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EXPLANATION IS A GENUS: AN ESSAY ON THE VARIETIES OF SCIENTIFIC EXPLANATION

ABSTRACT. I shall endeavor to show that every physical theory since Newton explains without drawing attention to causes–that, in other words, physical theories *as physical theories* aspire to explain under an ideal quite distinct from that of causal explanation. If I am right, then even if sometimes the explanations achieved by a physical theory are *not* in violation of the standard of causal explanation, this is purely an accident. For physical theories, as I will show, do not, as such, aim at accommodating the goals or aspirations of causal explanation. This will serve as the founding insight for a new theory of explanation, which will itself serve as the cornerstone of a new theory of scientific method.

1. THE PHILOSOPHICAL TRAJECTORY

I propose to show that physics does not seek causal explanation, but instead explanation of a different sort altogether. This will establish that physical theories do indeed explain, as well as predict and describe. The widespread suspicion that physical theories like quantum mechanics do not – on their merits as physical theories – explain, rests on the prejudice that physical theories do not, as physical theories, aspire to an explanatory goal specific to the discipline and context of physics. And this prejudice, in its turn, rests in the falsehood that physical theories begin life philosophically innocent, presupposing nothing. For, as I will show, physical theories begin life already presupposing a general metaphysical framework for explanation, exclusive of specific physical content, but specific to the context of doing physics. In other words, formulation of something recognizable as a modern physical theory, involves adopting a certain constellation of goals and conceptual apparatus, so coherent and fundamental as literally to escape notice. Very specifically, physical theories begin life already having adopted the goal of illuminating what I will call a *physical dependence relation.* This discovery will provide the basis for an argument to the effect that each sort of explanation – and there are many kinds – is illumination of a (corresponding) kind of dependence relation.

The central moral of this paper – namely, that explanations are illuminations of various kinds of dependence relations – will rest fundamentally on

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a taxonomy of dependence relations in which causal dependence figures as only one, and not even a very distinguished one, among many. The most neglected of these dependence relations will be the *physical dependence relations* of physics, which as I will demonstrate, are quite different from relations of causal dependence. Indeed, there is no genus-to-species relationship between the two. But together with their fellow dependence relations, and the families these fall into, they form a space of dependence relations, and hence an explanation space. This explanation space will be the locus of a new proposal (to be sketched in a very broad hand at the end of the paper) for scientific methodology in the spirit of Aristotle, which respects all the lessons we have accumulated from Hume through Kuhn.

Thus I will endeavor to make manifest the metaphysical foundations of any physical theory, no matter what its content, by demonstrating that physical theories aspire to an explanatory goal that is wholly distinct from that of causal explanation, but a quite respectable goal all the same. If this is right, then even if sometimes the explanations achieved by a physical theory are *not* in violation of causal expectations, this is purely an accident, as physical theories do not, on their merits as physical theories, aim at accommodating these expectations. And toward the end of the paper I will show – indeed it will require little argument by that point – that quantum mechanics does in fact aspire to the goal of physical explanation, and thus that quantum mechanics, in addition to its predictive capabilities, possesses also the capability of explaining the EPR-type correlations it so very unerringly predicts. My proposal that to explain in physics is to illuminate physical dependence relations will, in addition, shed light on the status of the so-called "uncertainty" relations in quantum mechanics, as well as suggest a potential explanation of the failure of magnitude-definiteness in that most successful of physical theories.

2. EXPLANATION

What is scientific explanation? How does a response to a why-question such as: Why do unsupported objects fall toward the center of the earth?, qualify as a scientific explanation? What credentials does it share in common with a response to the different why-question: Why has the dodo gone extinct? One influential answer is that a response to a why-request qualifies as a scientific explanation, when it illuminates significant aspects of the confluence of *causes*, in the modern sense of that term, of the fact for which explanation is sought.[1] According to friends of this causal answer, *theories* do not qualify as explanations – at least not on their credentials *as* theories. For example, there is widespread suspicion that *physical* theories, while oftentimes quite good instruments for purposes of prediction, are not necessarily good instruments for purposes of explanation. This is because physical theories do not always admit of being understood as illuminating causes. And the favorite example of a physical theory that predicts but does not explain causally is quantum mechanics.

I shall argue that the acclaimed causal answer to the question of explanation is too narrow in its outlook, not least for the reason that it dismisses out of hand such things as scientific explanations in mathematics, since obviously mathematics does not trade in causes so understood. I shall reject the proposition that mathematics does not furnish explanations on all fours with the "sciences". Thus I shall uphold the idea that causal explanation is neither the alpha nor the omega of scientific explanation. And I shall offer an answer to the question of explanation that Hume himself – that trend-setting skeptic about the unseen – might have been proud of. It will lead naturally to a new account of scientific method.

Perhaps the biggest rivals of the causal theory of scientific explanation are descendants of the prototype articulated in Hempel and Oppenheim's watershed article: the DN – for "deductive-nomological" – model. (This is the classic standard by which every rival is nowadays compared. It says, roughly, that explanations are valid arguments with statements of law-like regularity in the premises and statement of the fact one seeks explained in the conclusion.) The idea there is simply that explanation is a certain form of *argument*. One forceful articulation of this idea in currency today is due to Philip Kitcher (1989). Very roughly, his idea is that explanation is a relation amongst propositions, and that a proposed explanation is good to the extent that it harmonizes with, and also simplifies, our total account of the world. In other words, to the extent that our theories are themselves simple and unified, they explain what the world *out there* is like. On this view of explanation, the mathematician's story about why Fermat's last theorem is true, for example, also can turn out to be an explanation, if the mathematician can produce a piece of argumentation in the theorem's favor, whereas on the causal view no piece of mathematics can pass for a scientific explanation, since no piece of mathematics ever invokes a cause in the proper sense of that term.

The important contrast between Kitcher's view and that of causal theorists is this: whereas Kitcher conducts his analysis of explanation in the kingdom of our speeches and proofs propounding and elaborating upon the facts we want explained – in other words, in the realm of theory – his rivals conduct theirs in the kingdom of the facts themselves. Facts explain facts, on the causal theorists' account, whereas on Kitcher's it is language or theory – in other words, our construal of facts, expunged of all the particles and dirt of reality – that does the explaining. (Kitcher's perspective owes a considerable debt to an era in which philosophers conducted their business as if it were all a matter of mustering clouds of witnesses to stand up for prevailing sensibilities vis-à-vis what is appropriate in certain dialogue settings. It was a golden age of linguistic analysis, in which attention to facts as such was both unnecessary and irrelevant. And nobody was bothering to ask whether the assorted subject matters about which language users purported to express themselves, merited differential attentions according to differences amongst their subjects.)

I do not like Kitcher's position any better than the contrasting explanation-as-cause position. A story (most probably apocryphal) is circulated about the novelist and general wiseacre Mark Twain. Twain, it seems, had a fondness for profanity, which his wife did not share. She sought one day to cure him of it, by fighting fire with fire. Whereupon Mark Twain is said to have replied: My dear, you've definitely got the words, but you haven't got the music. Now I think that Kitcher's got the words, but not the music. He has fashioned an account whereby everyone who does something theoretical, and executes a passage (roughly) from general propositions to more specific ones, is engaged in the business of explanation; but it's all done with mirrors, as it were, because Kitcher's got language rather than facts doing the explaining. And his rivals have it the other way around: they've got the music – they've actually got the facts entering into explanatory relations with one other – but on their account we get too few explanatory relations. I want to bring music and words together; so that my account shall have the advantages of both words and music, whilst avoiding the disadvantages of each on its own. My counter-causal proposal is that scientific explanation is illumination of a dependence relation of some sort, but that causal dependence is only *one* species of dependence relation among many – and a marginal one at that, when it comes to the family tree of dependence relations. To make a strong case, I shall of course have to produce a taxonomy of dependence relations, and argue forcefully that different sciences trade in different dependence relations, with causal dependence being among the poorest cousins of the robust relations in which mathematics, physics, psychology, biology and their close relatives, trade. I can't do all of that here, but I will get as far as physics. That by itself is a large task, and suggestive of how we might proceed to finish the job.

3. PRELIMINARY POINTS OF CONTRAST

I shall say that explanation, as such, is the illumination of a dependence relation, that each discipline deals with a roughly proprietary stock of dependence relations, and hence that a proposed explanation involves (either implicitly or explicitly) a decision as to which scientific discipline is best positioned to handle the fact for which explanation is sought. And of course this proposal shares a certain family resemblance with the causal theorist's proposal that explanation is illumination of causal relations exclusively. This suggests – correctly, as I will now argue – that my proposal outperforms the DN model in many of the same ways that the causal theorist's proposal does. As I've already mentioned, it will outperform the causal model in the ways that the DN model does – primarily by acknowledging more sorts of explanation than there are sorts of causal relations. So it has the best of both models. And more besides. Here's how.

Suppose (famously) that two quantities (for example the height of a certain flagpole and the length of its shadow) are functionally dependent – that (roughly, for now, but in way that will be made very precise shortly) they vary together in a certain regular way. Then it will be possible to assemble a derivation of the magnitude of one from premises stating the magnitude of the other, provided enough other general laws are brought to bear as well. The DN model gets in trouble over this fact, which is called the *problem of asymmetry*. For (as some allege), a flagpole's height explains its shadow's length, but the reverse is not also true.

My proposal might be suspected to fail in exactly the same way as the DN model fails. For an illumination of a functional dependence is surely an illumination of a dependence relation; and so if functional dependence is symmetrical (as surely it is), then surely my account (just like the DN model) will be committed to saying that explanation between functionally dependent quantities is symmetrical as well.

Causal theorists are in a position to say that the derivation in reverse (of flagpole height from shadow length) derives a cause from an effect, and so does not provide an explanation, since an explanation must be mounted in reverse (cause explains effect and not the other way around). Similarly but also more subtly, I answer as follows.[2] First and foremost, my account discriminates amongst the species of explanation (as species of dependence) in which the self-same shadow's length stands to the flagpole's height, and so must allow that there may be more than one sort of explanation for any given fact in terms of the same explanans. The most self-evident relation in which the two quantities stand is functional dependence. So there is a functional explanation of shadow length in terms of flagpole height. Now if our reaction to a particular case (as for example to the flagpole case) is that the explanation relation between them cannot be symmetrical between explanans and explanandum – between flagpole height and shadow length, in this instance – then my account will be in a position to say that we were not seeking a functional explanation of the shadow length in the first instance. Since there are other sorts of dependence relations in which stand flagpole height to shadow length, there are correspondingly different sorts of explanatory relations as well. One of them is (of course) causal dependence; but another is what I shall call *physical dependence*. I will show that even when two quantities are functionally dependent, this does not guarantee that they are physically dependent or causally dependent. So if one is seeking a physical explanation, it will not do simply to take notice of a functional dependence and derive one quantity from another on the basis of it taken together with some general laws. One must also show that there is physical or causal or whatever other sort of dependence is deemed appropriate. Simple functional dependence is generally deemed appropriate only for very abstract domains.

Suppose now that a certain magician waves his wand over a sample of table salt, thereby "hexing" it. It is true (and suppose also lawlike) that all samples of hexed salt are water soluble. So the DN model would admit as an explanation a derivation from that law-like generalization, taken with statements describing the magician's activity, to the fact that this "hexed" sample of table salt dissolves in water. But presumably this should not count as an explanation of *any* kind, never mind a scientific one. The defender of causal explanation will say that this derivation does not count as an explanation because it does not illuminate a genuine cause, in spite of the valid inference. More subtly, my account will say that this derivation does not count as a causal explanation for exactly the reason the defender of causal explanation says. But my account will also say that the derivation does not count as a physical explanation either. (The reason, which will become clear by and by, is that the derivation does not illuminate any of the true degrees of freedom in the case.)

Finally, consider the case of an individual whom we shall refer to as Grace. Grace is good-natured. She also happens to be the daughter of a member of the city of Boston school board. And all children of school board members of the city of Boston are good-natured. Surely, however, we cannot explain Grace's good nature by reference to these facts, even if arranging them just so yields a deductively valid and sound argument. The DN model tells us why: there is nothing law-like about the generalizations contained in that argument. The causal theory has a quite a different account of the matter: it says that there are no causal relations between

Grace's good nature and her parent's membership in the school board. I should like to account for this too. My proposal comprises the other two, thus preserving the kernel of truth in each (and still others besides): it is that there are *no suitable dependence relations* between the generalizations on offer and Grace's instance.

We shall begin now with the taxonomy of dependence relations.

4. TAXONOMY OF DEPENDENCE

There is an ubiquitous type of statement in physics textbooks, of which the following is a specimen: kinetic energy *depends upon* both mass and velocity. I shall make it the ultimate goal of this section to give a clear sense to this statement. This type of statement contains, as we shall see, the kernel of the metaphysical foundations of physical theory – a metaphysical foundation that makes no use whatever of the notion of cause. And we can elucidate this type of statement without being concerned with the content of any particular physical theory. In fact, underlying this statement is the metaphysical framework upon which are erected the specifics of any given physical theory. When once this becomes clear, we shall have all the ingredients for a general account of explanation – one, moreover, upon which to build a scientific methodology.

According to statements of the general kind we wish to elucidate, a certain *quantity* depends, not on another (single) quantity, but on a *class* consisting of a collection of other quantities. It is quite a general rule that statements of dependence propose the existence of one-on-one relations, whose second term is a class comprising an unrestricted number of entities. Let us call that class, on which a dependent term depends, the class of *arguments*. And let us represent time throughout by the variable t, and its various moments by subscripted t's.

A quantity is a characteristic of the universe which may vary in *magnitude* with time, taking on no more than a single magnitude at a single moment in time. Thus a quantity is a *concretum*, metaphysically speaking, which possesses an identity through time; it is *not* an abstract object like the mathematician's variable. And this will be an important point in what follows. An event, by contrast, is the (repeatable) occurrence consisting of a certain quantity taking on a specific magnitude. We will be concerned with dependence relations among both quantities and events.

4.1. *Functional Dependence*

I will proceed with the taxonomy of dependence relations by utilizing as a springboard the brilliant analysis of logical dependence put forward by Kurt Grelling (1988). Grelling, inspired by Edmund Husserl's *Logical Investigations*, undertook in the late 1930's to taxonomize the dependence relations treated by logicians and mathematicians. (Incidentally, his work on this subject was published no less than 50 years after first being presented.) Following Grelling I begin with a definition of *functional dependence*, as a first attempt at giving expression to the textbook statement about kinetic energy. Let Ψ be a class of arguments, all with t as argument, and let X be a single quantity, also with t as argument. Then $F(X, \Psi)_t$, to be read as 'X functionally depends upon Ψ, whose common argument is t' shall be defined as follows:

DEFINITION 4.1. $F(X, \Psi)_t =_{\mathrm{df}} \forall t_1 \forall t_2 \{\forall f [f \in \Psi \rightarrow f(t_1) = f(t_2)] \rightarrow X(t_1) = X(t_2)\}$.

According to this definition**,** if, for some t_1, every argument in Ψ (that is, every argument upon which X depends) takes on the same magnitude as for t_2, then X itself must take equal magnitudes for t_1 and t_2. This definition captures, just as intended, the mathematician's definition of a function X over the set of arguments Ψ.[3] A functional dependence of the quantity X on the argument class Ψ guarantees that for each complete set of magnitudes of the argument class, there is a *unique* magnitude of X. But the reverse is not assured: there is no guarantee that for each magnitude of X there are unique magnitudes for each member of the class Ψ.

The arguments of a function are sometimes called the *independent* terms of the functional relation. With the implication that there is a *dependent* term as well as an independent term to every functional relation, and thus that we are dealing with an asymmetrical species of dependence. But asymmetrical terminology for the terms of a functional dependence relation is inappropriate, since functional dependence, as such, is neither symmetrical nor asymmetrical. In other words, just as $F(X, \{Y\})_t \rightarrow F(Y, \{X\})_t$ is not a theorem, neither is $F(X, \{Y\})_t \rightarrow \neg F(Y, \{X\})_t$ a theorem. For some functional relations entered into by two quantities are functional relations on both sides, while others are functions on only one side. The 2-sided species of functional dependence, which may be called *mutual* or *reciprocal functional dependence*, is symmetrical, for $F(X, \{Y\})_t \rightarrow F(Y, \{X\})_t$, while not generally true, will hold for the 2-sided species of functional relations. We must therefore reject asymmetrical terminology for the terms of a functional dependence relation.

4.2. *Covariation*

The magnitudes of two quantities can vary together systematically.[4] This is true as well in the example of kinetic energy and momentum. Now it would seem that the idea of covariation is not captured by the formula 'X and Y are reciprocally functionally dependent', for two quantities may be reciprocally functionally dependent even when both variables are constant in time, and even when one is monotonically increasing while the other changes wildly over time, taking on one magnitude no more than once. We might suppose it desirable to have a means of capturing covariation, so as to exclude these two problematic cases from qualifying.

Let us make the following attempt. As before, let Ψ be a class of quantities, all with t as argument, and let X be a single quantity, also with t as argument. Then $V(X, \Psi)_t$, to be read as 'X covaries with each member of Ψ, whose common argument is t' shall be defined as follows:

DEFINITION 4.2. $V(X, \Psi)_t =_{\mathrm{df}} \forall t_1 \forall t_2 \{(\exists! af)[f \in \Psi \wedge f(t_1) \neq f(t_2)] \rightarrow X(t_1) \neq X(t_2)\}$.

According to this definition, X covaries with each of the argument class Ψ when and only when for every pair of arguments t_1 and t_2 for which one and only one element of Ψ takes on different magnitudes, X takes on different magnitudes as well.[5]

Covariation, like functional dependence, is as such neither symmetric nor asymmetric; in other words, neither statement (i) $V(X, \{Y\})_t \rightarrow V(Y, \{X\})_t$, nor (ii) $V(X, \{Y\})_t \rightarrow \neg V(Y, \{X\})_t$, is a theorem. For there might be a pair of arguments t_1 and t_2 for which X takes the same value, but for which Y varies. So, once again, asymmetrical terminology for the terms of the covariation relation is inappropriate.

However the two cases discussed at the beginning of this section, which we would have liked disqualified as specimens of covarying quantities, qualify nonetheless under the definition just laid down, just as much as they qualify as specimens of functional dependence. For according to the definition of V, constant X covaries with constant Y, just as monotonically increasing X covaries with wildly varying Y if the latter takes on a given value at most once. In fact, there is even *reciprocal* covariation in these two problematic cases. However the definition V also helps reveal why we should *not* disqualify these two apparently problematic cases as instances of covariation. Just like the definition for F, the definition for V

is stated in terms of same and different magnitudes. And there will always exist *some*, though not always any *simple*, means of expressing how the difference between two quantities, which obey the definition of V, change in magnitude over time – perhaps through an equation, or some more complicated mathematical or set-theoretical mechanism for associating one object with another – which will justify applying the term *covariation*. For at the heart of the matter lies the fact that the problematic cases differ from the unproblematic ones only in degree of complexity of the covariational pattern. And the difference between *simple* and *complex* covariations is not a difference of *category*, but a difference of degree. Hence we will not be able, qualitatively, to disallow the two problematic cases – that is to say, we will not be able to disqualify these via a definition employing all-or-nothing (i.e., categorial) concepts like *same* and *different*.[6]

Grelling writes that $V\,(X, \Psi)_t$ is in a certain sense the converse of $F\,(X, \Psi)_t$, since the following can be proven:

THEOREM 1. $V\,(X, \{Y\})_t \equiv F\,(Y, \{X\})_t\,.$

Theorem 1 states that X covaries with Y whenever Y is functionally dependent upon X. Theorem 1 entails:

THEOREM 2. $V\,(X, \{Y\})_t \wedge V\,(Y, \{X\})_t \equiv F\,(X, \{Y\})_t \wedge F\,(Y, \{X\})_t\,.$

Theorem 2 states that X and Y covary reciprocally under exactly the same conditions as they are reciprocally functionally dependent. This fact – that functional dependence and covariation overlap in their reciprocal varieties – is further justification for classifying F and V as species of the *same* genus of dependence, which Grelling has with good reasons entitled *logical dependence.*

Combining F and V we may define:

DEFINITION 4.3. $FV\,(X, \Psi)_t =_{\mathrm{df}} F\,(X, \Psi)_t \wedge V\,(X, \Psi)_t\,.$

Grelling shows that if X stands in the relation FV to Ψ, and one succeeds in keeping constant all the elements of Ψ but Y, then a strict correlation (which we are entitled to think of as a species of equivalence relation) will hold between X and Y. Thus we should view Grelling's achievement as that of producing a substantive answer to the (metaphysical) question: What does a correlation consist in? Answer: a confluence of functional dependence and covariation.

Now, if two quantities stand in the relation FV to one another, this might be evidence that one "reduces" to the other in some important sense of this term. But which to which? After all, nothing prevents FV being symmetrical, while the notion of reduction presupposes an asymmetry. To answer this question, we shall have to proceed with the enterprise of taxonomizing dependence relations beyond the point – this point – where Grelling left it off.

4.3. *Physical Dependence*

We have defined the relations F and V for quantities, using definitions Grelling intended for variables (the mathematicians' abstract objects which are capable of taking on more than one *value*, though never more than one at once) – since everything that goes for variables can go for quantities too. When *quantities* enter into any of relations F, V, or FV, they can conveniently be represented by *variables* that stand in the corresponding relation.

But there is a third species of dependence relation which physical quantities can enter into, but which variables cannot. (As will become clear, this fact is partly due to the fact that variables are abstract while quantities are concrete.) This further dependence relation is not a formal, mathematical or logical relation; it is not, in other words, a relation having to do simply with how magnitudes – marks on a given scale, that can be compared only as to which is greater – vary over time. Rather it is a *physical* relation of dependence. The correlative, and more basic, notion is that of physical *in*dependence: it is the notion of *degree of freedom*. The idea is this: some quantity X is a *physically independent* quantity whenever it is among those quantities whose magnitudes *shape* the state of a system to which they belong, and it is *physically dependent* when it is *given shape to* by other quantities. The textbook statement, to the effect that kinetic energy depends on momentum, should be read as stating also that momentum gives shape to kinetic energy, whereas kinetic energy does not give shape to momentum. If this is right, then shaping is an asymmetric relation, and cannot coincide with either functional or covariational dependence. I will treat the concept of shaping as a primitive, and suppose it to be governed by the following axiom:

AXIOM 1. If a quantity X shapes the state of a system σ, or if it gives shape to a quantity Y of system σ, then it is false that X is given shape to by any other quantity.[7]

On my account, shape-giving is both immediate (i.e., not mediated) and irreflexive. What's more, it's absolute, not comparative or relative to any purpose or any so-called "level of explanation."

Practicing physicists use the terms ‘independent quantity’ and ‘degree of freedom’ as variants (just as I am here), but they formulate the idea in epistemological terms: X is an independent quantity or degree of freedom, they will say, whenever X is among quantities whose magnitudes must be specified in order to specify completely the state of the system to which they belong.[8] However formulation of the conception indirectly, in the epistemological mode, will not do for purposes such as ours, because we are examining the metaphysics of the idea. For an indirect formulation such as this cannot distinguish between (for example) physical and functional dependence. And so it might inadvertently lead to error, or (worse) to confounding two dependence relations, as (logically speaking) what may serve for complete specification of a system may not necessarily coincide with what gives shape to that system. I therefore urge adoption of the following definition of physical independence:

DEFINITION 4.4. X is a *physically independent* quantity or a *degree of freedom* of a system $\sigma =_{\text{df}} X$ is among those quantities whose magnitudes shape the state of σ.

Physical dependence should subsequently be regarded as the privation of physical independence, as follows:

DEFINITION 4.5. X is a *physically dependent* quantity of a system $\sigma =_{\text{df}}$ X is *not* among those quantities whose magnitudes shape the state of σ.

Different theories may assign the role of degree of freedom to different physical quantities. So we may say that

DEFINITION 4.6. X is a physically independent quantity or degree of freedom of a system σ *according to a theory or scheme of representation* $T =_{\text{df}} X$ is named or otherwise designated by T as belonging among those quantities whose magnitudes shape the state of σ.

There is therefore a clear sense to the question: which (if any) theory or scheme is correct in its designation of the degrees of freedom? And there is a clear sense too to the question, often answered in the negative: is there a unique set of degrees of freedom for every system?[9]

It pays to lay heavy stress, starting now, on the point that the conception of physical dependence just defined does not coincide with the conception of functional dependence. To begin with, the former is not strictly speaking a *relation* among quantities, but a property which belongs to quantities individually (albeit a property whose manifestation requires that the quantities to which it belongs be related to other quantities, and whole systems, in certain ways). However, there is a closely allied relation – I propose that we call it *physical foundation* – into which physical quantities may enter, which can be defined in terms of the property of being a degree of freedom. I will give only a necessary condition for this:

PROPOSITION 4.7. *Y is physically founded upon $\{X_i\}$ in σ* (abbreviated '$P(Y\{X_i\})_\sigma$') only if the following two conditions hold: (1) all of the members of $\{X_i\}$ and Y belong to σ, and (2) each of the members of $\{X_i\}$ is a degree of freedom of σ, but Y is not.

It follows from this necessary condition that physical foundation is (as required) an asymmetrical relation. It also follows that physical foundation is irreflexive and immediate, as shaping is both. (Note: the converse of Proposition 4.7 is false, since when each member of $\{X_1\}$ is a degree of freedom but Y is not, it does not follow that Y is founded on X_1, for it might be that Y is founded on some third quantity which gives it shape, and is not given shape to by X at all. Hence we have not presented sufficient conditions for physical foundation in terms of shaping.)

Are all forms of physical foundation those of quantities on quantities? No, for we shall in what follows have need to speak of certain *events* too as being physically founded on others. I propose to extend the definition of physical dependence to events as follows. Consider the set of events which consist of quantities taking on definite magnitudes. Let us say that

DEFINITION 4.8. *Event E_Y^y*, consisting of quantity Y taking on magnitude y at a given time, is *functionally dependent* upon the set of events $\{E_{A_i}^{a_i}\}$, consisting of quantities A_i taking on magnitudes a_i at certain other times $=_{\mathrm{df}}$ $F\ (Y, \{A_i\})_t$.

And we will say that

DEFINITION 4.9. *Event E_Y^y* (consisting of quantity Y taking on magnitude y) is *physically dependent upon* the event E_X^x, consisting of quantities X taking on magnitudes x at certain other times $=_{\mathrm{df}}$ $\exists\sigma\, P(YX)_\sigma$.

Applying the latter definition to the standard case of physical foundation we have been exhibiting, the kinetic energy of body B taking on magnitude K, which is an event, is physically dependent upon that body's momentum taking on the magnitude P, which is again an event.

Now perhaps it will be said that the notion of physical foundation I have just defined, with its applications to both quantities and events, is nothing other than the notion of *supervenience,* according to which we can divide the world of quantities into two categories so that the following condition holds: once quantities in category A have been invested with magnitude (at a given time), the magnitudes of quantities in category B are uniquely established (at that time); in other words, there is no "freedom" in choosing magnitudes for quantities in category B, once the magnitudes of those in category A have been chosen. Supervenience, according to its friends, was to be the instrument that would help us avoid dualism and reclaim materialism, without falling victim to any of the evil reductionisms. J. Kim (1993, p. 147) writes: "Much of the philosophical interest that supervenience has elicited lies in the hope that it is a relation of dependence; many philosophers saw in it the promise of a new type of dependence relation that seemed just right, neither too strong nor too weak, allowing us to navigate between [the Scylla of] reductionism and [the Charybdis of] outright dualism".

But the problem with supervenience (defined as once quantities in category A have been invested with magnitude, the magnitudes of quantities in category B are uniquely established), whose prospects were once upon a time so bright, is that it gets us no further than any of the logical species of dependence relations introduced by Grelling for variables, and reintroduced here also for quantities, because it does not move beyond treating dependence relations in terms of differences and samenesses of magnitude. (And this is true whether or not we decide to handle all the merely potential magnitudes, in addition to the actual ones.) And so supervenience, as actually defined by its patrons, is no more an asymmetrical relation than functional or covariational dependence are. (This point was made some time ago by Grimes (1988), but it has not deterred too many philosophers from use of the notion as if it were an asymmetrical one.) Thus the notion of supervenience has not lived up to its promise. I suggest that supporters of supervenience had in mind something more like my notion of physical foundation, but did not have resources for articulating it, as they did not suspect that the conceptual foundations of physics itself could help with formulating materialism. (This fact is rather ironic, for physics was *not* to be the model for the sciences for which antireductionists hoped to make room.)

In our present account physical dependence is defined in terms of shaping, and the latter is taken as primitive. Are we making progress, or merely spinning wheels? Shouldn't we rather be disappointed that the explication of physical dependence itself depends on something that is left unexplicated, and might as well be called – to use a naughty, because polymorphous, term – causation? Two points in reply. First, the aim of the present account is less to make progress towards a unified account of the world as it is, than it is to articulate concepts, as clearly as possible, which can be put to describing the world as it might be. In other words, the aim of my account is less to describe the world as it is, and more to fashion instruments for describing the world, however it might be. And of course one has to have primitives, somewhere. The unavoidable reason for having shaping come in primitively, is that covariation ideas alone can't suffice, as I have been at pains to argue, and as efforts to define a satisfactory notion of 'supervenience' have shown, better just cannot be done. Second, I claim that the instruments I am aiming to fashion are precisely those on which physics, as a discipline, has staked its claim. If I am right, the concepts I am articulating belong to physics itself. So if the description of the world using these concepts is unsatisfactory, this failure does not belong to the present account, but rather to physics itself, as a discipline. And if physics itself is in fact unsatisfactory, for these reasons, it is at least unsatisfactory differently from the way a causal account of the same matters might be unsatisfactory, for as I shall show, physical dependence and causal dependence are quite different things, and should not be confused. So if physics is unsatisfactory (as I am unwilling to grant), we shall nonetheless have a deeper understanding of why it is so.

Is physical foundation a relation into which enter the objects of experience and common sense? This question is beyond the scope of our enterprise, as we are not here dealing with objects *at all,* never mind the objects of common sense or experience. The physical sciences – I hazard to say the natural sciences as a group – treat purely of quantities. Objects, as such, never make an appearance, but are only hinted at (in applications of theory to true-life systems) by intuitive and unstructured groupings of quantities, that are officially unstructured and ungrouped. Systematic reconstitution of objects from quantities is left, unceremoniously and without even a word of thanks, to other disciplines, such as perhaps philosophy.[10]

4.4. *Some Important Relations between Functional Dependence and Physical Foundation*

Does the proposition, that quantity Y is functionally dependent upon the class consisting of a single quantity $\{X\}$, in a system σ, entail the proposition that Y is physically founded upon X? No.

Proof. As before, let '$P(YX)_\sigma$' stand for 'quantity Y is physically founded upon quantity X in system σ' and '$F\,(Y, \{X\})$' stand for 'quantity Y, of system σ, is functionally dependent on quantity X, also of σ'. Suppose for purposes of *reductio* that $\forall_{X \neq Y}[F(Y, \{X\}) \mapsto P(YX)_\sigma]$. Then $\neg P(YX)_\sigma \mapsto \neg F(Y, \{X\})$ and $\neg P(XY)_\sigma \mapsto \neg F(X, \{Y\})$. But because

P is an asymmetrical relation (see penultimate paragraph of last section), so $P(YX)_\sigma \mapsto \neg P(XY)_\sigma$. So $F(Y, \{X\}) \mapsto \neg P(XY)_\sigma \mapsto \neg F(X, \{Y\})$. Hence $F(Y, \{X\}) \mapsto \neg F(X, \{Y\})$. However this is false in many instances, in particular those in which X and Y are mutually functionally dependent. The hypothesis must therefore be denied: functional dependence does not entail physical foundation.[11]

But is the converse true? Does the proposition that Y is physically founded upon X entail the proposition that Y is functionally dependent on $\{X\}$? Surprisingly, the answer to this question is also no. The case in which Y is physically founded upon X, and also physically founded upon a third quantity Z, is sufficient to prove the point. For this is consistent with Y covarying with Z, and X remaining constant. In such a case Y will not be functionally dependent on X, since there will be many magnitudes of Y to the same magnitude of X. In such a case, however, Y will be functionally dependent on the argument set $\{X, Y\}$.

A more difficult, but nevertheless important question is: does the proposition that Y is physically founded upon X, and not on any other quantities, imply that there is functional dependence of Y on $\{X\}$? I propose to answer this question also in the negative. The reason: it is logically possible that a quantity Y is physically founded upon a quantity X, and on no other physical quantity, but that Y, while restricted in what magnitudes it can take on by the magnitude of X, does not have a particular magnitude "forced" upon it. In other words, the magnitude of X may constrain the magnitude of Y to lie within some range, but not demand that Y take on a *unique* magnitude within that range: the magnitude of X shapes the magnitude of Y, but only up to a point. So on different occasions when X takes on the same magnitude, Y may take on different magnitudes. Hence there is no functional dependence of Y on X. Someone might challenge the sense of talking about such a case as one in which there is true physical foundation, and would therefore substitute the negative answer given here with a positive one. Even so I am prepared to call this a case of physical foundation, since there is a perfectly good sense in which the magnitude of X may be said to make a contribution towards shaping the magnitude of Y. When it comes to shaping activities, a case in which Y is physically dependent on X, and on no other quantities, but is not functionally dependent on X, is no different from a case in which Y is shaped *in part* by X and in part by something else, say Z; in that case we are prepared to say that Y is shaped by X but not entirely. And I think we should say exactly the same thing about the case in which Y is physically dependent on X, and on no other quantities, but is not functionally dependent on X. All of this is of course consistent with its being true that *in the preponderance of cases* – what I will refer to as *representative* cases – physical dependence is accompanied by functional dependence.

Consider now that case in which functional dependence is present between two quantities, but there is no physical dependence between them. We may ask: how do these cases differ from those in which the quantities are *both* functionally *and* physically dependent? More specifically, we wish to have an answer to the following question: given a case in which a quantity Y is both physically *and* functionally dependent on a quantity X, is there a third relation between X and Y which is such that both (i) it is entailed by the relation of physical foundation of Y on X, and also that (ii) it is *not* entailed by functional dependence of Y on X? If there is such a relation, then this may be what is is added (at least in representative cases) to a case of functional dependence to bring it "up" to a case of physical dependence. It is this relation which is *missing* in cases where functional dependence but not physical dependence exists between two quantities. This third relation must admit of asymmetrical instances, since physical dependence is asymmetrical but functional dependence is not.

4.5. *Ontological Dependence*

Consider the following relation, which has been alleged to hold between a work of art and its audience (cf. Smith 1988), as well as between God and her creatures:

DEFINITION 4.10. An entity A *ontologically depends upon* an object B wherever A cannot remain in existence unless B also exists.

I shall adapt this new idea of dependence (or, as I should perhaps say, old idea) in the service of formulating the missing relation discussed at the conclusion of the previous section.

I shall say that

DEFINITION 4.11. A quantity Y *ontologically depends upon* a quantity X– abbreviated '$O(YX)$' – just in case any state of affairs in which Y takes on some magnitude or other (but no specific one) is by necessity a state of affairs in which X takes on some magnitude or other (but no specific one).

And so it appears that ontological dependence is yet another dependence relation in which kinetic energy stands to momentum, for nothing can be possessed of a kinetic energy without some or other of its parts being in states of motion.

The example of kinetic energy and momentum might suggest that ontological dependence between quantities is always symmetrical, for the converse is also true in that case: nothing can be in a state of motion without also possessing a kinetic energy. I reply that ontological dependence might be asymmetrical. Consider the relationship in which temperature stands to momentum. No system can exhibit a temperature unless some or other of its parts are in states of motion. It does not follow, however, that a subatomic particle in motion exhibits a temperature. So ontological dependence between quantities may be either one-sided or reciprocal.

I now claim that physical foundation – at least on those occasions in which it is manifested together with functional dependence – is one part functional dependence and one part ontological dependence. Physical dependence, under most circumstances, is a combination of functional and ontological dependence:

CONJECTURE 4.12. $\forall\Psi\{[X \in \Psi \wedge F(Y, X)_t \wedge P(YX)_\sigma] \longrightarrow [P(YX)_\sigma \equiv F(Y, X)_t \wedge O(YX)_\sigma]\}$.

I do not know how to prove this conjecture.

5. THE PROPOSITION

And so it would appear that physical theories – if they can be said to boast a cast of characters which includes both physically dependent and physically independent quantities – are much more than mathematical formalisms or equations; for the latter are at best statements of functional and covariational dependences, and cannot capture the idea that there are physical dependences in the world. Equations, as such, cannot mark a certain set of quantities as founding and another set as founded.We must therefore be utilizing auxiliary hypotheses when we move from mere equations to full-blown theories that apply to actual physical systems. Hence *applications* of the equations, in the form of full-blown physical theories, to particular systems in particular contexts, will presuppose the designations which are missing from the equations as such. This is one way of putting the result of the last section.

There is another, equivalent way. This is to say that physical theories, which consist of equations *purely*, are expressions of some combination of functional and covariational dependence relations, and that a specific *application* of a physical theory, in a particular context, presupposes or asserts much more than the existence of logical dependences. On this way of putting it, it is *application* of theories, and not theories themselves, which presuppose some scheme of dependent and independent relations amongst quantities. It is purely a matter of semantics – of the semantics of the term 'physical theory' – which way of putting things one likes better. And whichever way one likes to put it, the result is the same: a physical *explanation* – as such (which, if we like the first statement of our results, is the expression of a theory, and if we like the second statement it is the application rather than the theory itself) – is the illumination of a physical dependence, not the illumination of a causal dependence. This is my proposal, then, that physical theories do not explain by directing attention to causes. Instead, they explain by directing attention to physical dependence relations – combinations of functional and ontological dependence relations. And generalizing from this point, scientific theories explain by directing attention to the appropriate dependence relation.

This proposition – that scientific theories explain by directing attention to an appropriate dependence relation, and that there exists a space of dependence relations that is itself open to philosophical scrutiny *a priori* – explains, amongst other things, why mathematics is so strikingly useful in the sciences, and in physics especially. For the dependence relation which physics is concerned with illuminating, is a combination of two less complex dependence relations, one of which falls within the domain also of the mathematician. Thus this fact – that the subject matters of physics and mathematics overlap – itself illuminates in particular the relationship between physics and mathematics, and sheds light on the usefulness of mathematics to science (physics in particular): mathematical analysis is in a position to explore territory that physics too must cover. We can say also that *applied* mathematics is the exercise of mathematical tools in explanation of territory that belongs both to mathematics and to another science.

Perhaps it requires explaining why philosophers have yet to take notice of the distinctions amongst dependence relations that I am now making, particularly between physical and causal dependence. I offer the following by way of explanation. Past interpreters of physical science have been occupied in one of two ways: they have been either (1) focused upon rendering the contents of a specific physical theory intelligible through translating mathematical statements (those of Newtonian mechanics, say, or quantum mechanics) into ordinary language; or (2) they have been wholly agenda-driven, for example focused upon interpreting physical theories within a causal idiom. As the first phase of this investigation now reveals, there is nothing wrong with the enterprise of those who fall under category (1), but their results will fuse together the contents of a specific physical theory with the metaphysical presumptions of all physical theories. However, unlike the enterprise of those who fall under category (1), the enterprise of those who fall under category (2), is misguided, as it presupposes – incorrectly as now I will undertake to show – that physical theories aspire to explain by illuminating causes.

6. CAUSAL DEPENDENCE, IN A CLASS OF ITS OWN

Ever since Hume there has been enormous controversy over how to illuminate the conception of cause. I shall not – for I need not – argue for or against any approach to illuminating this vexed conception. I have taken that up elsewhere.[12] All I propose here is to show that physical dependence (as manifested by events) is not a specimen of causal dependence, nor a genus under which causal dependence falls. And this argument requires no doctrinal allegiance – no allegiance of any kind – to a particular approach to the elucidation of causation. All that is required is the proposition that causal relations admit of common causes, and therefore that my arguments shall be acceptable no matter which approach to elucidating causation you prefer.

Thus I ask you to accept, as part of your account of causality, the principle I shall call *common cause possibility:* It is possible that events of type A and B, which as it happens cooccur at better than chance, might have a common cause in an event of a third type C. The principle says that when an event of type A cooccurs with an event of type B, that this can be so because they are both effects of an event of type C, which causes each of A and B separately. And thus that under such circumstances A and B are not related as cause and effect. (As an example, suppose that smoking and heart disease cooccur at better than chance. Then the common cause possibility will say that this can happen when the two are related as common effects of some third thing – some genetic factor, perhaps.) The common cause possibility principle is to be construed weakly, as not being committed to the existence of a third thing in each such instance, only to the possibility that a third thing can cause both under suitable circumstances. This proposition, I think, all accounts of causality are prepared to accommodate. And so now to show that physical dependence is not a specimen of causal dependence, nor a genus under which causal dependence falls.

Physical dependence is a specimen of causal dependence only if physical dependence implies causal dependence. But it does not, for physical dependence is, at best, a combination of functional dependence and ontological dependence. And neither, nor are they together, sufficient for causal dependence. The standard case of physical dependence which served earlier will also serve very well to illustrate this point. The event E_K^k, consisting of the kinetic energy of a given body coming to take on magnitude k, is physically dependent upon the event E_P^p consisting of that body's momentum coming to take on the magnitude p. It does not follow, however, that E_P^p causes E_K^k. For the two might be effects of a common cause, for example an event that takes place prior to the time at which both occur. (This is just a direct application of the *common cause possibility* principle.) In other words, E_P^p and E_K^k might cooccur, but it does not follow that the one causes the other, since they might be effects of a common cause. In fact, isn't this the typical case?: those who take E_K^k to be caused, will take it to be caused by something besides E_P^p, and they take E_P^p to be caused by that very same something that causes E_K^k. So physical dependence is not a specimen of causal dependence.

Next, causal dependence is not a specimen of physical dependence either, because physical dependence implies ontological dependence, while causal dependence does not. Consider the process consisting of this billiard ball moving from here to there along some surface, with a constant momentum. Those who go in for causal explanation might say that the ball's departure from here with the certain momentum, stands in the relation of cause to its arrival there with the same momentum, since (according to these fans) a billiard ball in motion is a causal process *par excellence*. Now if causal dependence is a specimen of physical dependence, it should follow that the momentum of this billiard ball is founded on itself. But this statement is false, because physical foundation is irreflexive (by Definitions 4.9 and 4.7). So no self-respecting theory of causality should embrace the proposition that the momentum of a body is founded on itself.

Perhaps it will be replied that one *could* offer a definition of physical foundation which, unlike mine, is not irreflexive, and which would nonetheless serve the purposes of physical theory equally well. That may well be, but a move in this direction will not help to salvage the proposition that causal dependence is a specimen of physical dependence. A second example will make this point clear. Consider two billiard balls approaching one another at an oblique angle, colliding and eventually separating again. Let the magnitude of the momentum of the first ball be p^b before collision, and its magnitude p^a thereafter; let the magnitude of the momentum of the second ball be q^b before collision, and its momentum q^a thereafter. Let $E_P^{p^b}$ and $E_P^{p^a}$ represent, respectively, the events of P – the momentum of the first ball – taking on magnitudes p^b and p^a, and *mutatis mutandis* for $E_Q^{q^b}$ and $E_Q^{q^a}$. Causal theorists wish to say that $E_P^{p^a}$ and $E_Q^{q^a}$ are effects of causes $E_P^{p^b}$ and $E_Q^{q^b}$, and therefore that $E_Q^{q^a}$ is (in the singular) an effect of causes (in the plural) $E_P^{p^b}$ and $E_Q^{q^b}$. Now if causal dependence were a specimen of physical dependence, it should follow that (quantity) Q is founded in part on (quantity) P, by Definition 4.9. But this is false (as the momenta of unattached billiard balls are ordinarily taken as independent degrees of freedom), so no self-respecting causal theory should embrace the statement, even if we are prepared to say that Q is founded on itself (for purposes of accommodating reflexivity in the relation of physical foundation). Thus causal dependence cannot be a specimen of physical dependence.

It follows that causal dependence and physical dependence, as manifested between events, are independent relations. And that causal dependence is in a class of dependence relations all its own, unrelated to the other species of dependence relations (logical and ontological, with physical dependence being a combination of the two) we have here surveyed. Since physical and causal dependence are unrelated, it follows that physical theories which distinguish between dependent and independent quantities are not, as such, causal descriptions. So if it so happens that the sequences of episodes which a certain physical theory can account for, are in conformity with causal expectations, they are in conformity by a happy coincidence, and not because the physical theory in question achieves or even aspires to the goal of causal explanation.

7. PHYSICAL EXPLANATION

The common cause principle due to Hans Reichenbach states that explanation of better-than-chance correlations between spatially separated events, when no causal influence passes across the separating expanse from one of them to the other in the time elapsed between their occurrences, must proceed by illumination of a common cause. If the common cause principle, unrestricted, belongs to the theory of causality, then, as John Bell proved, certain extremely well-documented and extraordinarily robust correlations – known among philosophers as "EPR-type correlations" because they came to philosophical attention in the course of a debate sparked by a paper by Einstein, Podolsky and Rosen – cannot be explained by illumination of causes.[13] A number of reactions to Bell's discovery are currently on the market:

1. Any account of causal explanation, like Reichenbach's, that puts the common cause principle unrestricted at the center of the theory of causality, requires substantial reform (Sober 1988; Redhead 1987).

Specifically, the common cause principle cannot occupy a place of honor in the account of causal explanation, much less the center.

2. The common cause principle requires restriction in scope. Causal explanation of correlations of which the EPR-type are a special species, normally does proceed by illumination of common causes, as ordinarily causation is "local", but it may take other forms under certain special conditions since there may be forms of "nonlocal" causation (Cartwright 1989; Forrest 1997).
3. EPR-type correlations are explainable by illumination of causes, consistent with the demands of the common cause principle unrestricted, but we must for allow backwards-in-time causation (Price 1994; Dowe 1996).
4. EPR-type correlations are positively unexplainable (suggested by some passages in Salmon 1989).
5. EPR-type correlations do not demand explanation, much less explanation of a causal nature (Fine 1989).

I am hereby proposing to add one more to this already substantial, but nonetheless unappealing menu of options: I affirm that EPR-type correlations call for explanation. And moreover that to explain them *according to the ideal of causal explanation* is to call on a common cause. My proposal is that it is not our account of *causality* that stands in need of renovation, but our account of *explanation,* and its varieties. In particular, it is our account of how physical theories explain, which requires a new treatment, at which we have made a beginning in this essay already, and will now endeavor to explicate at greater length.

7.1. *The Goal of Physical Explanation, and How it Differs from the Alternative*

I have proposed that to explain *in physics* is to illuminate *physical* dependence relations. This idea is in harmony with the more fundamental one that to explain, unqualified, is to illuminate a dependence relation of some sort or other. The contrasting proposal to mine – to the effect that to explain a physical event is to illuminate a cause – is not in obvious harmony with the idea that logicians and mathematicians explain, in the same sense of the term 'explain'.

Now the proposal that to explain a physical event is to illuminate a cause, rests upon the proposition that explanation requires drawing attention to certain facts and factors occurring at a "higher" point in the causal process. What, by contrast, does explanation by illumination of a physical dependence rest on? It rests on the proposition that, for one reason or another, or several in combination, there is a finite number of degrees of freedom in the universe, and that this fact leads to there being systematic correlations (functional dependences) among quantities. In general, suppose the number of degrees of freedom in some system is N, and that we can designate – possibly not in an absolute or unique fashion – the quantities $Q_1, \ldots, Q_N$ as the degrees of freedom. Correlations will therefore be manifested between the magnitudes of these N quantities, and those of any quantity which is not among these N. (The specific details of how these correlations manifest themselves is, of course, the job for a specific physical theory to disclose.) More pedantically, since $Q_1, \ldots, Q_N$ exhaust the degrees of freedom, there will be no "freedom" in choosing magnitudes for the remaining quantities once $Q_1, \ldots, Q_N$ have taken on magnitudes. We can see this as follows. Let's write '$\neg PI(Q_i)$' as an abbreviation for 'Q_i is not physically independent' – or, in other words, Q_i is not a degree of freedom – and let $P(E_{Q_i}^{q_i})$, or just $P(E_{Q_i})$, stand for the probability of the event that Q_i takes on the magnitude q_i. Then the following will hold in representative cases, since physical dependence is normally accompanied by functional dependence:

PROPOSITION 7.1. $\neg PI(Q) \longrightarrow P(E_Q | \bigwedge_{i=1}^{N} E_{Q_i}) \in \{0, 1\}$.

The antecedent of this proposition will be satisfied when Q does not belong among $Q_1, \ldots, Q_N$. And when the consequent of the proposition is met, we are sure to encounter correlations of the sort that – as supporters of causal explanation are right to insist – call for explanation. How do these correlations come about? The quantity Q_{N+j} (with $j > 0$), because it is not among $Q_1, \ldots, Q_N$, cannot vary independently. So it must obey $P(E_{Q_{N+j}} \wedge \bigwedge_{i=1}^{N} E_{Q_i}) \neq P(E_{Q_{N+j}}) \times P(\bigwedge_{i=1}^{N} E_{Q_i})$. (This, by the way, is true whether or not we are speaking of the representative cases in which physical dependence is accompanied by functional dependence. See section 4.4.) For Q_{N+j} may vary independently of $Q_1, \ldots, Q_N$ only on condition that $P(E_{Q_{N+j}} | \bigwedge_{i=1}^{N} E_{Q_i}) = P(E_{Q_{N+j}})$, which is true *only* if the proposition above is false – only if Q_{N+j} is a degree of freedom – which by hypothesis it is not.

Just as Reichenbach's common cause principle lays the ground for a quantitative analysis of (partial) causes, Proposition 7.1 opens up the way for physical explanation in the probabilistic case – a way we shall travel in the next section when we take up explaining EPR-type correlations.

Someone might complain that my proposal is not in keeping with the actual practices of physicists, who are famous for designating all manner of quantities as degrees of freedom, and then proceeding to say that there are systematic correlations among them which come about as a result of physical laws, such as for example the conservation of something or other. Thus we cannot think of degrees of freedom in the way I am suggesting – as independent modes or sources of variation. Take for example the case exhibited in section 6 of the two billiard balls colliding. Physicists will tell us that there are systematic correlations between the momenta of these two balls, while at the same time saying that the momenta are nevertheless degrees of freedom in the system the balls comprise.

I reply that the challenger's is an incorrect interpretation of what the physicists have in mind, which is more along the following lines. The physicists are saying that we can suppose, *a priori,* that there are at least *eight* degrees of freedom present in a system consisting of two billiard balls confined to move in a plane: two degrees to each ball for its displacement from a designated point, and two each for momentum in the two-dimensional plane. But as a matter of fact the balls do not exhibit that many degrees of freedom, since they are under obligations to comply with the following (four) restrictions: they are to conserve momentum in each of two linear directions, as well as conserve energy, as well as remain confined within the boundaries of the table. Effectively, this reduces the number of degrees of freedom, computed *a priori* to be eight, to a meager *four* (or somewhat less depending on how we like to count the last of these restrictions).

But all of this is consistent with my proposal that degrees of freedom are independent modes or sources of variation in a physical system. For with each of the restrictions named (each of the conservation laws or boundary conditions) comes a certain type of restriction in the combined magnitudes of the quantities exhibited by the system, and so a potential for correlations. So, for example, with the restriction to conserve energy comes the correlation that the energy of one ball must be equal to the amount of energy it is obligated to maintain less the energy of the other ball; with the restriction that linear momentum be conserved comes the correlation that the linear momenum of one ball in a specified direction must be equal to the amount of linear momentum it is obligated to maintain (in that direction), less the linear momentum of the other ball in that same direction; with the restriction that the balls remain on the surface of the table comes the correlation that neither ball is ever outside these bounds; and so on.

In summary, the proposal that to explain a physical event is to illuminate a physical dependence is in agreement with its competitor, that to explain a physical event is to illuminate a cause, on the point that better-than-chance correlations among events call for explanation. Some supporters of causal explanation – notably, those who adhere to the common cause principle descended from Reichenbach – propose to explain better-than-chance correlations by identifying those conditions or events which make the better-than-chance correlations invisible. It is the making of better-than-chance correlations thus invisible that friends of the common cause principle call by the name explanation. This conception of explanation is only as firm as the notion that the only contrary to the explainable is the random, which presupposes a very coarse division of things, into explainable and unexplainable. I am proposing a much more fine-grained division of the explainable, that admits of overlapping categories. On my proposal there must exist equally many ways of being explainable as there are ways of being dependent, and these ways can overlap. And so as many ways in which something can cry out for explanation.

As I now propose to show, correlations of an EPR sort cry out for explanation in a number of ways – at least two, anyway: physically and causally. I will show that it is possible to respond to the cry for physical explanation successfully, whilst it is not possible to produce a causal explanation successfully. For the two are completely independent goals. So if certain correlations cannot be explained causally, this does not rule out their being explained physically.

7.2. *EPR Correlations, and How Quantum Mechanics Explains Them*

A certain example, selected for purposes of exhibiting quantum theory's ostensibly revolutionary character, concerns the spins of particles in the so-called "singlet state." This same example can serve to illustrate the ordinariness of quantum mechanics, as physical theories go. We measure the spins, in a variety of directions, of pairs of separated particles, originally produced together at a common site in the singlet state, and set in flight in opposite directions. Let S^L_θ represent the spin of the partile in the "left" wing of the setup, in the direction θ transverse to the particle's line of flight, and let $S^R_{\theta'}$ represent the spin of the particle in the "right" wing of the setup, in the direction θ' transverse to that particle's line of flight. Experimentally we determine that:

1. each of S^L_θ and $S^R_{\theta'}$ can take on only one of two magnitudes; I will say either $+1$ or -1;
2. the relative frequencies of each of : (a) $S^L_\theta = +1$; (b) $S^L_\theta = -1$; (c) $S^R_{\theta'} = +1$; and (d) $S^R_{\theta'} = -1$; is $\frac{1}{2}$;

3. the relative frequencies of (a) and (c) together, or (b) and (d) together, when $\theta = \theta'$, is zero;

and – surprisingly, to those who adhere to the standards of causal explanation –

4. the relative frequencies of (a) and (c) together, or (b) and (d) together, when the angles are unrestricted, is $\frac{1}{2}\sin^2\left(\frac{\theta-\theta'}{2}\right)$; similarly the relative frequencies of (a) and (d) together, or (b) and (c) together, is $\frac{1}{2}\cos^2\left(\frac{\theta-\theta'}{2}\right)$.

J. S. Bell showed that these experimental facts are inconsistent with the proposal that there is a common cause explanation of correlations between distant events. In particular, he showed that the only common-causal explanation of (3) above is inconsistent with (4). (This is because (4) is inconsistent with the common-causal assumption that $P[(a) \wedge (c)|\lambda] = P[(a)|\lambda] \times P[(c)|\lambda]$ – which states that S^L_θ and $S^R_{\theta'}$ are statistically independent once contributions from the (unknown) common factor λ has been accounted for – which is the only common-causal way of explaining (3).[14])

Thus supporters of causal explanation are put in a position to diagnose a demand for explanation, but are at the same time unable to meet that demand, if they adhere to the common cause principle, because their explanatory goals would violate the empirical facts.

Supporters of the proposal that to explain these correlations is to call on physical dependence relations, agree there is a need to explain EPR correlations. But they, by contrast with the friends of causal explanation, have a proposal for explaining them: the correlations (3) and (4) are due simply to the fact that between the quantities S^L_θ and $S^R_{\theta'}$ there is only one degree of freedom, which results in both constraints on the magnitudes of S^L_θ and $S^R_{\theta'}$. In other words, there is a reduction in the degrees of freedom of a system such as the one being considered, due for example to a conservation law, in such a way that S^L_θ and $S^R_{\theta'}$ have only one degree of freedom between them. This fact results in the sum of S^L_θ and $S^R_{\theta'}$ vanishing to zero when $\theta = \theta'$, *and also* in the correlation presented in (4). And here the physical explanation ends.[15]

In contrast with the goal of causal explanation enunciated in the common cause principle, the goal of physical explanation does not put restrictions of any kind on the kinds of correlations that may be manifested between quantities. Therefore it gives physical theories much more latitude in the kinds of correlations they can accommodate. Thus those who embrace the goal of physical explanation need make no *empirical* claims about admissible correlations, in contrast with those who embrace the goal of causal explanation. Thus the enterprise that embraces the goal of physical explanation for physical events, proposes to explain simply by illuminating how the degrees of freedom in the world are distributed, how reductions in number of degrees of freedom are made, and giving the details of how these facts result in observable functional dependences amongst quantities. But that enterprise does not limit the kinds of correlations that are acceptable. By contrast, an enterprise that seeks causal explanation proposes to rule out as inexplicable, by its lights, certain types of correlations – and to do so *a priori*.

Now a critic will suggest that my proposal does not offer a *new* reaction to Bell's results, but a variant on the proposal to restrict the scope of the common cause principle and look for something that deserves to be called "nonlocal causation" in those instances where it's violated. Perhaps, the friendly critic might suggest, my physical dependence relations deserve to be called *causal dependences of a nonlocal kind*. I reply that I have no objection to this characterization of my position, so long as the critic will concede that the term *causal dependences of a nonlocal kind* does not refer to a species of *causal dependence*, as my physical dependence is not a species of causal dependence (simply because neither relation entails the other). In other words, I would agree to call physical dependence by the term 'causal dependence of a nonlocal kind,' so long as we are all agreed that the latter term is really a misnomer. But what's to be gained by insisting on misleading terminology?

7.3. *Complaints and Replies*

Someone might complain that the explanation of (3) and (4) presented in the last few paragraphs, for correlations obtained in Bell-type experiments, is unsatisfactory, because it calls on (i) the fact that there is a reduction of degrees of freedom, in such a way that the quantity pair S_θ^L and $S_{\theta'}^R$ have only one degree of freedom between them, and (ii) this results in the sum of S_θ^L and $S_{\theta'}^R$ vanishing to zero when $\theta = \theta'$, and in the other correlation as well, *without explaining either of these facts*. Whereas we are held to *higher* standards by the universal demand for common causal explanation of better-than-chance correlations. Those who aspire to physical explanation are, according to our critic, settling for a lesser good than those who aspire to causal explanation.

Two points in reply. First, embracing the goal of causal explanation is not incompatible with embracing the goal of physical explanation too. (Although I, for one, favor giving only physical explanation when it comes to systems in the physical world in which no agents are involved or have practical interests.) We might, conceivably, aspire to both goals; in that instance it would be encumbent upon us to produce *both* types of explanation as the need arises. And, as already discussed, meeting one of these obligations does not in any way advance us meeting the other.

Now our critic, who holds us to the purportedly "higher" goal of causal explanation, presupposes that there is a basis for evaluating schemes or standards of explanation as to higher and lower grade. But since (as established) the two types of explanation are completely independent, in what sense could one be of better quality than the other? Possibly our critic is under the impression that a physical explanation calls on unexplained facts for purposes of explaining, whereas a causal explanation does no such thing. This is not true. For the goal of causal explanation demands illumination of a cause. And the cause to be illuminated will presumably also call for causal explanation as well. Since there is no end to further requests, except we arrive at an uncaused event, it's difficult to see how the goal of causal explanation *itself* avoids the sort of criticism targeted at the goal of physical explanation, since the obligation to explain everything cited as explanation is never met in any real-life instances of purported causal explanation. So if there's a ground for the proposition that one type of explanation is superior to another when it comes to explaining events in the world, this ground itself has yet to be illuminated.

Second, the so-called "higher" standards often involve a combination of two types of legislation: (i) a statement about what type of correlationf is admissible in the world; and (ii) a statement to the effect that the enterprise of proposing physical theories, as such, must be separate from the enterprise of explaining, since physical theories are – at best – descriptions of the physical dependences in the world. In my view, the first type of legislation is out of place in a theory of explanation, as the theory of explanation should concern itself with explaining whatever realities we might happen to run into, not with making *a priori* claims about what is admissible if we are to explain all that we shall survey. And the second type of legislation, while perhaps not exactly out of place, is nonetheless unnecessarily deflationary of the scientific enterprise, as it states that physical theories, as such, require *further* metaphysical interpretation or restriction – because they cannot be interpreted as explanations in their own right (since they are not, as such, causal explanations). I submit that these so-called "higher standards" do not deserve our allegiance at all, much less our greater allegiance.

Someone might complain that my own account of physical explanation comes uncomfortably close to asserting that we have an explanation when we have a successful theory that implies certain types of correlations, and that further demands for explanation are misplaced. I reply that I am not *merely* uncomfortably close to this assertion: I am making *this very* assertion. For my view is that physical theories are stating much *more* than that correlations will be observed, but also that correlations result from certain quantities being founded upon certain others. Statements of this sort, I have been asserting, are metaphysically substantial, and are consequently worthy of serving as the foundations of a certain type of explanation. Now this does not prohibit us seeking physical explanations also of the physical explanations, just as we are not prohibited (under the goal of causal explanation) from seeking causes of causes. But for all that the intermediate physical explanations are no less physical explanations.

7.4. *How Physical Explanation Illuminates the Status of Quantum Mechanical "Uncertainties"*

There has been considerable dispute about how to "interpret" the commutation relations of quantum mechanics, which lead to the so-called "uncertainty relations": $\triangle Q_m \triangle Q_n \geqslant c$ (some constant magnitude). (Inequalities of this form are deducible from so-called "commutation relations" into which the operator representations of certain quantities enter.) The most famous interpretation of these relations is the Copenhagen interpretation, according to which these inequalities are epistemological statements, to the effect that (either) one cannot know certain pairs of facts simultaneously, or (else) that one cannot use certain combinations of concepts (for the purpose of representing quantities) in the same system of descriptions. Recent analyses, stemming from the work of Kochen and Specker (1967), have established that the force of the uncertainty relations is not so much epistemological as metaphysical: if two quantities enter into an uncertainty relation, then (according to quantum theory) there is no admissible state of the universe in which both take on definite magnitudes. Thus that not all quantities in the universe can be magnitude-definite at the same time.[16]

How could a physical theory have such a consequence? I propose to argue that we do not require quantum theory to tell us that this is a possible consequence – that, in effect, we could have determined this as a possibility *a priori.* All we need in order to acknowledge this as a logical possibility is a clear-headed analysis of the nature of physical explanation.

Physical theories, according to my proposal, proceed in the daily business of explanation by first stating how reductions in the degrees of freedom in the world come about (for example, by identifying conservation laws) and how degrees of freedom are distributed, and then afterwards specifying the correlations we should expect to find as a result of the reductions and distributions in degrees of freedom. Suppose that two quantities,

Q_1 and Q_2, are acknowledged by a particular theory for a certain hypothetical system. Of course functions of these quantities as well, such as $Q_1 + Q_2$ and perhaps also $Q_1 \cdot Q_2$ or Q_1 / Q_2, will be acknowledged as additional quantities that will manifest themselves in this hypothetical system; let us suppose that it is appropriate to consider $Q_3 = Q_1 + Q_2$ as a quantity of our hypothetical system. Let us further suppose that there are, according to our theory, no other quantities manifested by this system, except those that are functions of Q_1 and Q_2. Suppose now that our hypothetical physical theory says that there must be *exactly* two degrees of freedom shared between Q_1 and Q_2. And suppose, finally, it also says that Q_3– the sum of Q_1 and Q_2 – must be constant (k, let's say). This makes for a certain kind of difficulty: for if Q_3 must be constant, how can there be more than one source of independent variations in our hypothetical system? Doesn't it take just one of Q_1 or Q_2 to give shape to all the quantities in the system? How can we accommodate the demand that there be exactly two degrees of freedom under the conditions just described?

Of course one solution to our difficulty is to rule out (on *a priori* grounds, naturally) the possibility that the two constraints – (1) that there be exactly two degrees of freedom; and (2) that the sum of the two quantities be constant – can fall together. This is just to say that the theory we've just formulated must be incorrect. But this is too restrictive. For there is a way of accommodating both constraints within our theory – a way which, coincidentally, quantum theory has embraced. This is simply to allow the possibility that when one quantity takes on a definite magnitude, the second must be magnitude-indefinite, and to view this situation as *not* in violation of the constraint that the sum be constant. Thus there can be two sources of independent variation in our system; for it becomes possible to give shape to one quantity in the systems without thereby giving shape to all quantities in it.

The need to accommodate the two types of conditions used as illustration in the last two paragraphs just might explain quantum mechanics' failure to accommodate value-definiteness for all quantities, and possibly also the failure to accommodate determinism – the doctrine that once all the quantities of the universe have taken on magnitude, the magnitudes of these quantities are forever afterward prearranged. The moral is that failure of magnitude-definiteness can be the price one pays for retaining a certain number of degrees of freedom. And we don't need quantum theory as such to make this point.

8. SCIENTIFIC METHODOLOGY

Ever since Hempel and Oppenheim it has been customary to divide between the methodology of science, on the one hand, and its explanatory functions, on the other. Epistemological issues, including those to do with confirmation, fall to one side of this divide – to the side of methodology – whilst broadly metaphysical, interpretational or practical issues fall to the other side. This state of affairs is due in no small part to Hempel and Oppenheim themselves, who sought an account of scientific explanation as a wholly independent affair, nothing to do either with the issue of how scientific ideas originate, or how they come to be accepted.

But it wasn't always this way. And it was not this way particularly in the hands of Aristotle. Aristotle envisioned a certain link between the subjects of explanation and methodology.[17] He thought that good science proceeds via a now-and-forever methodology,[18] by a certain hard- and possibly even impossible-to-formalize passage, from a body of experiment or accumulation of empirical observation, to explanations of – amongst other things – the phenomena thus examined.[19] This is not to say that Aristotle did not distinguish between the so-called "context of discovery" and the "context of justification." It is rather to say that he thought there were strong links between the context of discovery and what he might have called the *context of application* of a scientific proposal. On his view, the now-and-forever methodology was in part an account of explanation.

But in our own time there are (at least) two famous difficulties that beset this rather natural idea. The first is that "passage" from observation to explanation (or even just to theory) cannot mean "deduction", so that it is not entirely clear what "passage" refers to. Of course this lesson comes most powerfully from Hume and his bunkmates. And it is not to be disputed. The second is that "observation" and "experiment" cannot mean "pure observation" and "experiment alone," respectively, not least because the phenomena of error and theoretical bias are only too well known. And so, on the basis of these two indubitable observations, a large school of thinkers has concluded that there can be no now-and-forever methodology for scientific reasoning, at least none that can be prescribed in advance. The best one can do, they say, is to gesture at common sense or good judgment (in an anti-foundationalist, hats-doffed-to-Quine-and-Duhem posture).[20] In this formula "common sense" and "good judgment" are labels for that admixed portion of reasoning that appears not to be objectively justifiable in full measure – the veiled extra something that leavens experience, increasing it to full-fledged, nontrivial scientific hypothesis. Someone might attribute this no-now-and-forever position, which (amongst many other things) gets called *naturalism*, to Aristotle, even as they attribute it to Thomas Kuhn and assorted critics of methodology. I can find no grounds for the attribution to Aristotle, as this brand of naturalism severs the link between the two subjects of methodology and explanation, which Aristotle keenly viewed as sides of a certain self-same empiricist coin. Naturally I prefer to attribute to Aristotle the position on the connection between explanation and method that I am about to propound.

The no-now-and-forever brand of "common sense" naturalism causes Trouble with a capital "T" for the following reason:[21] what if "common sense" and good judgment are just plain off course, as surely they must often be vis-à-vis matters of an appreciable distance from those that bear upon our survival? Surely common sense would have nothing whatever to do with quantum theory, for example, if common sense had any say in the matter. The common endowment of ideas and propositions, which is to say the endowment that has arisen in no small part through the process of evolution, is what it is because (if anything) it serves to enhance our *survival*, and not our *knowledge*, which are very different things. After all, what does our survival have to do with such things as what really transpires in the secret heart of an atom?

I propose to maintain the link between methodology and explanation, as Aristotle envisioned it, and to do this by putting forward, in the way of methodology, a certain account of how explanations (in the sense I have discussed above) are arrived at. And so my proposal is much more in the spirit of what Aristotle sought than what people think he found. My proposal will affirm that passage from experience to explanation is not deduction from observation, and also that observation has to be leveraged or leavened. Indeed it shall have all the anti-foundationalism characteristic of Karl Popper's method of conjectures and refutations, whilst keeping good faith with the insights garnered by Kuhn and Quine.[22] Here is very roughly how it will proceed. The most important step is already complete: we already have before us an argument to the effect that explanation is a genus under which fall many species. And that explanatory relations can be organized exactly as dependence relations are organized.

Proposals for scientific methodology are routinely conceptualized as recipes for manipulating theories (which in concrete terms are sets of propositions or probabilities attaching to propositions); or they are alternatively conceptualized as recipes for organizing a critical dialogue about propositions or probabilities. This is not at all what I propose. What I propose is instead a very abstract analysis of how science proceeds amongst propositions that are themselves organized within an *explanation space* – the space of dependence relations. I propose that we should raise issues pertaining to what maneuvers in that space of explanations amount to, as well as how to proceed within that space once promising would-be explanations are identified – either for the phenomenon for which explanation is sought, or for putatively similar others. We need not formulate specific recipes for maneuverings or trajectories through the space of explanations, though I do not think we should in principle be opposed to recipes, possibly as rules of thumb. My proposal is thus further on the side of description than on that of prescription. But I am insisting that an account of scientific method should at the very least tell us how explanation and justification are related, and so should be more than a theory of confirmation purely.

Once a space of explanations is clearly in view, we can ask, with regard to any given observation or singular fact, where in that space of explanations its own explanation lies. Determination of that answer can then be made in many different ways. And a variety resources can be brought to bear. For example, it might be useful to make comparisons of the observations or facts we seek explained, with others for which explanation has been sought (and by hypothesis found) in the past. Or it might be of some help to examine extant explanations of a range of putatively related phenomena, in case they already cover the case for which we seek an explanation. So part of the matter of how to proceed vis-à-vis a given fact, involves a decision as to what *discipline* is best positioned for its explanation. My full account of scientific method – for which there is no room here – shall thus be in terms of how we maneuver through the space of explanations, and thus through the space of disciplines, in the attempt to locate candidate explanations of a given fact. A maneuver through the space of explanations will amount at least in part to a determination of (or at least a search for) the species of dependence relation – and hence the discipline – best suited to the explanation sought.

But in spite of being a now-and-forever account of the relationship between explanation and method, my account need advance no now-and-forever *recipes*, apart from: use your best judgment. Innovation is, always and everywhere, the first order of business when no candidate for explanation is yet on hand. But after innovation has been achieved, the next order of business is, just as Popper argued, to test it – to open it up to criticism from every conceivable direction. We even have to be open to retracting the observation or purported fact (for which an explanation was sought initially) as being false, and hence as not deserving of explanation. In this regard my account repudiates the foundationalism which says that observation is always and everywhere more fundamental than other rational activities, and hence makes room for weaving back together the enterprise of explanation with that of testing.

My proposal, however, does not amount to saying that the method of science is simply the method of inference to the best explanation. For one thing, the term "inference" is most assuredly inappropriate. For another, my account need make no evaluations of explanations as to worse or better. Explanations are chosen or rejected only on grounds of suitability; there need be no universal scale of better and worse. Indeed, I have made explicit room for multiple, equally respectable varieties of explanation.

This roughly sketched account of methodology, whilst recipe-less (in precisely the same measure as Popper's methodology is recipe-less), is nonetheless now-and-forever, in no small part because the theory of explanation, devised in the bulk of the paper, is *a priori*. For the account I have offered of the space of dependence relations is *a priori*. It is, at any rate, without empirical content. Even so the methodology is not naively ahistorical (and where it is indeed ahistorical it is not naively so). Consider, for example, the testing portion of our proposal – the method of refutations, snatched with nary a word of thanks from Popper. It cannot be considered a strong test of a given proposal, that it passes a test that all its rivals also pass without hitch. So testing a proposal, as envisioned especially by Popper and his followers, while not a formula-driven activity, proceeds in part by paying very close attention to where predecessors of the proposal in question have faltered. In some sense, therefore, testing a theory is always at least implicitly a comparative matter: we test a proposal typically where some of its competitors are known to have already failed.

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NOTES

1 The most influential work in this spirit is Salmon (1984).

2 For caveats and qualifications see Thalos (1999).

3 Grelling called this relation 'Equidep', and derived interesting theorems about it.

4 Some covariation relations can be expressed by means of a single mathematical equation, for example $y(x) = x^2$. Others cannot, and require the high-powered instruments of set theory. (An equation is typically an abbreviation for a universally quantified proposition, for example '$y = f(x)$' is taken for granted as an abbreviation for the more wordy proposition 'for all x and y such that conditions φ hold, $y = f(x)$.')

5 This notion too is due to Grelling, who called it 'Vardep'.

6 Grelling considers the two problematic cases we have discussed, and writes, "However, in my opinion, from such trivial cases, well known to logicians, no serious objection can be derived against my suggestion of describing one sort of dependence by the statement [F]" (Grelling 1988, p. 218). He does not, however, explain why not, as I have done.

7 Metaphysical issues surrounding the relations between primitive quantities and those which are shaped by them will of course arise. ((Armstrong 1978) lays these issues out nicely.) This is not the place to handle these issues.

8 In the *Encyclopaedic Dictionary of Physics* (Thewlis 1962, p. 818) – one of the very few places in physical literature where there is any attempt to give instruction on usage of the term *independent quantity* – M. McGlashan defines *independent variable* as follows: "In a thermodynamic system at equilibrium $F = C + 2 - P$, intensive variables (such as temperature, pressure, densities of the phases) must be specified in order completely to define the state of the system, where C is the number of independent components and P is the number of phases." Here the term 'independent quantity' never even appears, only the term 'independent component'. The term 'degree of freedom,' in comparison, receives the following definition: "The number of degrees of freedom of any mechanical system is the minimum number of coordinates required to specify the motion of that system" (Thewlis 1962, p. 274).

9 The claim that designation of the degrees of freedom of a system cannot be made absolutely or uniquely is stressed in standard textbooks on classical mechanics. According to the *New Dictionary of Physics,* this nonuniqueness is due to the fact that the "generalized coordinates may be chosen *in more than one way*" (emphasis added). How so? If we wish to specify displacement of the center of a billiard ball, say, from a certain reference point, within a closed room, we can do it in (much) more than one way. We can specify rectangular coordinates of a vector with tail anchored to coordinates of the reference point; rectangular coordinates will be vertical distances of the center of the ball from planes which intersect at right angles at the reference point. We will, of course, require three coordinates to make the specification in a 3-dimensional room. Or we can designate the center of the floor as reference point and one ray lying in the floor and emanating from that point as direction of reference, then specify two angles (an azimuthal angle, and a vertical angle) and a distance from the center of the floor. And there are as many more schemes of specifying displacement as we might care to have about. Specification of all six coordinates mentioned above will reveal that (at least) three of them will be excessive, because (at least) three will always covary with the others. And if the object whose coordinates we wish to specify is constrained to remain on a particular surface, say a model train track, then we will recognize that even three coordinates is excessive, and we can make do with fewer yet (for instance, merely distance along the track from a certain point will pinpoint the caboose). The smallest number of coordinates required to specify the location of an object is that object's number of degrees of freedom.

10 Smith (1988) suggests the direction we might go in reconstituting the objects by binding the quantities together.

[11] This proof relies on the clear and important truth that no asymmetrical relation is necessitated by a symmetrical one in which at least two things or quantities stand to each other. *Proof*: Let A be asymmetrical, so that for all $x \neq y$, $A(xy) \mapsto \neg A(yx)$ and $A(yx) \mapsto \neg A(xy)$. Let S, on the other hand, be symmetrical, so that for all x and y, $S(xy) \mapsto S(yx)$. Then $S(xy) \mapsto A(xy)$ implies a contradiction provided there are at least two things or quantities that stand in the relation S to each other: $S(xy) \mapsto A(xy) \mapsto \neg A(yx) \mapsto \neg S(yx) \longmapsto \neg S(xy)$. There is no logical barrier, however, to the reverse being true: a symmetrical relation may follow from an asymmetrical one. For demonstration all we need do is furnish an example. Let $S(xy)$ designate the relation: $Fx \rightarrow \neg Fy$. Thus $S(xy) \mapsto S(yx)$ and $S(yx) \mapsto S(xy)$. And let $A(xy)$ designate the relation: $Fx \wedge \neg Fy$. So that $A(xy) \mapsto \neg A(yx)$ and $A(yx) \mapsto \neg A(xy)$. Then $A(xy) \mapsto Fx \wedge \neg Fy \mapsto S(xy)$.

[12] For more on that see Thalos (forthcoming, 2002).

[13] The collection of articles in Cushing and McMullin (1989) is a good introduction to this debate.

[14] Details of this inconsistency is found in the introduction to Cushing and McMullin (1989), as well as in Hughes (1989). We find there that folks are as much troubled by the failure of a condition called "locality" (which just amounts to no action at a distance) as they are troubled by failure of the common cause principle. If someone is troubled by the failure of locality, it is due to the fact that causal explanation of that phenomenon is impossible.

[15] Kitcher's DN-inspired position can give a similar explanation of EPR-type phenomena, though one that rests not upon issues pertaining degrees of freedom, but rather upon issues having to do with the coherence of our total physical theory of the world.

[16] See Hughes (1989) for details of this matter.

[17] Similarly for the American pragmatists. But that story is too long for this short space.

[18] It routinely gets called "induction," but Feyerabend (1978) calls it "common sense."

[19] There is another important connection between the two, which I examine in In Favor of Being Humean. And it is an aspect of this one.

[20] To be sure there is a very heavily subscribed alternative to this anti-now-and-forever position. Bayesianism is one.

[21] Of which, I take it, (Cartwright 1989) is also an instance.

[22] My defense of this position against charges (brought in the first instance against Popper's position) to the effect that there can be no room for objective rational criticism, is to be found in my "*The Logic of Scientific Discovery* by Karl Popper", in *The Classics of Western Philosophy*, J. Gracia, G. Reichberg and B. Schumacher, eds, Blackwell, forthcoming in 2002; and "Distinction and Judgment," in preparation.

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