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Précis of *Laws and Symmetry**

BAS C. VAN FRAASSEN
Princeton University

Laws and Symmetry has three main objectives. The first is to show the failure of philosophical accounts of laws of nature. The second is to undercut the epistemological principles at work in arguments for the reality of laws of nature. The third objective, nearest to my heart, is to be constructive as well, and to contribute to an epistemology and a philosophy of science antithetical to such metaphysical notions as laws of nature. Part One, in which the first objective is pursued, was the main subject of discussion in the symposium to follow. In this Synopsis, therefore, I shall concentrate on that.

In my view, as presented in Chapter 1, the concept of a law of nature is an anachronism, its proper life belonging to the 17th and 18th Centuries. Laws of nature played an important role in the philosophical-scientific thinking of Descartes and Newton, and functioned for them as a central clue to the structure of science. At the same time, two developments threatened the status of law. One was the empiricist critique of necessity and causality, notions closely allied to that of law. The other was that science was rapidly gaining autonomy not only from theology but from all of philosophy, and was exploiting concepts and methods foreign to metaphysics. Pre-eminent here is the birth of the symmetry argument. (Discussion of this subject is begun in Chapter 1 and continued in Chapters 10, 11 and 12.) Modern physics argues from symmetry and continuity—not from universality or necessity, natural kinds or essences, contingency or accident. The concept of a law of nature is a *vestigial* concept in contemporary science.

Chapter 2 collects the cluster of criteria for what laws must be and do, which are honored in the literature to some degree or other. We can divide the criteria to be met by any philosophical account of laws roughly into major requirements and secondary ones. The major criteria concern what I call the problems of *inference* and *identification*. The accounts must show that there is a valid inference from what laws there are to what regularities there are in the world. The account must also identify the relevant aspects of the world that constitute or give rise to its laws, if any. Typically these two tasks lead

* Oxford: Clarendon Press, 1989. Pp. xv, 395.

to a dilemma. If laws of nature are identified in terms of some sort of necessity in nature which is simply postulated as fact, then there is no logical reason to think that the inference from lawlike necessity to actuality is valid. (Calling the postulated factor "necessity" or "necessitation" does not help.) If on the other hand the semantic account of law statements is so constructed that the inference in question is logically valid, then typically the truth-conditions of law statements involve something unidentifiable. Chapters 3, 4, and 5 argue that leading contemporary attempts (by David Lewis, David Armstrong, and a host of others) fail to slip between the horns of this dilemma. Nor do they meet secondary criteria, such as showing that what they make out to be laws of nature are the targets reached, or even aimed for, in scientific inquiry.

Both Quine and Rorty have, in their different ways, proclaimed the death of epistemology. I think they are right about mainstream traditional epistemology. There Induction has given way to Inference to the Best Explanation (IBE) in the epistemology hospitable to realism, or to metaphysics in general. In my view, developed in Part Two, neither Induction nor IBE qualifies as a rational strategy for change of opinion. To that extent at least I endorse some of Quine's and Rorty's conclusions. But it also seems to me that the underground river of probabilism, slowly growing in force over three centuries, has burst forth above ground in the twentieth century and brought new hope for epistemology. In Part Two I argue that with the end of foundationalism, probabilism provides the framework for a new epistemology, which is also adequate for philosophy of science.

The remainder of the book (Chapters 8–13) is devoted to contributions to the semantic approach in philosophy of science, to support my call to leave metaphysics behind. The semantic approach does not *require* an anti-realist or anti-metaphysical stance. In fact it is also followed by philosophers with very different philosophical positions from my own. But that is just the point: this collaboration in philosophy of science is possible because the approach is in itself neutral, and does not *presuppose* metaphysical views. I will leave the details aside, since the present symposium concentrates on Part One, which was meant to be the destructive prelude to this constructive effort in philosophy of science.

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In Defense of Laws: Reflections on Bas van Fraassen's *Laws and Symmetry*

JOHN EARMAN
University of Pittsburgh

1. Laws as a Rorschach test for philosophy

The topic of laws of nature provides a kind of Rorschach test for philosophy. Some philosophers see in laws only Humean regularities; others see a kind of physical necessity; others see a necessity closer to logical necessity; others see expressions of causal powers; others see inference tickets; still others see relations between universals; ... ; and some see only a messy inkblot.

We can also perform a meta-Rorschach test on the results of the first test. When van Fraassen and I submit ourselves to this meta-test we both recoil with shock and horror. Where we differ is in drawing morals from this unpleasant experience. For me the opprobrium falls mainly on the way philosophy is practiced. For van Fraassen, the disgrace also touches the practice, but for him the source is the rottenness of the concept of laws.

2. The no-laws view

Van Fraassen draws up a laundry list of demands that philosophers have imposed on laws, and then with characteristic incisiveness he proceeds to show that sometimes individually and certainly collectively these demands cannot be met. What follows? Bas wants to conclude that there are no laws, or that there are no laws worth having. I could be persuaded of this conclusion, but for me the persuasion cannot come from pondering the vast introverted and reflexive philosophical literature on laws. Many contributors to this literature start with a hidden (and sometimes not so hidden) agenda containing doctrines about necessity, causation, explanation, universals, etc., and they then try to tailor an analysis of laws to fit the agenda. But for me the question "What is a law of nature?" is first and foremost a question of how the concept of law structures actual scientific practice. And in seeking an answer I adopt the attitude that we should let the chips fall where they may for necessity, causation, etc.

To be concrete, consider gravitational physics from Newton to Einstein to the present day. Is it possible to understand the history of this field without construing the scientific activity as being in large part a search for the laws of gravitation and an attempt to understand their implications? Is the use of the notion of law in this field so muddled that both working scientists and philosophers seeking to understand the methodology and foundations of science would be better off dropping the notion altogether? If the answers to these queries could be shown to be positive, then—and only then—would I agree that there are no laws of gravitational physics.

Lest I get carried away with my own enthusiasm, I should pause to note that there is a seeming difficulty with my position. I can put it somewhat impolitely by asking myself how I know what I am talking about. The problem arises because, for example, when I random sample the indexes of various textbooks on Einstein's general theory of relativity (GTR) I do not find any heading like 'laws of GTR'. The quick response is that scientists seek laws to provide predictions, explanations, unification, systematization, ... Thus, when I examine the famous cases of successful prediction and explanation in GTR—the advance of the perihelion of Mercury, the bending of light, etc.—I find that in each case the centerpiece consists of Einstein's field equations. I therefore take these equations to express the main laws of the gravitational field according to GTR.

My response is open to the charge that it pre-supposes a wrong-headed doctrine about the nature of scientific theorizing. The correct doctrine (so the charge goes) is that the goal of science is to find an empirically adequate theory, a theory that saves the phenomena in the sense that the theory has a model into which all the relevant phenomena can be fitted. My rejoinder is to note that in keeping with the proffered sense of empirical adequacy we didn't need Newton or Einstein to show us how to save the phenomena of gravitation; for by adopting postulates that say nothing substantive about gravitational phenomena we can assure that there is a model of the required type. On the contrary, after Newton no theory of gravity would have been regarded as adequate unless it subsumed Kepler's laws (so called), and today no theory of gravity would be regarded as adequate unless it correctly predicts the advance of Mercury's perihelion, etc. Thus, finding an empirically adequate theory is intimately bound up with the search for laws.

Next I note that there are some versions of the semantic conception of theories which would undermine the "statement view" of theories and, therefore, would wreck the idea that in their theorizing, scientists are out to capture laws in the form of statements.[1] For example, at various points in *Ex-*

[1] This is a version of the semantic conception of theories that (I believe) Bas does not endorse. In this section I do not mean to put words into Bas' mouth but only to review some considerations which might seem to militate in favor of a no-laws position and which are suggested by *Laws and Symmetry*.

plaining Science Ronald Giere seems to endorse the view that the allowed models of the theory are not picked out by a set of statements (aka the laws of the theory); rather scientists proceed by exhibiting paradigm examples of allowed models, and then the remainder of the target class is delimited by a family resemblance relation (see pp. 86ff). This view is an open invitation to disaster. If one has to rely on *sui generis* intuitions about the family resemblance of models to tell whether the theory predicts a phenomenon, then what one is doing is closer to pseudo-science than to science. It is certainly true that scientific theories have an open and vague texture. In terms of my own running example, Einstein's field equations were initially taken to mean without cosmological constant but later were taken to include such a term; and other constraints on the models, such as positive energy densities and the prohibition against closed causal curves are also contemplated. But I do not see the slightest indication that these problems of vagueness and open texture cannot be settled in propositional terms: we need to put Carnapian subscripts on 'GTR', the different subscripts denoting different theories where the differences lie precisely in the postulates (putative laws) being asserted.

I come now to the idea that in modern science symmetry considerations take the place of or obviate the need for laws. I partially agree with this idea, but I do not think that its valid core in any way supports the no-laws stance. For classical and special relativistic theories (and more generally for any theory dealing with geometric objects based on a fixed space-time) it can be argued that the invariances of the laws must match the symmetries of the underlying space-time (see my 1989). Thus, since Newtonian and special relativistic space-times are spatially homogeneous, it follows that laws of motion based in these settings must be invariant under spatial translations. And given a few additional assumptions, this invariance property in turn implies that momentum must be conserved for a closed system. In this and similar cases, symmetry considerations allow us to deduce important results without knowing the details of the dynamical laws. But there is no support here for the no-laws view; indeed, the status of the law of conservation of momentum (as a kind of law of laws) cannot be properly understood without appreciating the relation between laws and symmetries.

Symmetry arguments are especially powerful in quantum mechanics. As a minor example, it is readily proved that for any system which has an odd number of spin one-half particles and whose dynamics is time reversal invariant, the stationary states must be at least two-fold degenerate ("Kramer's degeneracy"). This result has important implications for paramagnetic ions in crystals. It might be thought that the quantum case is qualitatively different from the classical case since in a sense the quantum law of motion follows from symmetry considerations: the Schrödinger equation is just the infinitesimal version of the statement that time translation is implemented by a unitary operation, which in turn follows from the homogeneity of time. Let

us not delude ourselves here. The *form* of the law of motion follows from symmetry considerations; but the *content* of the law, which is specified by the Hamiltonian, does not. The content may, perhaps, be further delimited by symmetry properties—such as invariance under time reversal and parity. But these properties are, of course, contingent. And the details of the dynamics, which may be crucial to both practical and theoretical concerns, cannot be deduced from symmetry considerations.

Finally, it is worth mentioning the situation in GTR. The conservation of energy-momentum in the form of the vanishing of the divergence of the stress-energy-momentum tensor is a direct consequence of Einstein's field equations. But to integrate this local conservation law to produce a global conservation law hinges on symmetry properties of the space-time. Thus, to get to the familiar statement of the conservation of mass-energy in the form "total mass-energy at t_1 equals the total mass-energy at t_2" requires that the space-time admit a timelike Killing vector field. Since most general relativistic space-times do not possess such symmetries, the cherished form of conservation principles is unavailable.

In sum, I agree with van Fraassen that there is a fascinating interaction among the concepts of laws, invariances, and symmetries. But I not only cannot see anything in the interaction to support a no-laws view, I do not begin to see how the triad can be collapsed to a diad without collapsing altogether.

3. An account of laws

The only sure way to support a no-laws view is, first, to show that every extant account of laws is defective and, second, to perform a negative induction. I almost agree with the first step sinee I think that the vast majority of accounts offered in the philosophical literature founder either because of internal difficulties or because they fail to do justice to actual scientific practice. But I also think that what I have called the Mill-Ramsey-Lewis (M-R-L for short) account comes closest to the mark. Roughly the idea is that the laws of this world are the axioms or theorems of the best deductive system, where a deductive system is an axiomatizable, deductive closed set of true statements and where the best such system is the one that achieves the best compromise between simplicity and strength. Scientific theorizing can be seen as a *groping* towards the optimal system. I emphasize the word 'groping'. Even with respect to a limited domain of inquiry—say, gravitational physics—scientists don't consider all possible theories (= deductive systems) of the domain. For given our limited computational powers and limited imaginations we are lucky if we can explicitly produce more than a handful of theories that would be judged as minimally adequate. Nevertheless, I contend that scientists manage to convince themselves that they have made a fair stab at satisfying the

M-R-L requirements. Einstein, for example, used a series of heuristic arguments to convince himself and others that we are forced almost uniquely to his gravitational field equations if we start from the Newtonian field equations and follow the simplest and most natural route marked out by the special theory of relativity and various plausible guiding principles (such as the principle of equivalence).

The case for M-R-L can be strengthened by considering how various objections to it can be met. I have space only to consider what I take to be two of the most serious challenges. To set up the first, note that on the M-R-L account, whether an individual statement L expresses a law cannot be determined by features of L itself. So, for example that $\nabla \times \vec{E} = -\partial\vec{B}/\partial t$ expresses a law of electromagnetism depends on the fact that it fits harmoniously together with three other differential equations to form what are called Maxwell's laws. The objection is that this observation backfires. For on the M-R-L account scientists shouldn't have accorded Maxwell's equations the honorific of 'law'; they should have waited to see how these equations fit together with other equations to form a comprehensive system for all of physics. The response starts from the obvious: given the kinds of creatures we are and given the complexities we face, we can't investigate everything at once but have to focus on selected aspects of the world. We hope that we have managed to focus on a domain of phenomena that is fundamental in the sense that the "laws" we construct on the basis of ignoring everything outside the domain will survive in some recognizable form as laws as we extend the scope of investigation. The history of science for the last hundred years shows that "Maxwell's laws" are robust in this regard.

The second and more serious objection starts from the observation that on Lewis' version of M-R-L there is no presumption that there is a unique deductive system that achieves an optimal compromise between M-R-L virtues, and so he takes a law to be an axiom or theorem that appears in each of the optimal deductive systems. But, the objection continues, there are myriad ways to carve up the world; and different carvings can lead to very different optimal compromises. Hence, there is no reason to think that there will be a non-trivial intersection for axioms or theorems of all of the optimum compromises.

The response to this objection depends upon the reading of the metaphor of carving up the world. The radical reading of the carving up metaphor would reject realism and would take talk about the world, the way the world is, the facts, etc., to be pieces of a false metaphysics. To the contrary, there are no facts that are neutral with respect to all conceptual frameworks (or whatever), so that if the world is a world of facts, then there are many worlds. I must confess that I don't understand such fashionable anti-realism/relativism well enough to argue against it. So my response can only be that if this sort of anti-realism/relativism is correct, then the notion of law must be relativized

to a conceptual framework (or whatever). But the upshot is not a strike against M-R-L since it is M-R-L that tells us what a law cum conceptual framework is.

The less radical reading of the carving up metaphor would allow for ontological realism and would cash in the notion of different ways to carve up the world in terms of different descriptions of the one reality. Here we can imagine different versions of the problem under discussion.[2] We can imagine, for example, that our world is such that there are two or more deductive systems which have little in common and which tie for first place on any reasonable account of simplicity. In this case there would be far fewer laws than we like to think there are. Or we could imagine that there are two or more deductive systems with little in common such that one comes out ahead on my version of simplicity while others come out ahead on other accounts of simplicity. In this case the notion of lawhood would be more subjective than we like to think. I take David Lewis to be saying that in our current state of knowledge we have reason to hope that such cases do not in fact arise in the actual world. And I take actual scientific practice to be a practical expression of this hope. I take van Fraassen to be saying that this hope is a vain one. How do we settle the issue? And more fundamentally, what is a fruitful way for discussing it? I don't have answers to these questions; but I do have a modest suggestion: let us continue the discussion in terms of some concrete examples. Failure to produce them would support Lewis' hope.

4. Conclusion

In closing let me note that in *A Brief History of Time* Stephen Hawking wrote that "there are grounds for cautious optimism that we may be near the end of the search for the ultimate laws of nature." (p. 156) Hawking may perhaps be guilty of the overoptimism that led physicists at the turn of the century and in the 1920s to issue similar pronouncements. But I would not want to subscribe to a view about laws that implies that he must be mistaken.[3]

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2 Here I am indebted to David Lewis. But needless to say, he cannot be held responsible for the views I am expressing.

3 I want to acknowledge the many helpful discussions I have had with Bas on these matters. Space limitations prevented me from praising the many splendid features of *Laws and Symmetry*. But then the genius and abilities of its author are so distinguished as to be above my commendation.

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The Identification Problem and the Inference Problem

D. M. ARMSTRONG
The University of Sydney

In Chapter 5, Section 1, of *Laws and Symmetry* van Fraassen poses two problems for the version of the 'relations of universals' theory of laws of nature put forward by Dretske, Tooley and myself. First is the *Identification Problem.* What is the law-making relation that holds between the universals? The second is the *Inference Problem.* What information does the statement that one property stands in this relation give us 'about what happens and what things are like' (p. 96)? If I understand him, van Fraassen thinks that the two problems taken together behave like a short sheet or a refractory bulge in the carpet. A solution to one of the problems makes the other insoluble. In this note I will try to show that this dangerous-looking dilemma does not have to succeed.

Let us follow through the inference to these 'strong' laws. It begins from 'what happens and what things are like'—the four-dimensional scenery, as I take it. This scenery exhibits many regularities, some statistical only. In particular, I would argue, it includes many regularities whose instances are causal sequences. These causal sequences are singular: token-token. This striking causes the glass to shatter. But these singular causal sequences frequently instantiate regularities.

Some philosophers, perhaps van Fraassen, may object to my using the existence of singular causation as a premiss of the inference to laws. But it seems clear that we (and animals) do regularly perceive that one thing causes another. For myself, I believe that at least some of these perceptions are as epistemically primitive as any other perception. A conspicuous case is perception of pressure on our body.

So, I claim, we can start from singular causations that nevertheless exhibit a pattern, a pattern where, putting it very roughly, the same type of cause produces the same type of effect. It is at this point that a first inference can be made. We can infer to the existence of universals to explain these 'samenesses'. Suppose, to consider a type of pattern that is no doubt too simple, that it is found that something's becoming an F invariably brings it

about that that same something becomes a G. Suppose, no doubt also unrealistically, that it is reasonable to think that F and G are actually universals, instead of first approximations to universals. Then, if we take all the tokens of this type of causal pattern, they instantiate *the very same pattern of universals.*

At this point, despite postulating universals, we have nothing to offer as a law except regularities of pattern involving singular causation. But now the question arises whether the regular succession—this sort of cause bringing about this sort of effect—cannot itself be explained. We have the bunch of singular causations, the same sort of cause bringing about the same sort of effect. May we not seek to explain this? May we not hypothesize that this uniformity holds *because* something's being F *brings it about* that that same something becomes G? This latter is not a 'general fact', one expressed by a universally quantified proposition. Rather it is supposed to be an 'atomic fact', albeit a higher-order fact, a relation between the universals F and G. It is at this point that, I claim, the Identification problem has been solved. The required relation is the causal relation, the very same relation that is actually experienced in the experience of singular causal relations, now hypothesized to relate types not tokens. There is of course no question of *proof* that this hypothesis is true. It is rather a postulation that recommends itself because of its explanatory power.

If this, the most controversial step in the argument, is satisfactory then it seems that the Inference problem is solved. For if a certain type of state of affairs has certain causal effects, how can it not be that the tokens of this type cause tokens of that type of effect? The inference is analytic or conceptual.

I hope that this is an improved version of my previous attempts to explain how it is that laws as relations of universals can both entail yet explain regularities, and that this account evades van Fraassen's fork. It will be seen that the new version allows for different patterns of causal relation holding between types of states of affairs. I think that the account can be extended to probabilistic laws without too much difficulty.

One problem remains. My account has placed great weight on the notion of causality. But are there perhaps laws of nature that are both irreducible and non-causal? If there are, what account is to be given of them? This is unfinished business for me. If there are such laws then I hope that some account can be given of them by *analogy* to the causal laws. But that is as far as I can see at present.

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In Defence of 'This Worldly' Causality: Comments on van Fraassen's *Laws and Symmetry*

NANCY CARTWRIGHT
London School of Economics

1. Why are Causings Worse than Photons?

Recall van Fraassen's general scheme. Scientific models can depict both observable and unobservable processes. Science itself is supposed to tell us what is and what is not observable. We are to accept the theory if it gets right all that is observable; the rest can be of no matter to us, van Fraassen has suggested, because we could have no evidence about whether the model gets it right or not. We may believe in the model's picture of the unobservable world but that belief is based on faith not evidence, and should be recognised as such. Most of the stuff of modern physics, then, turns out to be in the unobservable part of the model: photons, quarks, gravity-waves, the electromagnetic field, forces, I suppose and even space-time itself. Where are causes? that is the question I want to discuss.

We can begin with causal laws. They are surely for van Fraassen not in the model itself any more than any other kinds of laws. They enter, rather, as a constraint on the kinds of structures we will allow our models to have. In a full model for the entire history of the world, or more realistically, in a small model for a given scientific experiment, event-kinds will recur with some frequency. These frequencies are represented by probabilities—though note that the probabilities are not in the models either. If we insist that our scientific theory in a given domain be causal, that will restrict the form of the probability functions which are allowed and hence, in some way, constrain the actual models, which have finite frequencies in them only. Some patterns of correlation will be allowed and some will be prohibited. Consider van Fraassen's discussion of the Einstein-Podalsky-Rosen (EPR) paradox in the follow-up to *Laws and Symmetry, Quantum Mechanics: An Empiricist View*.[1] There van Fraassen takes a particular relationship between probabilities—called *factoriz-*

[1] van Fraassen, Bas, *Quantum Mechanics: An Empiricist View* (Oxford University Press, 1991).

ability—to be just the relationship we should impose on models which, speaking inperspicuously in the material mode, we would ordinarily describe as "common cause" structures. J. S. Bell has shown, in his famous inequalities, that it is not possible to satisfy factorizability and still reproduce the statistical predictions of quantum mechanics for EPR. So, argues van Fraassen, if we insist that our models be causal, and we assume a straightforward connection between frequencies and probabilities, then all models with frequencies reflecting the quantum mechanically predicted probabilities for EPR will be excluded. That is, with causality (as represented, I think mistakenly, by factorizability) as a constraint we cannot have a model which adequately describes the probabilities which we observe in EPR.

That is a story about causal *law*. What about cause*s*? It is my view that if van Fraassen's models have (representations for) either the objects of modern physics in them or the ordinary objects of the world around us, they already have causes in them; and if these objects do in the models what they do in the world, the models have cause-effect sequences in them as well. If the model has cats or photons, it has causes; and if it has cats lapping up milk or photons scattering from atoms, it has causal sequences, or *causings*.

That is not what van Fraassen thinks. He wants to start with what might be described as "demodalized" properties; probably these are supposed to be something like the *sensible* properties of Locke and Hume. It is the history and arrangement of these, I take it, that is supposed to be depicted in the observable substructure of a scientific model—the part we can insist that the model get right. Following the philosophical tradition of Locke and Hume we are led to think of causality or necessity as insensible glue or cement that holds together the sensible slices of reality. We look for it, then, in the unobservable substructure of the model. But it is not there. van Fraassen does not want to allow causality anywhere inside his models. It can only function as a shorthand way of expressing constraints on model structures. Why is that?

van Fraassen usually argues in terms of observability. That seems not to be what is at stake here. If his quarrel with causality was that it could not be observed, it could still stand in the unobservable substructure among the photons, fields, and forces. We could suggest the answer of the British empiricists themselves: the trouble with causality is not that we cannot see it, but rather that we cannot understand it. The notion of causality is unintelligible. I hesitate to ascribe this answer to van Fraassen for two reasons. One is that his arguments against laws and other modalities do not directly accuse them of being unintelligible. Secondly, such an argument would have to be set in the context of some kind of theory of meaning or of concept formation. The Locke-Hume theories suppose the doctrine of impression and ideas, and the related associationist psychology of its day. But today we view associationism as a crazy kind of theory. Certainly it has no empirical standing. What

else then? Contemporary philosophers may hold onto it as a piece of metaphysics—a philosophical construction revealing the essence or meaning of concepts, the real structure that stands behind the phenomena of informing, entertaining, puzzling, calculating, and the like. That is a position that fits uncomfortably with van Fraassen's "worry only about the phenomena" point of view.

If van Fraassen's arguments do not bear on intelligibility, what do they bear on? The attack on laws focuses on two modern accounts of necessity; one, the possible-worlds account; the second, necessity as a relation between universals. The central argument against both is the tension that van Fraassen points to between the *problem of inference* and the *problem of identification.* He urges that stories that are at all informative about what necessity is, have trouble providing a story to account for why we can infer from *necessary A* to *A*. Conversely, theories that are good at defending the inference tend to be thin in information about what necessity is.

Here is how van Fraassen describes his *inference problem*: "What information does the statement that one property necessitates another give us about what happens and what things are like?" Focusing on this suggests an Ockham's-razor kind of reason not to put causes in our models. Van Fraassen cannot see how they can explain anything. They do no work; so it is superfluous to put them in. But that is equally true of photons and forces, indeed, of everything that appears in the unobservable substructure. They are there because of the way physics works. Physicists write down laws of the form:

> If such-and-such observable state and such-and-such photon state at *t*, then such-and-such (different) observable (and photon) state at t + Δ t.

Then we use these to construct the models that, on van Fraassen's semantic view, constitute the theory. But for the theory to be acceptable it need not even be right that there are photons, let alone describe correctly how they behave. You may assent to the claims about photons as an act of faith and maintain that you have now 'explained' the observable sequences. That depends on the pragmatics and psychology of explanation you subscribe to. The same can be equally so for causes.

2. How Modalities Are Better Than Photons.

van Fraassen begins with demodalized occurrences. Where do they come from?, we could turn to the sensible properties of the 18th Century empiricists. But without the copy theory of impressions and the simple associationist theory of concept formation, the distinction they wanted to draw, between sensible properties and powers, collapses as well. For them "Red" was *the*

property that looks like that. How then does a causing look? But for us, causings need no particular way of their own to look in order to be intelligible.

The other canonical source for demodalized properties are the fundamental laws of physics. I think we cannot really find them there. It seems to me that the attribution of these apparently non-modal qualities to the physical world around us is an unhappy consequence of the long attempt to give the abstract terms of physics a direct meaning. Maxwell's equation has *E's* and *B's* and *q's* and μ'*s* in them, so we interpret them with models that have fields in them, to which we ascribe "non-modal" properties, like electric and magnetic field strengths, charges, and measures of dialectic strength. But it must be remembered that those are models of the equations, not models of the physical systems the equations are supposed to treat. When science constructs a picture of bit of the world, the image is far richer. It contains atoms emitting not only electro-magnetic field strengths; it also contains photons, fields, light rays carrying momentum, magnets arranging patterns in iron filings. The scientific image of nature is no more devoid of cause and causings than is our everyday experience. The appearance to the contrary arises from looking only at science's abstract statements of law, and not how those are used to describe the world.[2]

In my view, to call a lapping of milk or the de-exciting of an atom a causing is to give a more abstract description to it. As is typical of the abstract/concrete relation, the abstract concept need not be definable as a big disjunction across its concrete manifestations. Nor need it supervene on them. My favourite example is *work.* This morning, I washed dishes, wrote part of this lecture, and negotiated with the Dean. Those three activities constituted my morning's work. But they did not supervene on it. Work is understood in relation to a number of other concepts like leisure, effort, preference, compensation and value. These are equally abstract relative to dishwashing, and whether dishwashing is indeed work depends not only on its more concrete properties, but on which other related abstract concepts it falls under as well.

Exactly the same is true of "causing." We have a cat lapping up milk in the observable substructure of our model. That is a causing. So we have a causing in the observable substructure. Perhaps we should say instead that the lapping is in the observable substructure but the causing is not, since the lapping is observable as a lapping, but the causing is not observable as a causing. But why not?, here is an example from van Fraassen himself in his last book: off in the distance we see the jetstream, but we don't see the jet. The problem with this example is that it gets us to focus too narrowly, on

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2 It would at any rate seem contrary to van Fraassen's program to look to highly abstract laws as the source for his demodalized events, since the quantities referred to there notoriously tend to inhabit the unobservable region of the model.

size or distance. Rather uncontroversially, the jet is too far away to be seen; an electron is too small. But neither of these problems beset the causing of the milk to disappear by the cat. What is characteristic of it, *vis-à-vis* the lapping (which is, I take it, admitted as observable) is that it is more abstract. Consider my favourite example again. You come into the kitchen and you see me washing the dishes. What else? You see me *working*. Ah! But don't you "really" only *see* me washing dishes and from that *infer* that I am working. We know that that is a slippery slope: perhaps you "really" only see me dipping my hands in hot water and infer I am washing the dishes....Is there some ultimately correct level of concreteness at which real observation takes place? I suppose not.

van Fraassen assures us that science can—and will—tell us what is observable. I agree that science may do so; but I suspect that if it does, it will do so primarily by answering the antecedent question of what there is in the world to be observed. If a more satisfactory economics or sociology eliminates the concepts of work and waste, or power, then we should admit that you were not, after all, observing *work* when you saw me doing the dishes even though you responded directly with the thought "Why is she working so hard on such a nice summer day?"

In *Laws and Symmetry*, van Fraassen has an argument against laws that we can try to adopt to defend his view of causality. He imagines a machine that takes you to another world "which has exactly the same world-history, past and future, as ours" (p. 90) but from which all laws have been deleted. It is called a "Hume world." He argues: "That Hume world is just like ours, all the same things happen in it, but it has no laws...so there is no *occurrent* difference between the two worlds at all. There is no observational or experimental evidence anyone could gather, that would have any bearing on whether we are in that Hume world, or in our supposed original. Equivalently, no such evidence could bear at all on the question whether we do not really live in a Hume world already" (p. 91).

We might try the same argument with respect to causings. Imagine a machine that takes you to another world "which has exactly the same world-history, past and future, as ours" but from which all causings are deleted. But that is ridiculous. Our world history has cats lapping up milk in it and photons being scattered by atoms. If we delete causings we have a very gappy history indeed, and I suppose that difference should be amenable to experimental evidence. Using my earlier example of an abstract concept, it is as if I proposed to transport you to a society just like ours where everyone looked just the same and all the same (more) concrete descriptions were true of their activities—they dug in mines, washed dishes, read books, went skiing, repaired automobiles. I want to charge you a very great deal for your ticket though because this is a very agreeable society indeed—it is a society in which no one *works*.

Perhaps an anti-causalist like van Fraassen will feel he need not mind leaving in causings in this sense. In fact, he may feel that my account of *causing* as a (relatively) abstract description can be turned to support his own view. So long as the new world has nothing in it but sequences of sensible properties, labelling some sequences as causal is perfectly all right. What must be resisted is the hypothesis that some insensible glue between the events is required, or some invisible relationship of necessitation, before the causal labels can be attached.

It is true that there is no glue in my story, but that should provide no comfort to the anti-causalist, for two reasons. First, there are not only causal claims true in this world, but a very, very great number of other (more) abstract claims as well that will sound suspiciously modal: "You made me love you," "It couldn't have been otherwise" "Here we stand, I can no other...." Nor are any of these concepts *reducible* to the more concrete level—that is characteristic of the abstract/concrete relation. Although every occurrence of the abstract concept needs to be instantiated in a more concrete situation ("The abstract exists only in the more concrete": G. E. Lessing) usually the meaning of the concept depends in large part on its relations to other equally abstract concepts and cannot be explained using only more concrete terms.[3]

So causality is not the glue that holds together sequences of sensible properties. Neither is it an indirect way of referring to complicated sensible features of those events, for instance, like the cannonical "regular association," "spatio-temporal contiguity," and "time ordering." It is (if one wants to use the language of properties) a separate property, related to but distinct from those properties which van Fraassen finds more palatable.

I seem to have gone far astray from the main topic. The first half of van Fraassen's book is about laws. But I have been talking about causes. The two are obviously connected in some way, but why talk about the one when I should be talking about the other? Because I think van Fraassen's criticisms of laws are based on a misconception of what laws are supposed to do. Put coarsely, he ascribes to them the wrong function; then he criticizes them for not being very good at satisfying it. Running throughout his arguments with Lewis and with Armstrong is the assumption that *laws explain* and that *laws necessitate*. I want to argue a familiar position in opposition: it is not laws that do either; it is causes. The cause necessitates its effect—it makes it happen, or brings it about; and the occurrence of the effect is explained by the occurrence of the cause, in the robust, realistic—non-subjective, non-interest-dependent—way that Hempel wanted. I start from a conventional empiricist view with which van Fraassen is sympathetic: laws do not prescribe; they de-

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[3] Abstract properties do not supervene on the more concrete, either. See N. Cartwright, "Fables and Models" "The Aristotelian Society, 'Supplementary Volume'" LXV, 1991, pp. 55–68.

scribe. But we immediately differ over what they describe. I think they describe *what causes are capable of doing*. They do not need to prescribe since something else—the cause—already does that.

I do not mean to suggest by this that van Fraassen's criticisms of Lewis and Armstrong-type programs are misguided. They, after all, do subscribe to the very functions for laws which I deny. They, all three, start out from the late British empiricist tradition that thinks it has stripped nature of all causings. But Lewis and Armstrong, unlike van Fraassen, worry about issues like the objectivity of counterfactuals or explanatory power or making sense of determinism, and so they try to do the jobs of causality with a surrogate they locate in some *non*natural place: for Armstrong, in a relation between universals; for Lewis, in some accessibility relations between possible worlds. They then fall prey to the tension van Fraassen notes between identification and inference.

By now it is clear what what I want to say about all this. There is no nature stripped of its causings' and there are no demodalized sensible properties to try to build one from. We are stuck with a nature causal through and through, and the this-worldly causality we have does not suffer from the dual problems of identification and inference. In *Laws and Symmetries* van Fraassen has told us what is wrong with non-natural locations for causality. The next project, I urge, is the account of what is wrong with the natural locations.

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Armstrong, Cartwright, and Earman on *Laws and Symmetry*

BAS C. VAN FRAASSEN
Princeton University

PRECIS

All three commentaries have challenged me to confront new issues. I want to thank Professors Armstrong, Cartwright, and Earman especially for not limiting this discussion to the confines of the book, but pushing it into areas where I had feared to tread.[1] To some extent my commentators are also each others' target. What Earman thinks we can hope for by way of an account of laws and science is in clear contradiction, it seems to me, to Cartwright's *How the Laws of Physics Lie*. His preferred starting point, the Mill-Ramsey-Lewis account, is of just the sort subjected to scathing critique in Armstrong's *What is A Law of Nature?* Armstrong's and Cartwright's current focus on singular causal statements makes them allies today, in opposition to views Earman has forcefully expressed elsewhere. But for Armstrong, this yields a way to explain what laws are. Cartwright's diagnosis of how I misconceive laws ("it is not laws that [explain, necessitate]; it is causes") is an indictment of what I thought I had learned from Armstrong. I should not take too much comfort from this (is the opposition divided? or am I being attacked from all sides?) but perhaps it means that we are all each others' allies in some respects. I am very happy to find such allies.

Earman and Cartwright are strong critics of traditional concepts of law, and especially of what Earman calls the "vast introverted and reflexive" philosophical literature on law. On the other hand, both Armstrong and Cartwright make a strong case against the Humean supervenience view, the idea that everything supervenes on the vast mosaic of local matters of particular fact, in David Lewis' striking metaphor. In contrast, Earman seems to warmly embrace that view. Here I am at least closer to Earman's way of thinking, in that I think of possibility and necessity as deriving in some way from models and language. In the second part of my Reply, I will put forward a very tenta-

[1] The papers by Cartwright and Earman are revised, shortened versions of their commentaries at the APA, Pacific Division, March 1991. Armstrong's commentary is a sequel to our interchange in the *Australian Journal of Philosophy*, 1988.

tive idea about how this way of thinking might still respect the role of causal discourse.

REPLY

PART ONE. THE THREE COMMENTARIES

Armstrong's and Cartwright's challenge to take causation more seriously will lead me farthest afield; I shall therefore begin with Earman's commentary.

1. Earman in defense of laws

As I see it, the concept of law of nature is vestigial. It played an important role in scientific thinking in the 17th and 18th century (and cognates did before that). But that concept survives today only in usages that have lost their content, remaining as wheels that turn no other wheels. The term "law" turns up as an endorsing epithet in such passages as Stephen Hawking's reflection on the possibility of a 'theory of everything' which Earman quotes ("... the search for the ultimate laws of nature"). There are undoubtedly hierarchies on the side of theory (some principles are deeper, farther-reaching, and historically more stable than others in the evolving body of science), but the terminology of laws honors them with the name of a presumed hierarchy in nature.

I think that Earman really agrees with this. As he says, his main examples of laws—Einstein's field equations, Maxwell's equations—are not called laws in the scientific literature.

If someone asks "What are some examples of laws of nature?", the kindly physicist will first perhaps interpret it as terminological, and look for cases where the title "law" is used. In that case the answer will be "Ohm's Law, Newton's Laws of Motion," and not "Pauli's Exclusion Principle"—no one ever says "Pauli's Exclusion Law." S/he may secondly interpret it as asking for very basic principles in today's physics, principles that belong to the foundations which will remain stable for the foreseeable future. Then the answer is "Schroedinger's Equation, the Exclusion Principle" or whatever, but not "Ohm's Law."

So Earman is calling us to a more meaningful philosophical enterprise, some significant way to illuminate the mysteries of physical science:

> for me the question "What is a law of nature?" is first and foremost a question of how the concept of law structures actual scientific practice. (Earman this issue, p. 413)

This question has a presupposition: that there is a concept of law which does in fact structure actual scientific practice. As the question is posed, it envisages—or so it seems—a situation in which the philosopher knows what the (relevant) concept of law is, and then sets off to investigate how this concept plays a role in shaping scientific practice.

Personally, I believe that this presupposition is not true. But Earman is right to point out that it is not shown to be false by my main arguments. For I concentrated on showing that concepts of law which have been elaborated or rationally reconstructed by philosophers are deeply flawed and cannot be playing such a role in scientific practice. That does not rule out that there is another concept—not equatable even with any cluster concept within the scope of the philosophical literature—that does structure scientific practice. But should it be anything like the concept of a law of nature?

I am in strong agreement with Earman's motives. Yes, by all means, let us turn to inquiries into science that are more likely to be illuminating. To that enterprise I tried to contribute with the second half of the book. While symmetries and the conservation laws they engender do not serve to fill the traditional role of laws of nature, they are a far superior clue to the structure of scientific theories and scientific theorizing. They are only one such clue, not a philosopher's stone or alchemist's key, but they guide us to what I call "laws of the model" as opposed to "laws of nature," and *that* I believe to be the right guidance.

It seems to me therefore that Earman and I are at one in our enterprise, enamored of the same philosophical strategy, differing only on tactics. Earman gives reasons for thinking that symmetry by itself will not suffice for our quest. It will allow us to reach much (as he points out with telling examples both from cosmology and quantum mechanics), but not as much as we can hope for. And this I grant.

We differ however with respect to his proposed starting point, the notion of law defined within the Mill-Ramsey-Lewis tradition. The misgivings I have about this are not only ones internal to the philosophical discussion, and certainly not only the ones that I share with Armstrong. In *Laws and Symmetry* (Chapter 3, section 6) I tried to show how very much at odds it is with any tenable view of science. The Mill-Ramsey-Lewis laws purport to be what science aims at finding, but a closer look at science shows that they cannot be that at all. Indeed, to think of science as engaged in that search would not only make it unlikely to succeed, but leave us incapable of judging its success today.

It does not seem to me that we have reached the impasse which Earman sketches at the end. After describing the conditions under which the laws in the sense of David Lewis would *not* be what Earman himself takes science to be after, he writes:

> I take David Lewis to be saying that in our current state of knowledge we have reason to hope that such cases do not in fact arise in the actual world. And I take actual scientific practice to be a practical expression of this hope. I take van Fraassen to be saying that this hope is a vain one. How do we settle the issue? (Earman this issue, p. 418)

What I want to ask here is: "John, if you were to suppose for a moment that Lewis' hope is vain, then would you immediately conclude therefrom that the science we have and are developing is a failure?" Imagine that those unlikely conditions obtain, and science is definitely not on the road to delivering to us the laws of this world in the sense of Lewis. Under that supposition, wouldn't you still say that the science we have is a worthwhile and very successful enterprise, and not a failure? In the larger paragraph I just quoted from, two sets of circumstances are described—one entails that the body of laws of this world, in Lewis' sense, gives little or no information about it. The other entails that whether science can reach those laws is a question that will have different answers for different people. Do you care? Doesn't science in fact succeed in giving us something which is very informative about the world, and which has value independent of anyone's preferred standards of simplicity? As David Lewis himself might say: I think, and so do you...

2. Armstrong and Cartwright's "Whiteheadian" turn

Already by the time *Laws and Symmetry* came out, both Armstrong and Cartwright had begun to focus strongly on causation and causal laws as opposed to laws generally. In his commentary Armstrong shows how for him this is a natural elaboration of his earlier account, and argues that it allows him to escape the "identification or inference" dilemma.

The position that singular causation is a pervasive part of the empirical world, and that we directly perceive causings was ably, perhaps I should say paradigmatically, defended by Alfred North Whitehead in a lecture series at the University of Virginia in 1927, published as his *Symbolism: Its Meaning and Effect.* Its phenomenological analysis of experience is vivid and convincing. Its criticism of Hume is incisive and, to my mind, devastating.

It does describe the character and structure of my immediate experience to say that I see the cat lapping up the milk, and also *see that* the cat is lapping it up. That is phenomenologically very different from the type of experience in which I see the process merely as a sequence of stages continuous in time and space. Indeed, in this example we are probably incapable of the contrasting type of experience if we aren't high on something, or in a yoga or meditation-induced altered state of consciousness. As Sartre and Merleau-Ponty also showed vividly in their discussions of psychological experiments, description of experience in terms of spatio-temporally combined sense-data either misdescribes or over-abstracts from perceptual experience. Like Armstrong and Cartwright today, Whitehead added that perceptual experience not only has the phenomenological character in question, but *discloses* what is there:

The bonds of causal efficacy arise from without us. They disclose the character of the world from which we issue, an inescapable condition round which we shape ourselves. (Whitehead, p. 58)

I do not accept that part of the view. Neither shall I take the contrary metaphysical view that we project this structure onto the perceived world. Before I say more about this very large issue, however, I want to address Armstrong's and Cartwright's more specific arguments.

3. Armstrong on the Identification-or-Inference dilemma

The four-dimensional scenery, says Armstrong, exhibits many regularities, including ones whose instances are causal sequences. To say that a sequence is a causal sequence does not mean that it exhibits a certain kind of structure or instantiates a certain type of regularity. It means rather that it is a sequence of a certain kind of events: causings, so to speak. Perception of causings is, Armstrong says, "as epistemically primitive as any other perception."

An example he gives is that a striking (by a rock, say) causes the glass to shatter. There are many sequences of this type, and we detect in them a pattern of regular succession. In each case we tend to explain what happens by saying that there is a causing (*this* glass shattered because of *that* striking). But what about the regularity exhibited by all these cases, "the same sort of causes bringing about the same sort of effect"? Armstrong continues:

May we not seek to explain this? May we not hypothesize that this uniformity holds *because* something's being F *brings it about* that that same something becomes G? This latter is not a 'general fact' ... Rather it is supposed to be an 'atomic fact', albeit a higher-order fact, a relation between the universals F and G. (Armstrong this issue, p. 422)

To the first question we all reply: *yes*, indeed. To the second I would say: you may so hypothesize, and offer that as explanation, but it is not satisfactory as an explanation. Note what this explanation is:

1. Each instance of striking/being struck causes shattering
2. That (i.e., 1.) is because the universal *striking/being struck* 'brings it about' that what instantiates it shatters, i.e., 'brings along with it' the universal *shattering*

As Armstrong says, we have two relations here. The first is referred to by "causes" in 1, and holds between individual, concrete events. Let us call that relation C_1. The second relation, call it C_2, holds between universals. It is described in two different ways. The first I quoted from Armstrong's paper, the second ("F brings G along with it") from his earlier writings. At first sight, the two relations are very different: for example, it is not the universal *strik-*

ing/being struck, but the event which is the individual striking, that causes the shattering.

My challenge was: please identify that relation C_2; and then show why we should infer that individual F-ings are accompanied by individual G-ings from the fact that F bears C_2 to G. I contended that these requests cannot be jointly honored. But Armstrong continues the above passage with:

> It is at this point that, I claim, the identification problem has been solved. The required relation is the causal relation, the very same relation that is actually experienced in the experience of singular causal relations, now hypothesized to relate types, not tokens. (Armstrong this issue, p. 422)

I suppose it is no use to reply that C_2 cannot be C_1, because they relate different sorts of entities. After all, the words we use to express C_1—"causes," "brings about"—might always have referred to a single relation, C_1-or-C_2. And indeed, since different descriptions of relations need not have different denotata, Armstrong can maintain that $C_1 = (C_1\text{-or-}C_2) = C_2$.

So there is no logical obstacle to this.[2] But the concessions I just made do not suffice to support Armstrong's assertion that the identification and inference problems are thereby simultaneously solved.

Armstrong says there is now no inference problem: "For if a certain type of state of affairs has certain causal effects, how can it not be that the tokens of this type cause tokens of that type of effect?" He really loses me there. If a relation holds between two types, and is the sort of relation that can also hold between their tokens, it still does not follow that their tokens are indeed so related. Romeo and Juliet's fathers hated each other but their children did not.

Suppose F bears C_1 to G; how does it follow that instances of F bear C_1 to instances of G? I imagine that any answer will have to capitalize on what C_1 is, but I don't see what aspect of C_1 could be cited here. Universals bear many relations to each other which do not transfer to their instances—why does this one? My doubt here is not predicated on a disinclination to identify C_1 and C_2; *replace* "C_1" by "C_1-or-C_2," and the problem is just the same.

Secondly, is the identification problem really solved by Armstrong's "postulation that recommends itself because of its explanatory power"? Armstrong holds that I am acquainted with C_1 because it appears in my perception of individual causings. Granting that $C_1 = C_2$, it follows that I am acquainted with C_2 as well. The statement that C_2 is C_1 is certainly an identification, *yes*. But identification by postulate may not be satisfactory in other respects.

Here is an analogy. I have often heard views that appear to universalize responsibility. Chernobyl and Dachau are the shame of the whole human race,

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2 However, philosophical English needs to be changed. For at present, the sentence "The universal *striking/being struck* causes (brings about) the universal *shattering*" appears to be wrong, but turns out to be right if the two described relations are the same. But our terminology can be regimented anew.

not just of the immediate perpetrators; We cannot look at the wretched of the earth without guilt, if we ourselves have what we need. To some extent these views are obviously true, but they can also be pressed to metaphysical extremes. Suppose, however, someone said the following. We are all familiar with the relation expressed by the assertion that each person is responsible for his own actions. Call this relation, which holds at least between a person and his own actions, R_1. It is analytic that if X bears R_1 to Y, and Y is bad, then X is guilty of something bad. Now there is also a relation R_2 between persons and others' actions. Of course there are many such relations, but I have in mind a very specific one. That X bears R_2 to Y explains why X is guilty if Y is bad, although Y was done by someone else. This is puzzling: what relation could that be? The sense of puzzlement disappears (?) if we hypothesize that $R_2 = R_1$.

The objection to this story is *not* that it entails the false conclusion that we are all guilty of every bad act ever done. The objection is rather that $R_2 = R_1$ *appears* to entail that, and is offered as entailing and thereby explaining that putative fact—but fails therein. For in fact, as soon is this identity is asserted, the concept of responsibility loses its moorings, and is set to float freely, ceasing to have the very conceptual stability that tempted us to try and appropriate it to a different role in the first place.

4. Cartwright's location of causings with photons

Most of the stuff of physics—photons, quarks, gravity-waves, even space-time itself—belongs to those parts of its models which do not correspond to anything observable. Some of those parts do not lend themselves to concrete visualization, and some not even to the idea that they represent putative concrete entities (whether visualizable or not): objects, events, processes. Here I am thinking not of elementary particles but of, for example, the space-time manifold and probability measures, both of which are easily found in models, but which I think definitely do not correspond to anything real. (I do not make such autobiographical assertions of disbelief about photons or quarks—that would be irrelevant to the philosophical discussion.) Most of all, of course, I do not think that any hierarchy we may spot in the models will reflect a hierarchical structure of facts, reflecting laws-of-nature versus mere regularity. (See *Laws and Symmetry*, Chapter 9, section 3 and *Quantum Mechanics*, Chapter 1, section 4.)

Nancy Cartwright's *How the Laws of Physics Lie* strongly reinforces my conviction on this point. But now she raises the question: why not allow causings in the models, why think worse of them than of photons?

To me the question is moot. The reason is that, as far as I can see, the models which scientists offer us contain no structure which we can describe as putatively representing causings, or as distinguishing between causings

and similar events which are not causings. Cartwright says that if models contain [parts representing] ordinary objects around us (such as cats, and cats lapping milk) then they contain [parts which represent] causes. The question will still be moot if the causes/non-causes distinction is not recoverable from the model. Some models of group theory contain parts representing shovings of kid brothers by big sisters, but group theory does not provide the wherewithal to distinguish those from shovings of big sisters by kid brothers. The distinction is made outside the theory. If Cartwright herself draws, extra-scientifically, a distinction between causes and non-causes, she can describe models furnished by science in terms of that distinction. But it may be a "hidden variable" description. She may be thinking of the structures scientists use to model data as themselves parts of larger, more articulated structures that carry the distinctions she makes.

Of course, this point is addressed in her later book and recent papers. I won't turn the tables on her by continuing this critical inquiry into the case she makes. Rather I should address the more serious point that the scientific image is a world picture of the sort she and Whitehead describe:

> When science constructs a picture of a bit of the world, the image is far richer... The scientific image of nature is no more devoid of cause and causings than is our everyday experience. The appearance to the contrary arises from looking only at science's abstract statements of law, and not how those are used to describe the world. (Cartwright this issue, p. 426)

Although I think that the main work is done by their assertions that certain models are adequate in certain ways, I do take the point that when scientists describe the world they do so in causal discourse. This is not surprising, since half of science is applied science. The language used even in the other half is adapted from the pre-existing forms of discourse that grew out of our practical dealings with the world. But I do not believe that we can do justice to that fact about scientific discourse by reifying its terms, or devising an ontology of causes, any more than of laws.

Cartwright agrees with my criticisms of what she calls Lewis and Armstrong type programs. But she thinks that I bought into their metaphysical starting point, and that the dilemma to which they fall prey will not affect someone with her different ontology:

> They, all three [Lewis, Armstrong, van Fraassen] start out from the late British empiricist tradition that thinks it has stripped nature of all causings. But Lewis and Armstrong, unlike van Fraassen ... try to do the job of causality with a surrogate they locate in some *non*natural place... They then fall prey to the tension [between] identification and inference.
>
> By now it is clear what I want to say about all this. There is no nature stripped of its causings and there are no demodalized sensible properties ... and the this-worldly causality we have does not suffer from the dual problems of identification and inference. (Cartwright this issue, p. 429)

I am not so sure that the dilemma does not arise for her in some way, *mutatis mutandis*. But I am not at present in a position to outline the next project she challenges me to undertake, of showing "what is wrong with the natural locutions," i.e., her this-worldly causality.

Instead I will do something more constructive and more dangerous. Cut to the quick by her charge (similar to the charge in Kockelmans (1987)) that my empiricism is itself a variety of 18th century metaphysics, I will try to sketch possibilities for a stance on these issues which involves no metaphysics. While I have no settled position on any of the issues I will now discuss, I will try to point to alternatives to both Humean and Whiteheadian metaphysics.

PART TWO. NATURAL PHILOSOPHY WITHOUT METAPHYSICS[3]

There are many ways in which, in my opinion, excessive attention to language has misled philosophy of science in our century. There are points however where I consider recourse to philosophy of language to be imperative. We need to resist the temptation to reify concepts, fostered by uncritical regard for our language in use. The main examples concern properties and universals, modalities of all sorts including probability and necessity, propensities *and causation*. Turning our attention to language does not by itself keep us from entering upon the paths of pre-Kantian metaphysics. We must consistently do philosophy in a new way.

1. Epistemology and perception.

As support for the primacy of singular causation, Armstrong and Cartwright make the same epistemological point. Armstrong says that we "regularly perceive that one thing causes another," and that some such perceptions are "as epistemically primitive as any other perception." Cartwright endorses this with her analogous discussion of "You see me working," and says that "our everyday experience" is not "devoid of cause and causings." This was, as I noted, a main theme of Whitehead's *Symbolism*.

Two things I must grant at once. If the event of my cat (of beloved memory) lapping up its milk is a causing, then I have seen causings. Even on that supposition, of course, it does not follow that I saw that one thing caused another. In *The Scientific Image* I took "observable" to be a term that classifies (putative) concrete entities: objects, events, and processes can be observable, but not properties, propositions, or numbers. The cat I observe, also the lapping; both are observable.

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[3] I have addressed the possibility of empiricism without (pre-Kantian) metaphysics in a very different way in my "Against Naturalized Empiricism." At this moment, I cannot yet unify these two approaches, and I beg the reader to see what follows as a tentative, defeasible try at something more constructive and less defensive, which I hope to improve on in the future .

But do we observe that one thing caused another? If Cartwright teaches me her way of speaking, and I learn to speak it as well as a native, then I will also be able to respond spontaneously to my experience by saying "I just saw that ... caused ...." She will point out, I think, that I was already using causal discourse before I knew her. I did say things like "Aspirin relieves headaches" and "I saw him breaking the window with a stone." My opinion already came expressed irreducibly in causal discourse. This is the second point I grant; I will address it below. What I want to deny here is the metaphysical move which adds: so you observed *something more* than the concrete entities aspirin, stone, glass, breaking, and the person-breaking-glass event.[4] What more could there be to see?

I do not see properties. When I see that the cat is grey, the only object I see is the cat; I do not see greyness or cathood. As Quine pointed out in "On What There Is," the truth of "The cat is grey" does not entail the existence of any property at all. Similarly, the truth of "I see that the cat is grey" does not entail the seeing of any property. It follows of course that "seeing that such and such" is not reducible to "seeing thing 1, event 2, process 3, ..." Rather than continue the epistemological discussion here, let us look more closely at the special case of causal discourse.[5]

2. A telling analogy for causal discourse

Cartwright quite rightly points out that causal discourse is irreducible. That is also true of psychological discourse, what is sometimes so quaintly called "folk psychology": discourse that mobilizes the concepts of person, intention, goal, purpose, value, emotion, thinking, perceiving, meaning, saying, acting, loving, community, and so forth. This is irreducible not only to physicalist discourse, but also to the hygienic discourse of the Humean belief and desire psychology, which we find unblushingly used in so much philosophy of mind today.

This irreducibility does not preclude eliminative materialism. Paul Churchland, for example, is famous for the conviction that this ordinary psychological discourse is deeply infected with bad and outdated theory. This conviction can also be expressed in the material mode: beliefs, desires, intentions, and emotions do not exist, any more than phlogiston.

Let us imagine an opponent of eliminative materialism who argues as Cartwright does about causes. She will first of all point out that our perception of persons does not have a lesser epistemological status than seeing

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4 I cannot join Cartwright in dividing occurrences into modalized and demodalized—that makes sense in her ontology and in Humean ontologies, and so on, but not for me. For me occurrences are not modal or non-modal, but only *described* by modal and non-modal language.

5 See further my elaboration of the concept of observability in "From vicious circle to infinite regress, and back again."

rocks or trees. Moreover, the shape our experience takes is intimately tied to psychological categories: we see that someone is angry, we see people work, wash dishes, make coffee to quench their thirst. To say that we see them pursuing and realizing goals is only a slightly more abstract way of putting that.

This imaginary opponent of eliminative materialism might then say that there is no good or intelligible analogue to Hume worlds. A world which is just like ours except that intentions do not exist in it, would not be just like ours. For example, John Earman would not exist in that world, because he is a person, and nothing can be a person unless it has intentions. None of the persons we know would exist in such a world. Obviously the eliminative materialist would respond that John Earman would exist there, but that to call him a person is to apply a defective concept. Our current way of describing him is irreducible to non-psychological discourse, but that does not affect him or his existence.

I do not agree with eliminative materialism. Churchland argued in *Scientific Realism and the Plasticity of Mind* that the whole of our language used for all purposes of expression and communication, can in principle and without loss be replaced by a language devoid of folk-psychological modes of discourse. One can imagine a similarly radical view about causal discourse. In fact I do not propose that view either with respect to causal or psychological discourse. In my opinion, the loss would be severe in both cases. But the status I assign to both (and note: they overlap considerably) makes them important for everyday life and applied science, but not for theoretical science.[6]

It may be a good idea therefore to reflect for a moment on how it is possible to resist an eliminative materialist. It is no use saying to him that our world is so rich that he cannot describe it adequately. For he will only counter that our modes of thinking, speaking, and describing are too rich for him, while the world we live in is the same. What he loses he counts well-lost. We had better admit that within the goals he can set or even express for himself, his language is adequate. But that is where we locate his loss. We can resist him if we can grant his adequacy unto himself, and to the many interactions with us which he can conceive, without feeling that we thereby indict or undermine our own position. We are in no way diminished by our inability to convict him of loss in his own terms, or by the respect we show for his position as adequate by his own lights. Cartwright may similarly say that at best, she will find my position coherent but impoverished in comparison

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6 This does not imply that pure science is in principle incomplete, in the sense that there are, so to speak, facts which cannot be scientifically described. For example, Rorty's and Churchland's eliminative materialism derive from the position of Sellars, who took it that the language of physics is in principle sufficient for all factual description, but needs to be supplemented with resources for the expression of intentions. See further my "From vicious circle ..." and *Quantum Mechanics*, pp. 465–66.

to her own. Yet I feel that in philosophy of physics, our concerns are shared to such an extent that she will feel the need to establish more than that.[7]

3. Causal discourse: a cue from Collingwood

The example of psychological discourse gave us a useful analogy. Now I want to go further: I submit that all the need we feel for causal discourse comes from the way we think of ourselves as persons, agents with goals and intentions, engaged in effective action. This covers the entire subject of applied science and therefore also such topics as prediction and explanation (which I classify as applications of science), as well as control and manipulation. Each of those so-called causal terms is, in its primary use, a psychological as well as a causal word. (The eliminative materialist eliminates also the concept of applied science and all its cognates.)

While I could cite Whitehead's *Symbolism* as the paradigmatic case made for the world-picture in which causation is fundamental, so I can point to Collingwood's *Essay on Metaphysics* for my own. (I do not mean that I accept Collingwood's position, any more than I meant to classify Armstrong and Cartwright as disciples of Whitehead.) The Whiteheadian sort of world picture typically comes with a certain hierarchy. I relieve my headache by taking aspirin: that is an event 'on the surface.' This is a causing: my action caused relief. But that was so because aspirin relieves headaches (one level down). That kind of causing in turn derives from a 'lower' one: presence of asalicyclic acid in the bloodstream causes the vessels to dilate. That in turn derives from causings which are chemical interactions, and those from something more fundamental yet: causal connections between events involving individual molecules, atoms, electrons and protons, quarks. The surface and in general higher level events are causings derivatively, *because* of the causings going on at the most fundamental level. The most important concept of causation is that of causal connection between fundamental physical events.

Collingwood proposed to turn the hierarchy upside down. He was convinced by Russell's writings that causation is found nowhere in the most fundamental description of nature by modern physics. When physicists start describing the world of physics to laymen, they will use language which is a metaphorical and analogical extension of the discourse of applied science and everyday life, i. e., human agency. The most basic causal discourse is that of folk psychology, in which "I relieved my headache by taking aspirin" signals that I engaged in intentional action, had goals, made choices, found my expectations fulfilled. This description of what happened is not reducible, as far

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7 In philosophy of mind I do not have the same sense of shared concerns with eliminative materialists. That is all the more worrying because it concerns matters much closer to our hearts than theoretical physics.

as meaning goes, to the language of physics—nor to any language devoid of concepts cognate to personhood.

Except for derivative uses and analogical extensions, causal discourse is part of psychological (intentional) discourse. That aspirin relieves headaches is not a fact of physics. We assert it because we relieve headaches by means of aspirin. That asalicyclic acid dilates—causes to dilate—the blood vessels, is not a fact of physics. We assert that because we can bring about the dilation by doing something which places that substance in the blood stream. On the earlier hierarchy of course, this "bring about" is a relation which in the first instance relates purely physical happenings. On Collingwood's view, it carries its basic meaning only when the subject term denotes a person. That the rock's striking the glass brought about the shattering is a metaphor [metaphorically imputing agency to the rock] or an analogy [to a person shattering the glass by doing something to it] or derivative from the more basic assertion that we shatter glass in various ways [of which throwing rocks at it is one].

If Collingwood is right then a lot of literature on action theory looks very perverse. Apparently the authors thought they could dissect what a person does into elements describable in psychological language (he intended, ...) and elements describable in non-psychological, even physicalist, terms (he brought it about that ...). But the dissection was illusory, for the latter terms do not belong to a purely physicalist language either.

If Collingwood is right, then it is also easy to understand why such analyses of causation as Reichenbach, Suppes, and Salmon have offered just don't succeed. Nancy Cartwright is one of the authors who has exhibited their failures most strikingly. Perhaps they did come up with good notions of causal models, or of the nearest there is to causation in the models of physics—but that is doomed to fall short of the real subject of causation.

I realize that this idea of Collingwood's is not much more than an idea. I do not have the wherewithal to carry it beyond that stage. I am not in a position to write the book Nancy Cartwright asked for. But the above sketches how I would like to write that book if I could.

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