly draw are conditional in form. This is the way valid reasoning from

3 They also contain pure mathematical equations as their consequents. See Barr (1971) and Nowak (1972, 1980, 1992) for this kind of analysis, but note that neither of them has pure mathematical equations as the consequents of such conditionals. The significance of this point cannot be pursued here.

assumptions works. That the conditionals can be vacuously true should be no surprise. If so, the question becomes: When are the resulting vacuously true generalized conditionals acceptable?

3. Now assume that the above analysis for mathematical laws is correct. Then, contrary to many realists, one should not take the ideal gas law to be approximately true. Instead, it is true and known to be so before any testing takes place. Also contrary to van Fraassen one should not accept (2) above **because** it is true; nor does it make any sense to accept this **true** generalization **because** it is **approximately true**. But while it is also clear that one could say that the **equations** of empirical science are false, following Cartwright (1983), from my perspective it is better to say that the calculated values ("C−values") obtained through the **laws** do not agree with the measured values ("O−values") for the same variable.[4]

Here then is the scientific realist position I wish to develop: (1) The mathematical laws/theories of science are true and known to be so prior to testing them; (2) the equations of science are false and known to be so prior to testing them; (3) the mathematical laws are deliberately constructed so as to misrepresent reality, i.e., they are not attempts to provide a literally true story; (4) the laws/theories are true, but they are not accepted because they are true; (5) there is no role for approximate truth; (6) the aim of testing is not to falsify; it is to determine how large the expected differences are between the values of the predictions and the measurements for the same variable; (7) it is a major goal of science to justifiably explain these differences; (8) being able to

4 Cartwright, at least in her *How the Laws of Physics Lie*, does not take the laws to be in part constituted by the ideal conditions. From my perspective, one obtains her false "laws" by stripping out the idealizing predicates in cases like (2) above.

explain these differences is the basis for acceptance of laws/theories that generate them.

One may now understand how to take such laws to be true and yet maintain that they misrepresent reality. And here the misrepresentation is deliberate. The scientist is in the position of facing a messy reality and in order to have a computable function "ignores" certain factors by idealizing them, where these factors are relevant to the values of the variables of interest. In the ideal gas law, the initial variables of interest are P, V, and T. But the scientist knows that the actual shapes of the molecules, their imperfectly elastic collisions, their attractive forces, and their non−negligible volumes affect the values for P, V, and T. She knows that if one idealizes these factors away, that the C−values and the O−values will not agree. In fact, the **existence** of these factors explains why the C and O−values are not the same. In creating an imaginary world where the mathematics will "work", the scientist does not lose sight of what has been idealized**, since it is these idealized factors that are the first court of appeal in explaining the expected differences**.[5]

For example, suppose that we use O−value inputs for V and T of a gas and calculate the output C−value for P. Call the calculated value for P, Pc, and the measured value, Po. The scientist expects that the absolute value of the difference between Pc and Po will not be equal to 0: |Pc − Po| > 0. He is now asked to explain why this is the case. He says that as long as we ignore the actual volumes of the molecules that we can expect a difference. The existence of the actual volumes of the molecules **explains in part** why |Pc − Po| > 0.

And so it does not seem quite right to say that science aims for a literally true

5 They are not the only court of appeal, however.

story. In the face of too many factors, difficulty with units, etc., perhaps one should see the goal to be one of computable functions based on ideal conditions. But if this is so, then why should anyone, Cartwright included[6], want to ignore the ideal conditions and their role in theorizing? More importantly, how does one know when a true idealized law is acceptable? Instead of the latter question, however, the question standardly asked is when are the false equations acceptable.

4. The false equations are standardly taken to be acceptable only if the results are "close enough". Instead of "close enough" Giere uses the term 'similar' when he states that the real system must be "*similar* to the proposed model in specified *respects* and to specified *degrees*." (1985, 80) While Giere's talk of similarity seems to be a form of hand−waving (see below), even so, the question for him and everyone else is: When are the C and O−values close enough? Certain approximate truth approaches will try to specify in general when the $|O - C|$ interval is "small enough" so that one is justified in saying that the theory/law that entails the C−values in question is approximately true.[7] Of structuralist approaches to specifying such intervals Jeff Ramsey says that "when it comes to specifying what counts as an acceptable range of discrepancy, structuralists resort to hand−waving and vague generalities." (1992, 155) I would agree with Ramsey and go further and say that there is no manner of knowing independent of the theory/law at issue when an interval is close enough. There is, however, a general principle, not recognized by Ramsey or (as far as I know) any other philosopher, that does come into

6 In *Nature's Capacities and Their Measurement* Cartwright seems by implication to take the ideal conditions to be constitutive of the laws, since she here (1989, 204) proclaims Leszek Nowak's method of concretion to be the method of science. Nowak (1972, 1980, 1992) **does** take the ideal conditions to be constitutive of the laws.
7 See, for example, Niiniluoto (1984, 1987).

play in actual scientific practice in connection with the question.[8]

5. The principle is: **(I) For any O and C−value pair, an |O − C| difference is "close enough" iff one can provide a complete and epistemically justified explanation for why |O − C| > 0.** Having rejected truth *a la* van Fraassen or approximate truth as a reason for accepting an idealized law/theory, we now have through (I) above a reason for acceptance: (II) A true idealized law or theory is acceptable iff its |O − C| differences can be completely and justifiably explained. Now (II) may be too strong, i.e., we may only want to have a necessary condition for acceptance. But one thing that the history of science surely demonstrates is that if one cannot completely and justifiably explain the differences, then the law or theory is in trouble. And so philosophers of science, in the grip perhaps of the deductive nomological conception of explanation, have failed to recognize one of the most significant facts in need of explaining, namely, the differences between the O and C−values. Hence, the subtitle of this essay.

6. We now examine the type of explanation in question and one method whereby one can provide epistemic justification for claiming to have explained the differences in whole or in part. The ideal gas law falsely assumes that the values for the molecular volumes of a gas do not affect the values for V, P, or T. Assume that we have a C−value for pressure of Pc and an O−value of Po, where |Po − Pc| > 0. Why is there this difference between these two values? In effect, it was Clausius who said that it was because we have neglected, among other factors, the actual volumes of the molecules in

8 I here assert that error measurement theory alone will not provide one with the resources to completely explain the |O − C| differences. A number of philosophers hold this position, notably, Ramsey (1992), Nowak (1972, 1980, 1992), Cartwright (1983, 1989), and Laymon (1985, 1989); so I will not argue the point here

the container with our idealization about negligible volumes. Clausius reasoned that the volume occupied by a gas should not be taken to be the volume of the container V, but that it should be the volume available to a single molecule, given the volume taken up by all the molecules in the container. The volume available is the volume taken up by all the molecules, which is b, subtracted from V. The amount subtracted from V is a function of the **number** of molecules in the container and their **radii.** Clausius' **corrected** version of the ideal gas equation is: $P(V - b) = RT$. In correcting the original **equation** Clausius drops the gas **law's** false assumption that the molecules have negligible volumes. With the corrected equation we have a new C−value for pressure, where this new value has **moved closer** to the O−value, i.e., we have increased the accuracy of our predictions. This increase in accuracy provides an epistemic justification for our belief that the actual volumes of the molecules are factors affecting the values of our variables. We now claim to know that if we ignore the actual volumes and number of the molecules in the container, we will get deduced results that are different from our measured results. Put another way, we now are justified in claiming to know that the number and volumes of the molecules in the container are causal factors that make gases have the measured values that they do. We are justified in claiming that we have explained **in part** why we have a difference in C and O−values for the ideal gas law.

In the example of Clausius' equation, one does not have a **complete** explanation for the $|O - C|$'s for the original ideal gas law by appeal to the actual volume taken up by the molecules. But one knows that one has explained **in part** these differences, since one has made the C−values move closer to the O−values in correcting the original equation. This means that the $|O - C|$'s are still not close enough. Meanwhile, van der Waals drops the ideal condition that the molecules exert no attractive forces on each other and corrects Clausius' equation by factoring in a correction for pressure based in part on the number of molecules in the container. Once again, the C−values move closer to the O−values, but also once again the |O − C|'s have not shrunk to zero. And so, one has explained in part the differences, but the differences are still not close enough. The differences are not close enough until they have been completely and justifiably explained. Of course, one may eventually drop all recognized ideal conditions and correct for them so that the |O − C|'s **are explainable** by appeal to measurement or experimental error.[9]

Now briefly consider some additional cases of unexplained differences: Why were the O and C−values for the position of Uranus different? Neither the idealized factors for Newton's laws nor measurement error could account for the differences. It was the existence of Neptune that ultimately did the explaining. Or again, why did the O and C−values differ for the perihelion of Mercury? No factor was found to explain the difference, and so the true idealized Newtonian laws are in trouble.. But one can correct for Newton's constant mass m, i.e., instead of m, one writes $m/(1 - v^2/c^2)^{1/2}$ (thereby creating the Special Theory of Relativity). This gets our C's to move closer to our O's, e.g., in the Mercury case. In doing this, we recognize that the original uncorrected laws require an additional idealizing assumption, namely, that mass is a constant. Or consider the Davisson/Germer experiment to determine the wave length of a 54−volt electron, using a Nickel crystal and Bragg's Law to get one value, and the De Broglie equation to get the other. The difference was only 0.02×10^{-8} cms. between the two values! This was not "close enough" for them, and they **explained** the difference ultimately in terms

9 See Sears (1956), Chapter 11, for the gas law account.

of refractive indices for electrons, **correcting** Bragg's Law in the process.[10]

7. This method of dropping and correcting is Leszek Nowak's method of strict concretization. At least in his 1972 and 1980 works Nowak takes the idealized laws to be vacuously true, and such laws are acceptable if they are elements in a sequence of dropping and correcting with increasing accuracy. Leaving aside initial conditions and auxiliary assumptions Nowak's sequences look as follows, where Tn is a law (universal conditional), In an ideal condition, and Cn a C−value: (a) [(T1 & I1 & I2 & .....In) −−−> C1]; (b) [(T2 & I2 &.....In) −−−> C2]; (c) [(Tn & In) −−−> Cn]. Here Nowak is dropping an ideal condition and correcting the original "theory" with the hoped for new and more accurate C−value. The In are singular ideal condition statements as already described. The ideal conditions are also "built into" the Tn as unsatisfied predicates in the antecedents. For Nowak, if for each dropping and correcting episode, one gets increased accuracy, then the sequence and each Tn in the sequence is acceptable. But Nowak recognizes that some of the In will not be amenable to drops and corrections. At this point Nowak maintains that one must specify an interval for the |O − C|'s that is "close enough". Here he joins Ramsey's hand−wavers.

Nowak does not see his method of strict concretization as a method of epistemically justifying the explanation for why |O − C| > 0. He thus does not see that explaining the differences is a major goal of science. For me his method of strict concretization is but one method out of at least five for providing epistemic

10 See Stranathan (1942), 541−545. Note that this example is not really a case of an O and C−value difference, but rather it is a case of a difference between two incompatible C−values of the same variable. Here there is no independent O−value to get by measuring or observation. See the last paragraph of this paper.

justification.[11,12]

8. Here is a fictionalized schematic account of the elements of theorizing using the ideal gas law. Assume that Mary believes that the pressure of a gas is affected by its volume and temperature and that she has units for these magnitudes. She also believes that the volumes of the molecules (F1), attractive forces between them (F2), shapes of the molecules (F3), and imperfectly elastic collisions (F4) are also factors that affect the values of P, V, and T. She tries to construct as accurate a computable function as possible by idealizing the factors other than P, V, and T. In a sense she is maintaining that P is a non−computable function of V, T, F1, F2, F3, and F4. She idealizes F1 through F4 to get the following: (3) If G1 & G2 & G3 & G4, then P = f(V, T). **Mary takes (3) to be true** and also reasonably expects the O and C−values to be different. She knows **that if F1 through F4 are all of the factors that affect the |O − C| differences** (outside of measurement error), then she should be able to justifiably and **completely** explain the differences. She sees that her ideal conditions are relevant only if the factors

11 I note that Ramsey (1992) also does not see the goal as one of explaining the differences, although he does see the need to epistemically justify what he calls the "approximations", i.e., my C and O−value differences.

12 Lest some may think either Nowak's or my position are akin to Laymon's (1985, 1989), then one should note my formalization of Laymon's sequences: (a) [(T & I1 & I2 & ....In) −−−> C1]; (b) [(T & I1' & I2 & ...In) −−−> C2]; (c) [(T & I1' & I2' & ...In') −−−> Cn. The important thing to notice is that T remains constant in his sequences and is not known to be (vacuously) true prior to testing. Increasing accuracy in the sequence confirms that the unchanging T is true; while failure to increase accuracy confirms that T is false. Note also that Laymon only talks about replacing ideal conditions with more realistic ones and he thus does not talk about dropping them without replacement. And so I1 goes to I1' for Laymon, while for Nowak I1 is simply dropped. Laymon also does not **explicitly require** that a correction be made when he replaces one ideal condition with a more realistic one. Thus, just about any T can be disconfirmed by moving from, e.g., 'the molecules are **perfectly** spherical' to 'the molecules are **nearly** spherical', where without any corrections we do not get greater accuracy. Also, Laymon repeatedly takes an equation or conjunction of equations to be a theory. Now either these equations are asserted conditionally on the basis of their ideal conditions or they are not. If they are, then the result is true. If they are not, then they are false and known to be so prior to testing. Thus, Laymon misconceives the process as one where we need to find out whether a given T is true or false. Space forbids my arguing these points here, but hopefully one understands why I maintain that Laymon's position is very different from that of Nowak (and also my position) and that what he needs to recognize is that the ideal conditions are constitutive of the laws, and that the goal is to explain the differences.

they idealize exist and explain the differences at least in part.

What this fictional account reveals is an element of theorizing that seems to be missing in this day of treating theories as models of real systems. What is missing is the characterization of the real system modeled. In the above, this characterization is given in terms of a non−computable function or list.[13] If one takes such "lists" to be elements in theory construction, then one will want lists that have all and only relevant factors on them. The statement describing such a list will not be taken to be approximately true. Instead, it may be taken to be false because incomplete. Or it may be considered false because it has factors on it that are irrelevant or do not exist. Importantly, an underlying element of theorizing for a mathematical law/theory may be seen to be a list or a statement describing the list. The latter will be a kind of statement of relevance.[14] We will want a "literally true story" for this list and we will accept the statement describing the list in part **because** it is true. For example, no matter what equations one might set forth, phlogiston theory is false because there is no phlogiston; caloric theory is false because there is no caloric substance.

9. van Fraassen does not take such lists or their characterizations to be elements of theorizing. Nor, for that matter, do many realists.[15] van Fraassen's "minimalist" portrayal of realism "accidentally" works for lists; but it does not fit the idealized laws constructed from such lists. To the eight principles given above characterizing my kind

13 This I came to independently of Nowak's use of what he calls the "image" of the law or theory. The latter is quite literally a kind of list of factors taken to be relevant to a given magnitude, where Nowak treats some of these factors as primary, e.g., P, V, T, and others as secondary. See Nowak (1980). It is interesting that Cartwright has also now taken to talking about lists for idealized and abstract laws or theories, apparently under Nowak's influence. (1989, 207)

14 Mill's method of concomitant variation might form the basis for explicating the kind of relevance at issue.

15 Nowak (1980) and now maybe Cartwright (1989) are exceptions.

of scientific realism, we now add: (9) the mathematical laws are constructed on the basis of lists of factors taken to be relevant to each other; (10) an ideal condition In for such laws is taken to be relevant only if the factor Fn that it idealizes exists and plays the appropriate explanatory role. These ten conditions do truly provide a different conception of scientific realism, without appealing to “approximate truth”; they show that science does and should aim at explaining the differences.

10. I shall now make some very brief comments concerning implications of the position herein developed. First, if the equations are false, then one cannot explain using the covering law model of explanation. There are a number of reasons why this is so, but I focus only on the fact that one cannot explain why an O−value obtains by deducing it from some true initial conditions and the covering laws. This is so because one deduces a “false” C−value instead of the true O−value. Cartwright tries to deal with this problem of explaining true O−statements by deducing false C−statements by offering us her simulacrum account of explanation. (1983, 151−162) It is not necessary to go into the details of her simulacrum account here, since she herself thinks that it is inadequate. Instead, she believes that what is needed is “a theory of explanation which shows the relationship between causal processes and the fundamental laws we use to study them, and neither my simulacrum account nor the traditional covering−law account are of much help.” (1983, 162) It is my view that this paper offers such a theory of explanation, one which requires, happily, that the *explanandum* be what scientists themselves think is in need of explaining. I am thinking here of the |O − C| differences generated by the false equations (her “fundamental laws”) being explained by the idealized factors (her “causal processes”). From my perspective we explain these differences without appeal to

universal laws (a point that I cannot argue here).

Second, the other methods whereby one explains at least in part the |O − C|'s (excluding appeal to measurement error) are the following: 1. Change the experimental set up to make the O−values move closer to the C−values; 2. drop one ideal condition with a correction and a replacement with another ideal condition; 3. verify the existence of an hitherto unrecognized factor. The latter usually involves using the list as a guide or involves adding a new factor to the list.

Third, not all differences that need explaining are |O − C|'s. The history of science is full of cases where there is no way of measuring or observing an O−value to compare with a C−value. Instead, there are many ways to calculate C−values of the same variable. with incompatible results. How are these C−value differences to be explained? Perhaps more importantly for the realism debate, why is one C−value taken to be more accurate than another? I hope that one will now see that these differences will be accounted for in part in terms of the ideal conditions invoked in the different equations used to get the C−values.

BIBLIOGRAPHY

Achinstein, P. and Hannaway, O. (eds.) (1985) *Observation, Experiment, and Hypothesis in Modern Physical Science.* Cambridge and London: The MIT Press.

Barr, W. (1971) "A Syntactic and Semantic Analysis of Idealizations in Science", *Philosophy of Science* 38: 258−272.

Brzezinski, J. and Nowak, L. (eds) (1992) *Idealization III*. Amsterdam: Rodopi.

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

___________. (1989) *Nature's Capacities and Their Measurement*. Oxford: The Clarendon Press.

Churchland, P.M. and Hooker, C.A. (eds.) (1985) *Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen.* Chicago and London: The University of Chicago Press.

Giere, R. (1985) "Constructive Realism", in Churchland and Hooker (1985), 75−98.

Hull, D., Forbes, M., and Okruhlik, K. (eds.) (1992) *PSA 1992*. vol. 1. East Lansing: Philosophy of Science Association.

Laymon, R. (1985) "Idealizations and the Testing of Theories by Experimentation", in Achinstein and Hannaway (1985), 147−173.

_________. (1989) "Cartwright and the Lying Laws of Physics", *The Journal of Philosophy*, Vol. LXXXVI, #7: 353−372.

Niiniluoto, I. (1984) *Is Science Progressive?* Dordrecht: Reidel.

________. (1987) *Truthlikeness.* Dordrecht: Reidel.

Nowak, L. (1972) "Laws of Science, Theories, Measurement: Comments on Ernest Nagel's *The Structure of Science*, *Philosophy of Science* 39: 533−548.

________. (1980) *The Structure of Idealization: Towards a Systematic Interpretation of the Marxian Idea of Science.* Dordrecht: Reidel.

________. (1992) "The Idealization Approach to Science: A Survey", in Brzezinski and

Nowak (1992), 10−63.

Ramsey, J.L. (1992) “Towards an Expanded Epistemology for Approximations”, in Hull *et al* (1992), 154−164.

Sears, F.W. (1956) *Thermodynamics, The Kinetic Theory of Gases, and Statistical Mechanics*. Reading: Addison−Wesley Publishing Co.

Stranathan, J.D. (1942) *The “Particles” of Modern Physics*. New York: McGraw−Hill.

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