TOWARDS AN ARISTOTELEAN THEORY OF SCIENTIFIC EXPLANATION*

B. A. BRODY

Massachusetts Institute of Technology

In this paper, I consider a variety of objections against the covering-law model of scientific explanation, show that Aristotle was already aware of them and had solutions for them, and argue that these solutions are correct. These solutions involve the notions of nonHumean causality and of essential properties. There are a great many familiar objections, both methodological and epistemological, to introducing these concepts into the methodology of science, but I show that these objections are based upon misunderstandings of these concepts.

Let us begin by considering the following explanation of why it is that sodium normally combines with chlorine in a ratio of one-to-one[1]:

(A) (1) sodium normally combines with bromine in a ratio of one-to-one
    (2) everything that normally combines with bromine in a ratio of one-to-one normally combines with chlorine in a ratio of one-to-one

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    (3) therefore, sodium normally combines with chlorine in a ratio of one-to-one.

This purported explanation meets all of the requirements laid down by Hempel's covering law model for scientific explanation ([5], pp. 248–249). After all, the law to be explained is deduced from two other general laws which are true and have empirical content. Nevertheless, this purported explanation seems to have absolutely no explanatory power. And even if one were to say, as I think it would be wrong to say, that it does have at least a little explanatory power, why is it that it is not as good an explanation of the law in question (that sodium normally combines with chlorine in a ratio of one-to-one) as the explanation of that law in terms of the atomic structure of sodium and chlorine and the theory of chemical bonding? The covering law model, as it stands, seems to offer us no answer to that question.

A defender of the covering law model would, presumably, offer the following reply: both of these explanations, each of which meets the requirements of the model, are explanations of the law in question, but the explanation in terms of atomic structure is to be preferred to the explanation in terms of the way that sodium combines with bromine because the former contains in its explanans more powerful laws than the latter. The laws about atomic structure and the theory of bonding are more powerful than the law about the ratio with which sodium combines with bromine because more phenomena can be explained by the former than by the latter.

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[1] I first called attention to the problems raised by this type of explanation in my [1].

I find this answer highly unsatisfactory, partially because I don't see that (A) has any explanatory power at all. But that is not the real problem. The real problem is that this answer leaves something very mysterious. I can see why, on the grounds just mentioned, one would prefer to have laws like the ones about atomic structure rather than laws like the one about the ratio with which sodium and bromine combine. But why does that make explanations in terms of the latter type of laws less preferable? Or to put the question another way, why should laws that explain more explain better?

So much for my first problem for the covering-law model, a problem with its account of the way in which we explain scientific laws. I should now like to raise another problem for it, a problem with its account of the way in which we explain particular events. Consider the following three explanations:

(B) (1) If the temperature of a gas is constant, then its pressure is inversely proportional to its volume
(2) at time t_1, the volume of the container c was v_1 and the pressure of the gas in it was p_1
(3) the temperature of the gas in c did not change from t_1 to t_2
(4) the pressure of the gas in container c at t_2 is $2p_1$

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(5) the volume of x at t_2 is $\frac{1}{2}v_1$.

(C) (1) if the temperature of a gas is constant, then its pressure is inversely proportional to its volume
(2) at time t_1, the volume of the container c was v_1 and the pressure of the gas in it was p_1
(3) the temperature of the gas in c did not change from t_1 to t_2
(4) the volume of the gas in container c at t_2 is $\frac{1}{2}v_1$

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(5) the pressure of c at t_2 is $2p_1$.

(D) (1) if the temperature of a gas is constant, then its pressure is inversely proportional to its volume
(2) at time t_1, the volume of the container c was v_1 and the pressure of the gas in it was p_1
(3) the temperature of the gas in c did not change from t_1 to t_2
(4) by t_2, I had so compressed the container by pushing on it from all sides that its volume was $\frac{1}{2}v_1$

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(5) the pressure of c at t_2 is $2p_1$.

All three of these purported explanations meet all of the requirements of Hempel's model. The explanandum, in each case, is deducible from the explanans which, in each case, contains at least one true general law with empirical content. And yet, there are important differences between the three. My intuitions are that (B) is no explanation at all (thereby providing us with a clear counter-example to Hempel's model), that (C) is a poor explanation, and that (D) is a much better one. But if your intuitions are that (B) is still an explanation, even if not a very good one, that makes no difference for now. The important point is that there is a clear difference between the explanatory power of these three explanations, and the covering law model provides us with no clue as to what it is.

These problems and counter-examples are not isolated cases. I shall give, later on in this paper after I offer my own analysis and solution of them, a recipe for constructing loads of additional problems and counter-examples. Now the existence of these troublesome cases led me to suspect that there is something fundamentally wrong with the whole covering law model and that a new approach to the understanding of scientific explanation is required. At the same time, however, I felt that this model, which fits so many cases and seems so reasonable, just couldn't be junked entirely. This left me in a serious dilemma, one that I only began to see my way out of after I realized that Aristotle, in the *Posterior Analytics*, had already seen these problems and had offered a solution to them, one that contained both elements of Hempel's model and some other elements entirely foreign to it. So let me begin my presentation of my solution to these problems by looking at some aspects of Aristotle's theory of scientific explanation.

Aristotle (*ibid.*, I, 13), wanted to draw a distinction between knowledge of the fact (knowledge that *p* is so) and knowledge of the reasoned fact (knowledge why *p* is so) and he did so by asking us to consider the following two arguments, the former of which only provides us with knowledge of the fact while the latter of which provides us with knowledge of the reasoned fact:

(E) (1) the planets do not twinkle
(2) all objects that do not twinkle are near the earth

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(3) therefore, the planets are near the earth.

(F) (1) the planets are near the earth
(2) all objects that are near the earth do not twinkle

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(3) therefore, the planets do not twinkle.

The interesting thing about this point, for our purposes, is that while both of these arguments fit Hempel's model,[2] only one of them, as Aristotle already saw, provides us with an explanation of its conclusion. Moreover, his account of why this is so seems just right:

> ... of two reciprocally predicable terms the one which is not the cause may quite easily be the better known and so become the middle term of the demonstration. ... This syllogism, then, proves not the reasoned fact but only the fact; since they are not near because they do not twinkle. The major and middle of the proof, however, may be reversed, and then the demonstration will be of the reasoned fact ... since its middle term is the proximate cause. (78^a 28–78^b 3)

[2] Leaving aside the question, irrelevant for us now, about the truth of these premises, we shall throughout this discussion just assume that they are true.

In other words, nearness is the cause of not twinkling, and not vice versa, so the nearness of the planets to the earth explains why they do not twinkle, but their not twinkling does not explain why they are near the earth.

It is important to note that such an account is incompatible with the logical empiricists' theory of causality as constant conjunction. After all, given the truth of the premises of (E) and (F), nearness and nontwinkling are each necessary and sufficient for each other, so, on the constant conjunction account each is equally the cause of the other.[3] And even if the constant conjunction account is supplemented in any of the usual ways, nearness and nontwinkling would still equally be the cause of each other. After all, both 'all near celestial objects twinkle' and 'all twinkling celestial objects are near' contain purely qualitative predicates, have a potentially infinite scope, are deducible from higher-order scientific generalizations and support counterfactuals. In other words, both of these generalizations are law-like generalizations, and not mere accidental ones, so each of the events in question is, on a sophisticated Humean account, the cause of the other. So Aristotle's account presupposes the falsity of the constant conjunction account of causality. But that is okay. After all, the very example that we are dealing with now, where nearness is clearly the cause of nontwinkling but not vice versa, shows us that the constant conjunction theory of causality (even in its normal more-sophisticated versions) is false.

Now if we apply Aristotle's account to our example with the gas, we get a satisfactory account of what is involved there. The decrease in volume (due, itself, to my pressing on the container from all sides) is the cause of the increase in pressure, but not vice versa, so the former explains the latter but not vice versa. And Aristotle's account also explains a phenomenon called to our attention by Bromberger [2], viz. that while we can deduce both the height of a flagpole from the length of the shadow it casts and the position of the sun in the sky and the length of the shadow it casts from the height of the flagpole and the position of the sun in the sky, only the latter deduction can be used in an explanation. It is easy to see why this is so; it is the sun striking at a given angle the flagpole of the given height that causes its shadow to have the length that it does, but the sun striking the flagpole when its shadow has the length of the shadow is surely not the cause of the height of the flagpole.

Generalizing this point, we can add a new requirement for explanation: a deductive-nomological explanation of a particular event is a satisfactory explanation of the event when (besides meeting all of Hempel's requirements) its explanans contains essentially a description of the event which is the cause of the event described in the explanandum. If they do not then it may not be a satisfactory explanation. And similarly, a deductive-nomological explanation of a law is a satisfactory explanation of that law when (besides meeting all of Hempel's requirements) every event which is a case of the law to be explained is caused by an event

[3] Unless one adds the requirement that the cause must be before the effect, the normal way of drawing an asymmetry between causes and effects when each are necessary and sufficient for the other, in which case neither is the cause of the other and Aristotle's account still won't do.

which is a case of one (in each case, the same) of the laws contained essentially in the explanans.[4]

It might be thought that what we have said so far is sufficient to explain why it is that (A) is not an explanation and why it is that the explanation of sodium and chlorine's combining in a one-to-one ratio in terms of the atomic structure of sodium and chlorine is an explanation. After all, no event which is a case of sodium and chlorine combining in a one-to-one ratio is caused by any event which is a case of sodium and bromine combining in a one-to-one ratio. So, given our requirements, deduction (A), even though it meets all of Hempel's requirements, need not be (and indeed is not) an explanation. But every event which is a case of sodium and chlorine combining in a one-to-one ratio is caused by the sodium and chlorine in question having the atomic structure that they do (after all, if they had a different atomic structure, they would combine in a different ratio). So an explanation involving the atomic structure would meet our new requirement and would therefore be satisfactory.

The trouble with this account is that it incorrectly presupposes that it is the atomic structure of sodium and chlorine that cause them to combine in a one-to-one ratio. A whole essay would be required to show, in detail, what is wrong with this presupposition; I can, here, only briefly indicate the trouble and hope that this brief indication will be sufficient for now: a given case of sodium combining with chlorine is the same event as that sodium combining with that chlorine in a one-to-one ratio, and, like all other events, that event has only one cause.[5] It is, perhaps, that event which brings it about that the sodium and chlorine are in proximity to each other under the right conditions. That is the cause of the event in question, and not the atomic structure of the sodium and chlorine in question (which, after all, were present long before they combined). To be sure, these atomic structures help explain one aspect of the event in question, the ratio in which they combine, but that does not make them the cause of the event.[6]

To say that the atomic structure of the atoms in question is not the cause of their combining in a one-to-one ratio is *not* to say that a description of that structure is not an essential part of any causal explanation of their combining. It obviously is. But equally well, to say that a description of it is a necessary part of any causal explanation is *not* to say that it is (or is part of) the cause of their combining. There is a difference, after all, between causal explanations and causes and between parts of the former and parts of the latter. Similarly, to say that the atomic structure is not the cause of their combining is *not* to say that that event had no cause; indeed, we suggested one (the event which brought about the proximity of the atoms) and others can also be suggested (the event of the atoms acquiring certain specific

[4] We have made this condition sufficient, but not necessary, for reasons that will emerge below. It will also be seen there that Aristotle, who had a broader notion of cause, could have made it necessary as well.

[5] To be sure, e_1 can have as causes both e_2 and e_3 (where $e_2 \neq e_3$) but only when either e_2 is the cause of e_3 or e_3 the cause of e_2. That exception is of no relevance here.

[6] It might, at least, be maintained that they are still the cause of that aspect of the event. But that is just a confusion—it is events, and not their aspects, that have causes.

electrical and quantum mechanical properties). It is only to say that the atomic structure is not the cause.

Keeping these two points in mind, we can see that all that we said before was that the perfectly satisfactory explanation, in terms of the atomic structure of the atoms, of their combining in a one-to-one ratio does not meet the condition just proposed because it contains no description of the event which caused the combining to take place. But since it obviously is a good explanation, some additional types of explanations must be allowed for.

So Aristotle's first suggestion, while quite helpful in solving some of our problems, does not solve all of them. There is, however, another important suggestion that he makes that will, I believe, solve the rest of them. Aristotle says:

> Demonstrative knowledge must rest on necessary basic truths; for the object of scientific knowledge cannot be other than it is. Now attributes attaching essentially to their subjects attach necessarily to them. . . . It follows from this that premises of the demonstrative syllogism must be connections essential in the sense explained: for all attributes must inhere essentially or else be accidental, and accidental attributes are not necessary to their subjects. (*Posterior Analytics* 74^{b} 5–12)

There are many aspects of this passage that I do not want to discuss now. But one part of it seems to me to suggest a solution to our problem. It is the suggestion that a demonstration can be used as an explanation (can provide us with "scientific knowledge") when at least one of the explanans essential to the derivation states, that a certain class of objects has a certain property, and (although the explanans need not state this) that property is possessed by those objects essentially.

Let us, following that suggestion, now look at our two proposed explanations as to why sodium combines with chlorine in a ratio of one-to-one. In one of them, we are supposed to explain this in terms of the fact that sodium combines with bromine in a one-to-one ratio. In the other explanation, we are supposed to explain this in terms of the atomic structure of sodium and chlorine. Now in both of these cases, we can demonstrate from the fact in question (and certain additional facts) that sodium does combine with chlorine in a one-to-one ratio. But there is an important difference between these two proposed explanations. The atomic structure of some chunk of sodium or mass of chlorine is an essential property of that object. Something with a different atomic number would be (numerically) a different object. But the fact that it combines with bromine in a one-to-one ratio is not an essential property of the sodium chunk, although it may be true of every chunk of sodium. One can, after all, imagine situations[7] in which it would not combine in that ratio but in which it would still be (numerically) the same object. Therefore, one of our explanans, the one describing the atomic structure of sodium and chlorine, contains a statement that attributes to the sodium and chlorine a property which is an essential property of that sodium and chlorine (even if the statement does not

[7] Even ones in which all currently believed scientific laws hold, but in which the initial conditions are quite different from the ones that now normally hold.

say that it is an essential property), while the other of our explanans, the one describing the way in which sodium combines with bromine, does not. And it is for just this reason that the former explanans, but not the latter, explains the phenomenon in question.

Generalizing this Aristotelean point, we can set down another requirement for explanations as follows: a deductive-nomological explanation of a particular event is a satisfactory explanation of that event when (besides meeting all of Hempel's requirements) its explanans contains essentially a statement attributing to a certain class of objects a property had essentially by that class of objects (even if the statement does not say that they have it essentially) and when at least one object involved in the event described in the explanandum is a member of that class of objects. If this requirement is unfulfilled, then it may not be a satisfactory explanation. And similarly, a deductive-nomological explanation of a law is a satisfactory explanation of that law when (besides meeting all of Hempel's requirements) each event which is a case of the law which is the explanandum, involves an entity which is a member of a class (in each case, the same class) such that the explanans contain a statement attributing to that class a property which each of its members have essentially (even if the statement does not say that they have it essentially).

It is important to note that such an account is incompatible with the logical empiricist conception of theoretical statements as instruments and not as statements describing the world. For after all, many of these essential attributions are going to be theoretical statements, and they can hardly be statements attributing to a class of objects an essential property if they aren't really statements at all. But that is okay, for it just gives us one more reason for rejecting an account, more notable for the audacity of its proponents in proposing it than for its plausability or for the illumination it casts.

There are two types of objections to essential explanations that we should deal with immediately. The first really has its origin in Duhem's critique of the idea that scientific theories explain the observable world ([3], Ch. 1). Duhem argued that if we view a theory as an explanation of an observable phenomenon, we would have to suppose that the theory gives us an account of the physical reality underlying what we observe. Such claims about the true nature of reality are, however, empirically unverifiable metaphysical hypotheses, which scientists should shun, and therefore we must not view a theory as an attempt to explain what we observe. Now contemporary theories of explanation, like the deductive-nomological model, avoid this problem, by not requiring of an explanation that its explanans describe the true reality underlying the observed explanandum. But if we now claim that a deductive-nomological explanation is a satisfactory explanation when (among other possibilities) its explanans describe essential properties of some objects involved in the explanandum event, aren't we introducing these disastrous, because empirically undecidable, issues about the true nature of the reality of these objects into science? After all, the scientist will now have to decide, presumably by nonempirical means, whether the explanans do describe the essence of the objects in question.

The trouble with this objection is that it just assumes, without any arguments, that claims about the essences of objects would have to be empirically undecidable claims, claims that could be decided only upon the basis of metaphysical assumptions. This presupposition, besides being unsupported, just seems false. After all, the claim that the essential property of sodium is its atomic number (and not its atomic weight, or its color, or its melting point) can be defended empirically, partially by showing that for this property, unlike the others just mentioned, there are no obvious cases of sodium which do not have it, and partially by showing how all objects that have this property behave alike in many important respects while objects which do not have this property in common do not behave alike in these important respects. Now a lot more has to be said about the way in which we determine empirically the essence of a given object (or of a given type of object), and we will return to this issue below, but enough has been said, I think, to justify the claim that the idea that scientific explanation is essential explanation does not mean that scientific explanation involves empirically undecidable claims.

It should be noted, by the way, that this idea of the discovery of essences by empirical means is not new to us. It was already involved in Aristotle's theory of *epagoge* (*op. cit.*, II, 19). I do not now want to enter into the question as to exactly what Aristotle had in mind, if he did have anything exact in mind, when he was describing that process. It is sufficient to note that he, like all other true adherents to the theory of essential explanation, saw our knowledge of essences as the result of reflection upon what we have observed and not as the result of some strange sort of metaphysical knowledge.

The second objection to essential explanations has been raised by Popper. He writes:

> The essentialist doctrine that I am contesting is solely the doctrine that science aims at ultimate explanation; that is to say, an explanation which (essentially, or, by its very nature) cannot be further explained, and which is in no need of any further explanation. Thus my criticism of essentialism does not aim at establishing the nonexistence of essences; it merely aims at showing the obscurantist character of the role played by the idea of essences in the Galilean philosophy of science. ([8], p. 105)

Popper's point really is very simple. If our explanans contain a statement describing essential properties (e.g. sodium has the following atomic structure . . .), then there is nothing more to be said by way of explaining these explanans themselves. After all, what could we say by way of answering the question "why does sodium contain the atomic structure that it does"? So the use of essential explanations leads us to unexplainable explanans, and therefore to no new insights gained in the search for explanations of these explanans, and therefore to scientific sterility. Therefore, science should reject essential explanations.

There are, I believe, two things wrong with this objection. To begin with, Popper assumes that essential explanations will involve unexplainable explanans, and this is usually only partially true. Consider, after all, our explanation of sodium's combining with chlorine in a one-to-one ratio in terms of the atomic structure of sodium and chlorine. The explanans of that explanation, besides containing statements attributing to sodium and chlorine their essences (viz. their atomic number), also contain the general principles of chemical bonding, and these are not unexplainable explanans since they are not claims about essences. In general, even essential explanations leave us with some part (usually the most interesting part) of their explanans to explain, and they do not therefore lead to sterility in future enquiry. But, in addition, even if we did have an essential explanation all of whose explanans were essential statements and therefore unexplainable explanans, what are we to do according to Popper? Should we reject the explanation? Should we keep it but believe that it is not an essential explanation? Neither of these strategies seem very plausible in those cases where we have good reasons both for supposing that the explanation is correct and for supposing that the explanans do describe the essential properties of the objects in question. It cannot after all, be a good scientific strategy to reject what we have good reasons to accept. So even if Popper's claim about their sterility for future scientific enquiry is true for some essential explanations, I cannot see that it gives us any reasons for rejecting these explanations, or for rejecting their claim to be essential explanations, when these explanations and claims are empirically well supported.

There is, of course, a certain point to Popper's objection, a point that I gladly concede. As is shown by his example from the history of gravitational theory, people may rush to treat a property as essential, without adequate empirical evidence for that claim, and then it may turn out that they were wrong. They may even have good evidence for the claim that the property is essential and still be wrong. In either case, enquiry has been blocked where it should not have been blocked. We should certainly therefore be cautious in making claims about essential properties and should, even when we make them on the basis of good evidence, realize that they may still be wrong. But these words of caution are equally applicable to all scientific claims; the havoc wreaked by false theories that lead enquiry along mistaken paths can be as bad as the havoc wreaked by false essential claims that block enquiry. And since they are only words of caution, they should not lead us to give up either theoretical explanations in general or essential explanations in particular.

Let us see where we now stand. We have, so far, rejected Hempel's requirements for an explanation on the grounds that they are not sufficient and we have suggested two alternative Aristotelean conditions such that, for the set of explanations meeting Hempel's requirements, any explanation meeting either of these requirements is an adequate explanation.[8] Doing this is sufficient to help us deal with the problem of self-explanation, another problem that the covering-law model has had difficulty with. As Hempel already recognized in his original presentation of the covering-law model, we need some additional requirement to rule out such obvious self-explanations as

[8] We have not, however, required as a necessary condition that any explanation must meet one of these two conditions. This is so, partially because of the problem of statistical explanations, but partially because of the possibility, raised by Aristotle, that there are additional types of explanations. After all, our two conditions let in explanations in terms of Aristotle's efficient and formal causes. We still have to consider, but will not in this paper, possible explanations in terms of what he would call material and final causes.

$$\text{(G)}\quad \frac{\begin{array}{l}(x)Px\\ Qa\end{array}}{Qa}$$

and slightly less obvious self-explanations such as

$$\text{(H)}\quad \frac{\begin{array}{l}(x)(Px \cdot Qa)\\ Qa \vee \sim Qa\end{array}}{Qa}$$

He proposed a simple solution to that problem but Eberle, Kaplan, and Montague showed that it wouldn't do [4]. Consider, they said, the following example of a bad explanation, that meets all of Hempel's requirements, of an object's having a property H. Let us take any true law of the form $(x)Fx$ (where there is no connection between an object's having the property F and its having the property H). From that law it follows that (where G is any third unrelated property)

(1) $(x)(y)[Fx \vee (Gy \supset Hy)]$

It also follows from Ha, the fact to be explained, that

(2) $(Fb \vee \sim Ga) \supset Ha$

But from these two true statements, we can derive Ha, and this derivation, a subtle form of self-explanation, meets all of Hempel's requirements, so Hempel still had not solved the problem of self-explanation. Now there exist several syntactic solutions to this problem, solutions that are partially ad hoc and partially intuitively understandable (see [6] and [7]). As such, they are not entirely satisfactory. Writing about one of them, Hempel admits that:

> ... it would be desirable to ascertain more clearly to what extent the additional requirement is justifiable, not on the ad hoc grounds that it blocks those proofs, but in terms of the rationale of scientific explanation. ([5], p. 295)

Now our theory offers a simple, non-ad hoc, solution to this problem. The derivation used by Eberle, Kaplan, and Montague does not meet either of our two conditions. Neither (1) nor (2) describe the cause of Ha. And neither (1) nor (2) ascribe an essential property to the members of a certain class of objects of which a is a member. Therefore, although their derivation meets all of Hempel's requirements, it need not be (and indeed is not) an adequate explanation.

By now, the advantages of our theory are obvious. It provides intuitively satisfactory, non-ad hoc, solutions to problems that the covering-law model cannot handle. And at the same time, it incorporates (by keeping Hempel's requirements) the elements of truth in the covering-law model. It only remains, therefore, to consider the one serious objection to this whole Aristotelean theory, an objection that we have already touched upon when we dealt with Duhem. Given what we mean by 'causality' and 'essence', can we ever know that e_1 is the cause of e_2 or that P is an essential property of e_1, and if so, how can we know this?

This problem can be sharpened considerably. There is no problem, in principle, about our coming to know that events of type E_1 are constantly conjoined with events of type E_2. All that we have to do is to observe that this is so in enough varied cases. And if 'e_1 causes e_2' only means that 'e_1 is an event of type E_1 and e_2 is an event of type E_2 such that E_1 is constantly conjoined with E_2', we can easily see how we could come to know that e_1 is the cause of e_2. But if, as our Aristotelean account demands, 'e_1 causes e_2' means something more than that, can we know that it is true, and if we can, how can we know that it is true? Similarly, there is no problem, in principle, about our coming to know that objects of type O_1 have a certain property P_1 in common. All that we have to do is to observe that this is so in enough varied cases. And if 'P_1 is an essential property of o_1' only means that 'o_1 is an object of type O_1 and all objects of type O_1 have P_1', we can easily see how we could come to know that P_1 is an essential property of o_1. But if, as our Aristotelean theory demands, 'P_1 is an essential property of o_1' means something more than that, can we know that this is true, and if we can, how can we know that this is true?

There is an important difference between these questions. If we conclude that we cannot, or do not, know the truth of statements of the form 'e_1 causes e_2' or 'P_1 is an essential property of o_1' (where these statements are meant in the strong sense required by our theory), then our theory must be rejected. After all, knowledge of the truth of statements of that form is, according to our theory, a necessary condition for knowing that we have (although not for having) adequate explanations. And we obviously do know, in at least some cases, that a given explanation is adequate. So if we cannot, or do not, know statements of the above form, our theory is false. However, if we conclude that we can, and do, know the truth of statements of the above-mentioned form, but we don't know how we know their truth, then all that we have left is a research project, viz. to find out how we know their truth; what we don't have is an objection to our theory.

This is an extremely important point. I shall show, in a moment, that we do have, and a fortiori can have, knowledge of these statements. But, to be quite frank, I have no adequate account (only the vague indications mentioned above when talking about Duhem) of how we have this knowledge. So, on the basis of this last point, I conclude that the Aristotelean theory of explanation faces a research problem about knowledge (hence the title of this paper), but no objection about knowledge.

Now for the proof that we do, and a fortiori can, have knowledge of the above-mentioned type. Our examples will, I am afraid, be familiar ones. It seems obvious that we know that

(1) if the temperature of a gas is constant, then an increase in its pressure is invariably accompanied by an inversely proportional decrease in its volume
(2) if the temperature of a gas is constant, then a decrease in its volume is invariably accompanied by an inversely proportional increase in its pressure
(3) if the temperature of a gas is constant, then an increase in its pressure does not cause an inversely proportional decrease in its volume
(4) if the temperature of a gas is constant, then a decrease in its volume does cause an inversely proportional increase in its pressure.

Here we have causal knowledge of the type required, since the symmetry between (1) and (2) and the asymmetry between (3) and (4) show that the causal knowledge that we have when we know (3) and (4) is not mere knowledge about constant conjunctions. Similarly, it seems obvious that we know that

(1) all sodium has the property of normally combining with bromine in a one-to-one ratio
(2) all sodium has the property of having the atomic number 11
(3) the property of normally combining with bromine in a one-to-one ratio is not an essential property of sodium
(4) the property of having the atomic number 11 is an essential property of sodium.

Here we have essential knowledge of the type required, since the symmetry between (1) and (2) and the asymmetry between (3) and (4) show that the essential knowledge that we have when we know (3) and (4) is not mere knowledge about all members of a certain class having a certain property.

I conclude, therefore, that we have every good reason to accept, but none to reject, the Aristotelean theory of explanation sketched in this paper. And I also conclude that it therefore behooves us to find out how we have the type of knowledge mentioned above, the type of knowledge that lies behind our knowledge that certain explanations that we offer really are adequate explanations.

REFERENCES

[1] Brody, B. A. "Natural Kinds and Real Essences." *Journal of Philosophy* (1967).
[2] Bromberger, S. "Why Questions." *Introductory Readings in the Philosophy of Science.* Edited by B. A. Brody. Englewood Cliffs: Prentice Hall, 1969.
[3] Duhem, P. *La Théorie Physique, Son Objet et sa Structure.* Paris: Chevalier et Rivière, 1914.
[4] Eberle, R. A., Kaplan, D., and Montague, R. "Hempel and Oppenheim on Explanation." *Philosophy of Science* (1961).
[5] Hempel, C. G. *Aspects of Scientific Explanation.* New York: Free Press, 1955.
[6] Kaplan, D. "Explanation Revisited." *Philosophy of Science* (1961).
[7] Kim, J. "Discussion: On the Logical Conditions of Deductive Explanation." *Philosophy of Science* (1963).
[8] Popper, K. R. *Conjectures and Refutations.* London: 1963.

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[1] **Natural Kinds and Real Essences**
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References

[1] **Natural Kinds and Real Essences**
B. A. Brody
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Stable URL:
http://links.jstor.org/sici?sici=0022-362X%2819670720%2964%3A14%3C431%3ANKARE%3E2.0.CO%3B2-E

[4] **Hempel and Oppenheim on Explanation**
Rolf Eberle; David Kaplan; Richard Montague
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Stable URL:
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[6] **Explanation Revisited**
David Kaplan
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[7] **On the Logical Conditions of Deductive Explanation**
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Stable URL:
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