

PROSPECTS FOR CAUSAL EXPLANATION
OUTSIDE OF MECHANISM

by

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This work is an attack on the standard neo-Humean conception of science in philosophy of science. The following pillars of this conception are targets of the critique:

(1) the claim that the role of causality in scientific explanation has been, or can be, superceded by scientific laws;

(2) the belief that a causal process can only be a disconnected, perhaps contiguous, series of events.

In addition to attacking these explicit views, the work challenges the background assumption that causal explanations for basic phenomena of physics have proved permanently impossible, leaving, henceforth and forever, laws and only laws as material for theoretical knowledge in a fundamental domain of science.

Examination of other contemporary causal realist philosophers shows that their contributions to an understanding of unobservable causes in physics have severe limitations, due to their retention of key Empiricist conclusions.

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DEDICATION

This work is dedicated to
the memory of
Michael J. Moravcsik
1928-1989
and to his family.

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INTRODUCTION

The discipline of philosophy of science in the twentieth century has been decisively influenced in its procedural doctrines and conceptions of science by dramatic developments in physics. These developments can be summarized in two parts: (a) the breakdown of mechanistic models (models involving matter in motion) for the unobservable causal processes presumed to underlie phenomena of electricity, magnetism, and optics, together with the abandonment of the idea of an "ether" as a material substratum for these processes; and (b) a shift in the meaning of "theory," begun in the last century, through which this term has come to mean some form of mathematical procedure for predicting the results of measurement under given quantitative conditions. Such theory is only concerned with correlations among data of measurement, and is independent of any inquiry about the nature of causal processes underlying the correlations.

Philosophy of science responded to these developments as if they meant that David Hume's views about causality and about science had been confirmed and established for all time. Hume had concluded that "causation" could only consist of reliable successions in observed events plus our mental reaction to such regularities. He accordingly thought that the permanent results of science would consist of knowledge

of these regularities--in a word, of laws. Thus recent physics in its actual course seemed to bear out Hume's conclusions.

The position taken in this work is that the basic conclusions of neo-Humean philosophy of science, and the philosophical projects resulting from these conclusions, are misguided.

One traditional neo-Humean project has been the effort to show that scientific explanation of phenomena can and does consist purely of descriptions of observable circumstances and knowledge of the applicable laws. Inasmuch as the motivation for this can be traced to the influence of physics and the contemporary character of its theories, this effort involves the suppressed premise that such theories are in fact explanations of phenomena. Chapter 1 of this work evaluates the arguments and conclusions of this law-based theory of scientific explanation, with attention to this suppressed premise, and finds the whole enterprise unconvincing.

Chapter 2 examines some explicit arguments given for the adoption of the Humean analysis of causality for application to contemporary science. These arguments are found to be based on an unnecessarily constraining presupposition about how events forming successions (in general) can and cannot be described. It will be argued in the long run that the endemic critical and skeptical qualms about the reality and character of causal connection result from the belief that suc-

cessive events which form parts of a single process must bear to one another the relation of contiguity, as in side-by-side objects or adjacent segments of spatial length. I claim that contiguity is an artificial and inaccurate reduction of the causal relation, despite the fact that it continually interposes itself in philosophical thinking. As an unerring alternative to contiguity between events, I propose to develop an understanding of causality on the model of A. N. Whitehead's theory of natural "creative" process as genetic ¹ succession. The proposal is to be devoid of the teleology that Whitehead thought was essential to the idea, and also seeks to avoid the application of biological concepts to non-living Nature, which is a troubling feature of Whitehead's later work when it is considered under the aspect of physical explanation. The concept of causality will be developed not only to overcome the reduction of causal connection to contiguity, but also as a concept of physical process for eventual application in an array of causal explanations which, I contend, have so far radically eluded science. Chapter 2 nourishes the beginning critical arguments for this project. It also examines and rejects the claim that indeterminism in atomic physics bolsters the neo-Humean position on causality.

Chapters 3 and 4 discuss some contemporary philosophers of science who attempt to counteract the neo-Humean view, or at least its extreme tendencies, by in some way attributing causal reality to that which is both inherently outside the

category of observable events and by all appearance not amenable to mechanistic models, namely, what physicists designate with such terms as "field," "radiation," "subatomic particle." These realist attempts are found to be seriously hampered in different ways by a heritage of Empiricism in science and philosophy.

Chapter 5 acknowledges the problems disclosed by recent physical investigation which appear to stand in the way of causal explanation for the domain of physics, and indicates the strategy by which the most prominent among these might be surmounted.

The overall claim to be argued for is that the recent transformation in physics has signaled neither a change in the meaning of explanation in science, nor a change in the general aims of science and the criteria for what is scientific, but has instead been a shift in the orientation of physics toward technological research and away from explanation, and that this historical shift leaves entirely open the question whether causal explanations are possible for this domain and may legitimately be pursued.

Notes

1. Alfred North Whitehead, Process and Reality (New York: Macmillan, 1929); also Whitehead, Science and the Modern World, (New York: Macmillan, 1925) 107-110.

CHAPTER 1

"LAW EXPLANATION"

The basic tenets of the recent tradition of philosophy of science have formed around the conclusion that scientific knowledge in its final form is not primarily knowledge of the causes in Nature for things observed. An important basis for this conclusion is that what is called "theory" in physics has come to have only the content of mathematical laws without a corresponding content of causal stories (which make up, for instance, theories of biological evolution and geological transformation, or the pre-twentieth century theories of light). As a result it has become the standard view that exact predictive knowledge of correlations in the data of observation, as opposed to causal explanation, is the essence of science.

This tradition was kicked off by the Vienna Circle of Positivism, which welded together two elements: (a) the Humean critique of causal connection in general; and (b) an adoption of the practical physicist's analysis of experimental data--formulae for quantitative correlations--as the model for advanced science, consigning to a more primitive stage of science the sort of causal inquiry represented, e.g., by the older theories of light as waves in the ether. An acausal

view of science was forged out of these elements.

Philipp Frank, a philosopher of science with direct ties to the original Vienna Circle, says the following about the transformation in physics which began in the late nineteenth century:¹

[The theoretical models of Maxwell and Lorentz for electromagnetic phenomena] were accepted only because the observed facts about the motion of bodies and the propagation of light could be derived...Principles of physics were accepted if they could stand the test of logical consistency and empirical confirmation. The era of mechanistic physics was reaching its end, and the era of logico-empiricist physics was beginning. Roughly speaking, we may say that the mechanistic era had extended from 1600 to 1900, and that the twentieth-century opened with the logico-empiricist conception of science in the making.

What Frank means at a minimum by "mechanistic" science here are causal theories of light and electricity which involve motion of matter, such as vibrational disturbances or spinning vortices in the all-pervading ether. But "the logico-empiricist conception of science" suggests a generalization not only beyond this particular area of physics, but beyond physics to science.

But his claim about the end of mechanistic science clearly cannot be generalized over all of science. Even if he means "mechanistic" in the strict sense of causal explanations involving matter undergoing motion and stress it would not be correct to include, for example, geology in the new conception as he specifies it, since in this science such

explanations have in no sense met their demise.

It cannot uncontroversially be claimed that explanation in biology, e.g., the theory of natural selection, is mechanistic in this sense. But if Frank is using "mechanistic" in a broader sense, to mean any causal story about a continuous series of events terminating in things observed, then the "logico-empiricist" conception of science must exclude biology as well. Integral to both of these sciences are narrative accounts of natural processes which have brought about existing conditions, and this fact is unaffected by any changes in the meaning of a "theory" in physics.

Throughout this work I will use the terms "mechanism" and "mechanistic" in the narrow sense, as causation which is reducible to the elements matter, motion, stress, and collision. To designate a broader concept of causal narrative explanation, a concept to be gradually developed, which may--and, I will be arguing, does--extend beyond mechanism, I will reserve the term "productionism." A "productionistic" explanation is a causal explanation of which the only essential ingredients are continuous series of events, bringing about one another and arising out of one another in succession, and terminating in the fact which the particular story explains. The causal process described may or may not be mechanistic.

The basis of Frank's claim about the passing of mechanistic science is merely that physics gave up on causal explanations for electromagnetic phenomena. But in light of the

actual philosophical views of science which are represented in the term "logico-empiricism," Frank intends just the generalization over all of science which, I have noted, appears untenable. What Frank says comes from the Positivist striving to "reconstruct" the whole of science as a logically interconnected system of lawlike propositions--which is to say, of conceiving the content of the whole scientific account of the world by analogy with modern physics, whose theories have come to consist entirely of mathematical laws usefully formulating the dependence upon one another of quantitative variables in specified contexts of measurement. Positivists thought that such a single system of science, with its well-defined allowable content of laws, would establish the clear partitioning of scientific knowledge as against metaphysical speculation and superstitious or otherwise unfounded popular beliefs, which they regarded as intellectual and social evils.²

Whatever the fate of this striving for a single system of science, there should be no disputing that Frank puts his finger on something important about the history of physics. About the two distinct eras of this science, divided roughly at the turn of the century, at least this much might be said uncontroversially: there was in the earlier era a living expectation among physicists that the regularities which their equations quantify, notably those in the context of experimentation with light, electricity, and magnetism, had

their explanations in underlying physical processes whose description in ordinary language might some day be completed. But in the new era this particular science came to have the whole of its progress and success in predictive equations developing in synchrony with experimental technology, while the prospect of a narrative description of the causal processes exploited in this technology floundered utterly amid philosophical turmoil. This was not so much a smooth and natural maturing of knowledge, as Frank suggests, but more like a difficult birth, namely, of the pragmatic renunciation of a traditional quest for narrative and naturalistic explanation, down to its last remnants.

Frank's "logico-empiricist" (which I will call "acausal-ist") conception of science sees as genuine and permanent results of science only systematically organized, lawlike propositions about observed regularities. From this point of view a pervasively causal-narrative approach to explanation, which I will in the long run be arguing for, will be seen as an errant nostalgia for "mechanistic" science, even though my aim is explicitly to conceive of causality as extending beyond the constraints of mechanism (in the narrow sense).

Whether explanations in physics can be furthered outside the mechanistic mode through a transformed conception of natural productive activity remains to be seen. This aside, the logico-empiricist conception may pose a challenge to my assumption that to produce explanations in science is to

produce causal explanations. A consideration of this possible challenge is the primary concern of this chapter.

Suppose we relieve logico-empiricism of the problematic aspect which I have made note of, by restricting our attention to physics and setting aside the hope for "unified science" on their model. Suppose further that the logico-empiricists can produce a sense of "explanation" which can apply to the domain of physics in accordance with their position--that not causality, but applicable laws, will be its essence. If this meaning of "explanation" is clear and convincing enough, they may have a case against my view that, where narrative accounts of physical processes are not produced, the explanatory dimension of science is lacking. They could claim that it is open to question whether causal explanations need to be produced here as in the rest of science in order for there to be genuine explanation of known phenomena. Perhaps physics just as it is displays the real, final, and complete character of scientific explanation, through its status as a foundational science, as the one, true, "unified" explanation of Nature!

And in fact the logico-empiricist conception of science, which is to say the standard outlook of philosophy of science, has been deeply occupied with the problem of explanation, with the construction of models of explanation based on laws and the criticism of those models. I will examine a classic text of logico-empiricist explanation theory after

first providing some background discussion.

Causal Understanding versus Mastery of the Regularity

As I said, much of the recent tradition of philosophy of science is under the influence of the Positivist movement. Their view was that a description of the causes of observations was by itself only a crude, antecedent form of science. Such an explanation does not rise to the contemporary standard achieved by the paradigmatic science: physics. The material for genuine scientific explanations consists, on this view, of precisely that content of theories of physics which remained and continued to succeed: laws. Scientific laws are equations calculating the dependence, within specific contexts of measurement, of some quantitative variables upon other quantitative variables, or the propositional form of such equations. Thus, laws formulate with exactitude regularities, which means certain sets of measured quantities which are observed to vary as a function of one another.

A classic example of the difference between laws and causal explanations is provided by Newton's successful formulation of, and subsequent reflection upon, the law of gravitation. The law states that the force of gravitational attraction between two bodies is directly proportional to their masses and inversely proportional to the square of the distance between them. Having established this universal law on the basis of observations of planetary motions, Newton

deemed this the limit of science on the matter, and insisted he would "feign no hypotheses" (the famous hypotheses non fingo)³ about the cause of gravity. Whether or not one thinks there is any legitimate question about the cause of gravitational attraction and of the functional dependence that the law identifies, it can be seen here that Newton recognized that a law organizes quantitative features of a class of observations (in this case of a specific force of attraction) without being or suggesting a causal explanation of these observations.

It is a fact much commented upon by philosophers that precise, predictive knowledge of regular correlations among observed events is independent of the knowledge or lack of knowledge of any causal processes lying behind the correlations. Inquiring into the connecting process, if any, which gives rise to such a regular correlation can be a concern wholly separate from the formulation of its law and the use of this law as a predictive tool, or as a template for precision design, in experimental research. That laws alone without corresponding causal explanations not only can play a central role in the development of science, but can seem to embody a complete and independent knowledge of the phenomena, has been basic to Empiricist physics and philosophy.

This independence of successful laws from knowledge of causes has traditionally harbored two related forms of epistemological skepticism, which I call the two forms of acaus-

alism.

The first, famous in David Hume, is the view that apart from regular succession in separate events there is nothing in Nature which can be called causal connection.⁴ On this view the inscription of a regularity in the mind (or in an equation) is all that can be meant by saying that one event "causes" another. For contrast, it might be supposed that there are not just spatially and temporally separate events (in physics experiments and elsewhere) but that there are also real and physical causal connections. To claim, as the present work will, that unreduced causal narrative is essential to scientific understanding of the world, is to maintain the supposition that there is more to causality than observed regularity in events, namely, there are connections which can be spoken of in general as the "production" of one event by another and which are made clear in a causal narrative account of how something--say, a mountain range--is brought about. The arguments typically given in support of the Humean view of causality are examined in the next chapter.

The second form of acausalism is the view that there is no justification for believing in the existence of the many kinds of unobservable causes in a special class investigated by modern physics--that is, what are called "particles," "radiation," and by some other designations, and include particular species such as electrons and photons--because they are inherently unobservable. Behind this conclusion

lies the view that a body of scientific knowledge is only a systematization of facts of observation, so that all that is available to comprise scientific explanations of observed events are the measured quantitative interdependencies found in these events, rendered in lawlike propositions or convenient algorithms. In other words, this conclusion is one result of what Frank calls "logico-empiricism," an acausal view of science in general which is modelled after contemporary physics.

As is the case with the first form of acausalism, one can take a position contrary to this fictionalist view of unobservables, and furthermore, this position has considerable weight of commonsense expectation. Innocently of the arguments for this view, it might well be thought (and this is in kinship with the physics of the "mechanistic" era) that there are some occurrences in Nature giving rise, for example, to forces of attraction--Newton's "cause" of gravity--and that there are propagations, consisting of physical events, intervening between what are called "emitters" and "detectors" of radiation. The presumption would be that knowledge not only of the existence, but of the nature of these physical processes should explain the specific features of what is observed and measured.

The Humean view and the view of the antirealist about causal "entities" in the domain of physics might be combined and referred to as a "regularist" view about causality, in

contrast to the prephilosophical "productionist" view, which will not cease to expect that real causal connections pervade Nature and can be successfully described in all detail as physical events. My use of the term "acausalism" is within the productionist standpoint. The regularist is free, if he does not think that "causality" is an entirely empty concept, to claim that it just means regularity in observed and discrete events, in which case he would not see his view as an acausalism. The productionist, on the other hand, claims that to throw out the reality of connection in the relationship of production is to throw out causality.

There is potent fuel for the view which Frank voices, the view that science on a modern conception does not explain the world in terms of causes, and it has the following ingredients: First, there is the fact that what are called "theories" in twentieth-century physics, such as the theories of relativity and quantum mechanics, have the character of predictive models or laws, rather than being descriptions of sequences in natural causal events. Second, physics is taken as a paradigm of science, due to its dramatically impressive intellectual and technological successes, and there is the inclination to suppose that the successes of this science must mean that it achieves explanation of phenomena and hence an understanding of the domain of nature with which it is concerned. These ingredients combine into the conclusion that this fundamental science, and perhaps the whole of sci-

ence as a unified system of knowledge, truly explains not by the adducement of causes, but through the successful formulation of algorithms and laws providing predictive capability.

Clarifying this reasoning exposes a serious non sequitur. There is an unargued assumption here that any technologically successful science must be a concomitant achievement in explanation, though there is no real basis in the character of what physics serves up through its theoretical inventions that this "theory" constitutes explanation or understanding of Nature. Remember hypotheses non fingo: why think that to have discovered a law of Nature is to know the explanation for the phenomenon whose measureable features it formulates? From the productionist point of view, the very fact of the acausal character of contemporary theories just suggests that they do not themselves explain, however powerful their utility. The claim that such theories are artifices and not explanations is quite defensible, just because of a well-known independence of laws from causes. An important argument in defense of this claim is given in the next section.

Instrumentalism and Antirealism: Breaking the Link

The instrumentalist view of theories which, as I said, fits with the productionist view about causality, was part of the Positivist view of science, where it was associated with

antirealism about unobservables. The reason for this association was as follows. Given the assumption, characteristic of Empiricism, that it is theory that produces the first knowledge of the unobservable, then if such theory is only an instrument of prediction, this would render the unobservable entity a construct. In a recent statement of this conclusion, Lawrence Sklar says that instrumentalism "has consequences too irrealist for many to swallow."⁵

Instrumentalism about theories has become unpopular, because this association was not broken, and philosophers wanted to think that what on the face of things appear to be real entities (where this refers, safely, to species of causal propagation) studied and named by physicists, of which specific causal properties are mastered in a variety of applications, are in fact such real entities.

The claim that the electron and the photon are not known by science to exist but are instead useful fictions, though it has long had and continues to have major proponents, is unacceptable to many philosophers. As for Positivists, it was not that they necessarily believed electrons were fictitious, if they were to state their view of the matter "off the cuff." Nor were they necessarily explicitly instrumentalist about theories. But their Empiricism, in particular the focus on mathematical laws, irresistibly suggested both. Rudolf Carnap's Empiricist arguments provide an illustration.⁶ He starts out a passage intending to show the falsity of the

view that mathematical theories are "'mere formalistic constructions,' 'mere calculi.'" He claims a theory must have in addition an interpretation which is an understanding of Nature. But his discussion ends up with an "understanding" for theories in the domain of the electron which is actually no understanding, but only utility. This is the "kind of understanding which alone is essential in the field of knowledge and science."

A resurgence of realism in philosophy of science helped to displace Positivism and along with it instrumentalism. But I want to raise the possibility that instrumentalism may represent a clarity that was thereby lost.

In the work of the contemporary philosophers of science Ian Hacking and Nancy Cartwright, important arguments have been put forth which tend to break the association of instrumentalism with antirealism. Cartwright makes the point that in the identification of causes, while there may be several incorrect causal hypotheses, there is only one correct one, whereas it is often the case in physics that different mathematical models have equal applicability to the same phenomenon, with no one of these being the "correct" one.⁷ This indicates that theory in the modern sense may not be what identifies causes at all, and that the disclosure and naming of entities efficacious in experiments may actually be a process other than theorizing.

And why should the electron (for instance) be permanent-

ly a "theoretical entity"? The atom of electric charge was "theoretical"--which is to say, a hypothesis--at one time in physics, but the subsequent history is that this hypothesis was experimentally verified by the Millikan oil-drop experiment, which can be called the detection of the irreducible unit of charge through measurement. Should we take physicist's own account of the history of their science to be philosophically naive? How then could it be said that the experiment showed anything?

There is a perfectly good candidate other than theory construction for what identifies and differentiates "particles," namely, the exactly specified experimental conditions under which their characteristic effects are observed. This is how I understand Hacking's critique of antirealism. Hacking presents the matter from the standpoint of the experimental physicist who is making use of unobservable transmission ("particles") in his investigations.⁸ He knows the transmission consists of positrons because he has designed just the emitter that emits positrons, and what is useful to him is that this kind of transmission has a set of real and highly specific causal properties or "powers." This basis for the reality and specific identity of the unobservable is not a matter of theory construction.

In these ways Cartwright and Hacking make a cogent case at least that the question of the reality of unobservables is separable from the question of the status of theories, so

that one need not reject instrumentalism about theories in order to avoid antirealism. If theories in physics are not descriptions of the world, this may simply mean (and I think it is quite plausibly the case) that the unobservables of physics remain undescribed in spite of thorough quantitative knowledge of the conditions of their occurrence and of their characteristic effects. This removes the pressure to think that, if the entities are real, then the theories in question must be some kind of explanations.

The "Covering Law" Model

The "logico-empiricist" conception of science, however, is an acausalism combining Hume with the conclusion which I have been critiquing, that physics establishes a standard for scientific knowledge by explaining acausally. This leads to the conclusion that an acausal theory of scientific explanation in general is needed. This, as I have already remarked, seems strange in light of the evident fact that other branches of science, such as geology and evolutionary biology, explain causally. There is intellectual resistance here to the possibility that physics may be unique among the sciences in that, though it treats with highly efficient mathematical models the regularities in detailed events which it encounters by experiment, it does not, in a sense basic to natural science, explain them. But there is nothing incongruous in this possibility. The thrust of my argument at present is to

set alongside standard acausalism the possibility that physics as it is today is a technological science rather than an explanatory science, and that its "theories" are simply artifices of mathematical ingenuity with which practical research equips itself.

The law-based model of explanation which I will presently consider is one outcome of the acausalist tradition, transmitted through Positivism. This law-based model represents the influential view that, whatever we may think is the meaning of causal accounts, a genuinely scientific explanation is comprised of laws--statements of correlation between discrete events. Philosophers who have committed themselves, for the reasons I have discussed, to such an acausal theory of scientific explanation, have endeavored to develop a meaning for "explanation" by which it contrasts explicitly with "description," so that explanation of phenomena can be a matter of the laws covering them, superceding the idea that descriptions of causal events can themselves constitute explanations which are fully scientific. Through a formal definition, they thought, "explanation" could be given a meaning confined to the citing of a law or laws specific to the circumstances of the thing to be explained.

The standard account of explanation according to this prescription is found in a famous paper by Carl G. Hempel and Paul Oppenheim on "The Logic of Explanation."⁹ These authors describe explanation as involving a kind of syllogism. Sup-

pose we have an occurrence or fact we wish to explain. We take as one premise a proposition describing in adequate detail the circumstances surrounding the occurrence or fact; we take as another premise the law stating what will come about in those exact circumstances--namely what we are trying to explain; and there follows deductively a proposition stating that what we are explaining is indeed the case or does occur. A law of science thus becomes a conditional statement logically implying the phenomenon to be explained. This is the "covering law" model of explanation, in which a law or laws are the operative features. The syllogism has the form:

Premise (1): the covering generalization or law, an "if, then" statement specifying a set of conditions and telling what happens when these conditions obtain;

Premise (2): a statement asserting the occurrence or actuality of these "initial" or given conditions;

Conclusion: a statement that the particular thing being explained does in fact occur.

In short, to explain is to utilize known scientific laws to set into action some deductive machinery which yields the thing-to-be-explained. To be thus generated deductively from a covering law is to be explained.

An initial response one might have to this claim is to ask, "Why does this deduction of something from a covering law (plus initial conditions) explain the thing? Does this not simply amount to saying, 'It always occurs in those cir-

cumstances?' What is explanatory about that?" A background view, already alluded to, is what seems to alleviate this problem in the minds of the proponents: a view of science as a total interconnected system of lawlike propositions. With this picture, what explains a phenomenon is not just that one law. Its covering law explains the phenomenon with the backing of the whole system of laws, once disclosed (I will return to this).

Hempel and Oppenheim offer examples of explanation which might come up in daily life and which they claim illustrate this "covering law" or "deductive-nomological" model. There is an introductory passage in their paper intended to drive home the distinction between description and explanation:

To explain the phenomena in the world of our experience, to answer the question "why?" rather than only the question "what?", is one of the foremost objectives of all rational inquiry, and especially, scientific research in its various branches strives to go beyond a mere description of its subject matter by providing an explanation of the phenomena it investigates.

Then the following example is supplied to illustrate the deductive model:

A mercury thermometer is rapidly immersed in hot water; there occurs a temporary drop of the mercury column, which is then followed by a swift rise. How is this phenomenon to be explained? The increase in temperature affects at first only the glass tube of the thermometer; it expands and thus provides a larger space for the mercury inside, whose surface therefore drops. As soon as by heat conduction the rise in temperature reaches the mercury, however, the latter expands, and as

its coefficient of expansion is considerably larger than that of glass, a rise of the mercury level results.

The authors then attempt to identify the features of the deductive model which occur in this explanation. It consists, they tell us, of two kinds of statements: a description of some antecedent or co-occurring conditions--a mercury thermometer is dipped in hot water--and some laws covering the case. Specifically, these laws "include the laws of the thermic expansion of mercury and of glass, and a statement about the small thermic conductivity of glass."

It is not clear that these laws--if they are laws--are central to the explanation presented. The "small thermic conductivity of glass" is simply not mentioned in the text as part of the explanation. The coefficients of expansion of mercury and glass are mentioned. But, to start with one of the problems, why should these coefficients be called "laws"? They are quantitative properties of substances; at least in this sense they are "regularities"--always the same for a given substance. But this "regularity" is not a variation in quantity A as a function of some other quantity B. It seems unlikely that physicists would have a reason to refer to these quantitative properties as "laws."

But let's try to take them as laws in the sense which Hempel and Oppenheim would need for their model. The model calls for the conditional, in this case, which says, "If this substance is mercury, then it will expand a specific amount

with a given rise in temperature." Together with the corresponding conditional for glass, and given the initial conditions involving a thermometer, the coefficients which are in fact applicable to this case are deductively (trivially) generated. Now, the explanation under discussion does not say what these coefficients are. It only says that one is "considerably larger" than the other. What is used in the explanation, the authors claim, is the "considerable" difference of the two.

These coefficients are brought up in the explanation of why the mercury level rises after the brief dip in the level--the original thing to be explained--occurs: "...as its coefficient of expansion is considerably more than that of glass, a rise of the mercury level results." The authors can be taken to be explaining, by this difference of coefficients, how the thermometer works, telling why the mercury level can be seen to change at all.

But it seems erroneous to take this difference to be the principle of the thermometer, which is more like this: The space in which the mercury can expand is flattened, as far as is practical, in one of its dimensions. An increase in volume cannot spread in this one dimension, so that the expansion along the length of the space will be observable for a usefully small variation in temperature. The truth in what Hempel and Oppenheim say about the coefficients of expansion is that it makes sense to use a liquid which ex-

pands to a relatively great degree. But for all we know given their explanation, if enough flattening of the expansion space were achieved, a moderately useful thermometer might be made in which the coefficients of expansion were not "considerably" different. It is more a matter of geometry than of exact properties of materials. If one thinks carefully about it, one does not find that these numbers explain, but rather, to take them as the explanation only draws one into questions. The point is that these coefficients of expansion by themselves are not a very appropriate centerpiece for an explanation of the thermometer.

And as I already mentioned, the thing-to-be-explained is the brief dip of the mercury, and not just its usual rise.

Hempel and Oppenheim aim to give a scientific explanation. They want to explain observations, and not merely to describe them as any ordinary observer could--in this way they seek what is both explanatory and scientific. As a result they take as the very heart of the explanation some scientific-sounding things, coefficients of expansion, thought of as laws in order to fit the covering law model. But our examination seems to have arrived at the question: Why should it be thought necessary at all for the explanation to talk about these coefficients? This talk sounds slick and scientific, but to try to decide whether these quantities are actually important to explaining either the dip, or how a thermometer works, is deeply confusing. This consequence

does not especially favor the purported explanation.

Going back to the starting point in this text: After describing the mercury dip phenomenon, they ask, "How is this phenomenon to be explained?" Note that they do not specify that "explain" has any specially scientific or specially philosophical sense. They just ask how it is to be explained. Now, despite the problematic character of the explanation and commentary together, we were given to understand why it happens. So let's try to put it into our own words. What is the explanation?

The heat from the water, of course, reaches the glass before it reaches the mercury. The expansion of the glass bulb slightly increases the volume for the mercury filling it, drawing the column down slightly. There is the explanation. With it, I understand how and why this happens. It does not make use of any scientific laws. But then, it does not fill the bill for an acausal sense of explanation. It is simply a narrative account of the mechanism involved.

To be fair to the Hempel and Oppenheim explanation and commentary, the unmentioned "law" about the "small thermic conductivity of glass" should be considered. This seems to get something right. The heat takes a certain interval of time to pass through the glass, and this is important to the explanation. But the exact heat conducting properties of glass as compared with other materials is no centrally important fact in the explanation--or at least, it is not clear

why it should be. To take this specific quantitative comparison to be the explanation would lead to some puzzlement: Does this mean that the dip would not occur at all if the thermometer had a bulb made, say, of metal? If what we were explaining to begin with was an observation that a dip occurred with glass but not with metal, then a comparison of conductivities would be just the thing wanted. But in the case at hand, the only relevant fact along these lines, and it is understood in the narrative explanation we came up with, is that conduction through the glass takes enough of an interval of time to produce an observable effect. The specific conductivity of glass is not really to the point in explaining the phenomenon.

We are left with the simple causal story above as a solid version of the explanation, one which does not need any scientific references or lend itself to any philosophical theory.

(Along the same lines as my promotion of narrative causal explanation in physical science, William Dray¹⁰ rejects the covering law model for explanation in history, and proposes instead the "model of continuous series," according to which explanation is a tracing of a course of events.)

The objection might be raised that as a commonsense explanation, it is not scientific, and therefore not what Hempel and Oppenheim are after. We could not take this to mean that the explanation is unscientific in the sense of

unsound, as if it were superstition or guesswork. It meets no objection or skepticism from science. What could the above objection, then, mean? When philosophers of science seek a specially scientific sense of "explanation," what are they after?

Someone who makes the above objection wants this sense to sharply contrast with the ordinary sense of explanation by causal mechanism. He or she therefore is thinking that an acausal theory of science is needed. If it is not needed, causal explanation would bridge scientific understanding and ordinary understanding, and not allow this sharp distinction. The arguments that a need for an acausal theory of science exists are treated elsewhere in these first two chapters. The present project is an innocent examination of the constructed theories of explanation, which historically have taken place once the acausalist assumption is well ingrained in tradition.

The upshot of the discussion so far is that a classic text has (a) provided a correct, if clumsily expressed, explanation of a phenomenon, and (b) adopted a strategy for understanding that explanation which is quite unworkable in the specifics it is given.

The same Carl G. Hempel presented a revised version of the deductive model seventeen years later, in 1965.¹¹ In this text he provides an example of an explanation and a genuine scientific law which has a closer relevance to the

explanation than does the above example. The new example is
¹²
 described as follows (emphasis added):

In a beaker filled to the brim with water at room temperature, there floats a chunk of ice which partly extends above the surface. As the ice gradually melts, one might expect the water in the beaker to overflow. Actually the water level remains unchanged. How is this to be explained? The key to an answer is provided by Archimedes' principle, according to which a solid body floating in a liquid displaces a volume of liquid which has the same weight as the body itself. Hence the chunk of ice has the same weight as the volume of water its submerged portion displaces. Since melting does not affect the weights involved, the water into which the ice turns has the same weight as the ice itself, and hence, it also has the same volume as the displaced water; hence the melting ice yields a volume of water that suffices exactly to fill the space initially occupied by the submerged portion of the ice. Therefore, the water level remains unchanged.

Let us give this example the setting of a high school science classroom. The teacher places a beaker full of water on top of a valuable textbook which, she tells the class, she really does not want water-stained. There is a chunk of ice floating in the water, let us say one of those irregular chunks that are found in ice bagged for sale. She carefully adds more water with an eyedropper until there is a rounded bulge at the edge of the water from surface cohesion. It appears that another drop of water, no matter how delicately added, would cause it to spill over and damage the textbook. The chunk of ice projects an irregular point above the water like a tiny iceberg. With students on the edge of their seats, the teacher leaves the beaker alone to deliver a

lecture.

At the end of the lecture, the attention of the class is turned again to the forgotten beaker of water. The ice has melted, the edge of the water is still bulging, and the textbook remains unharmed. In keeping with Hempel's example, suppose that the teacher's intent was to use this demonstration as a way of introducing Archimedes's principle of displacement, which she then goes on to talk about.

Why were the students on the edge of their seats at the beginning of the lecture? Why "might one expect" the water to spill over as the ice melts, so that when it does not, there is something to be explained? If we expect it to spill over, we have to be thinking that when ice melts it turns into an equal volume of water, so that whatever ice is protruding above will be added, upon melting, to the volume of water in the beaker. We are not cognizant of the fact that water expands slightly when it freezes, making the ice slightly less dense than the water. Hempel does not talk about this when explaining the "phenomenon" or when giving his predictable characterization of his explanation in terms of deduction from laws. But it seems that, far from introducing laws either explicitly or implicitly, the explanation corrects an assumption. We might well have assumed that water contracts when it freezes; it might not occur to us that it could expand.

This case of an explanation recalls Hilary Putnam's

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remarks:

Explanation is interest-sensitive and context-relative. We expect an explanation of a fact to cite the factors that are important (where our notion of importance depends on the reason for asking the Why-question).

In this example the reason for the Why-question is an expectation based on an error.

Before she talks about Archimedes's law, the teacher asks a bright student, "Why did you expect it to spill over?"

The student replies, "I assumed that the part of the ice which was sticking out above the water would be added to the volume of water in the beaker."

"And what would cause you to assume that?"

"Not realizing that water expands when it freezes."

Even with this much understanding of the experiment, to the point that he understands why he was wrong in his expectation, the student may have yet to learn about Archimedes's law, and he might well not be thinking about the relationship between displacement and weight.

Archimedes's principle says that a floating object displaces an amount of liquid with the same weight as itself. When I said earlier that this has a close relevance to the explanation, I meant that one could go about explaining why the beaker does not overflow by starting with a reflection on this principle. The teacher in the science class might in her follow-up lecture have said what Hempel says, that since

the floating ice turns into water, the principle implies that it will turn into the same weight and hence the same volume of water that, as floating ice, it displaces, necessarily leaving the level of water unchanged. In taking this tack of explanation she would at the same time be saying something about science. Hempel took this tack to try to establish a uniquely scientific sense of "explanation." This made a better example of the importance of a law to an explanation than the thermometer example, where the "laws" about the exact thermal expansion and conductivity of certain materials seemed distinctly peripheral if not irrelevant.

It is problematic, however, to say that in the tip-of-the-ice-chunk example Archimedes's principle is the "key" to an explanation, when the reason an explanation had to be given (in our example) was just a natural but mistaken assumption about the case which needed to be corrected. It seems that this remark of Hempel's came only from the pressure at work to see scientific knowledge as comprised of laws and their logical entailments.

Another example which Hempel uses in this same work¹⁴ is taken from John Dewey's book, How We Think.¹⁵ In his book Dewey relates an explanation he produced for himself after making an observation one day while washing dishes. He noticed that when he placed a tumbler, fresh from hot soapy water, upside down on a plate, soap bubbles would emerge from under the rim and expand for a short time, then come to a

stop and recede.

Dewey came up with the following explanation of this occurrence: When a hot glass is first set down, it traps air which is cooler than the glass. The air is warmed by the glass, so that it expands and produces the bubbles. As the air recools, the bubbles retract.

Naturally, Hempel wants to construe this explanation as consisting of two parts: first, a detailed description of the case, including temperatures, materials, and so on; and second, laws of various kinds--gas laws (relating pressure, volume, and temperature), laws about the behavior of soap film, and so on. Now, he does suggest in this text that he is offering a special, scientific kind of explanation. But he is also talking about Dewey's explanation. How does he manage to read Dewey's explanation as something so different from what it appears to be?

The answer is that he thinks all these laws which are not stated in the explanation really belong to it as its suppressed detail. When like a blossoming thistle this detail is brought forth, he thinks, the case fits a deductive model. He writes (emphasis added),

While some of these [laws] are only hinted at by such phrasings as "the warming of the trapped air led to an increase in its pressure," and others are not referred to even in this oblique fashion, they are clearly presupposed in the claim that certain stages of the process yielded others as their results. If we imagine the various explicit or tacit explanatory assumptions to be fully stated, then the explanation may be conceived as a deduc-

tive argument...

Later in this discussion he says more specifically what it is he refers to as a "claim that certain stages of the process yielded others as their results" (emphasis added):

...reliance on general laws is essential to a D-N [covering law] explanation; it is in virtue of such laws that the particular facts cited in the explanans [the description of initial conditions] possess explanatory relevance to the explanandum phenomenon [the thing-to-be-explained]. Thus in the case of Dewey's soap bubbles, the gradual warming of the cool air trapped under the hot tumblers would constitute a mere accidental antecedent rather than an explanatory factor for the growth of the bubbles, if it were not for the gas laws, which connect the two events.

This is quite a startling thing to say. Consider Dewey's reasoning, which was as follows:

Why should air leave the tumbler? There was no substance entering to force it out. It must have expanded. It expands by increase of heat, or by increase of pressure, or both.

He tried to think why it happens. And he came to see why, to realize what makes bubbles arise and recede. And that of which he became cognizant was precisely "the gradual warming of the cool air trapped under the hot tumblers." For Hempel, this process that Dewey found explanatory, these "conditions" of a phenomenon--the very thing he identified as a cause, and with which he felt satisfied that he understood what a moment ago he did not understand--would fail to explain, would be explanatorily impotent and empty, were it not for the fact

that the gas laws are "hinted at," "presupposed," and/or "assumed."

Hempel thinks that to realize that the air inside the tumblers is gradually warmed would be not relevant to understanding the occurrence "if it were not for the gas laws." Now, scientists have been able to formulate the gas laws because the pressure, volume, and temperature of a gas are functionally connected. These variables depend mechanically on each other according, it is found, to more-or-less precise quantitative rules--the gas laws. These laws might prove indispensable for designing equipment such as steam boilers and pneumatic chambers. They certainly would be essential in any precisely designed experiment which worked like the tumbler-soap bubble phenomenon. But why should we suppose that they have to play a role in John Dewey's thoughts while he is washing the dishes, that they are thus essential in this sense also? The gas laws might well not be in his thoughts at all. He certainly is not, simply by explaining the phenomenon to himself or recognizing why it occurs, necessarily also calculating and confirming his result with known laws.

Hempel's claim that the explanation hinges on the gas laws is cogent to this extent: it is necessary to the explanation that a functional interdependence among the variables of pressure, volume and temperature exist. However, the tidiness and quantitative exactitude of the law, that which makes it an indispensable tool for precision design, seems to

have no central bearing on the explanation. It is much more clear to say that the phenomenon, the explanation, and the gas laws themselves, would not be what they are but for the fact that these variables are mechanically connected. The "qualitative" description of this mechanical interconnection, which makes clear in what manner they are connected, telling which variables increase as the others decrease, is distinctly more pertinent to an understanding of the phenomenon than the exact quantitative formula.

Someone might follow Hempel's lead and object to my pitting against the deductive model simple narrative explanations such as the one given for the "dip" phenomenon of the thermometer, or Dewey's narrative explanation of the expanding and receding bubbles, by saying that these narratives merely fail to mention the various scientific laws which are "implied" in them. What could be meant by this? There are more detailed processes involved which could be unfolded in the story, such as the subatomic processes responsible for the properties of various substances. The "hidden laws" would then presumably be the mathematical inventions by which atomic physics models its observations. Certainly no one or all of these equations are referred to by implication in any ordinary causal account of events.

Carnap argues that all explanations are based on laws,¹⁶ whether explicitly or not. In one of his examples, someone seeks an explanation for why his watch is missing from the

table, where he left it. The explanation is that "Jones took it." Carnap thinks even this is really a "law explanation in disguise." He says this:

..."Jones took it" would not be considered a satisfactory explanation if we did not assume the universal law: whenever someone takes a watch from a table, the watch is no longer on the table.

Carnap's claim is that a "universal" regularity involving an action and its immediate consequence is presupposed in the explanation. Perhaps this is true. But there are two points to be made here. First, pointing out this presupposed "law" does not improve on the explanation as it was. Second, to cite this "universal law"--action X always has consequence Y--by itself would not satisfy the watch owner's query at all, and therefore cannot be the undisguised form of the given explanation.

This leads to the question, What is to be gained by the insistent quest for laws at the heart of explanations? The law uncovered neither substitutes for, nor furthers, the given explanation. If the motivation, as I believe, is to be able to understand how it is that quantitative-empirical physics is not only an explanatory science, but is paradigmatic explanation, then the claim that it in fact is such is unargued, and, I think, without basis. Another possible motivation is the general aim of disclosing a formal logic within explanations, but this is peripheral to the project of an acausal theory of science, and is outside my concern.

It may be the tidiness and precision of laws that leads a thinker to value them as against "crude" or "merely qualitative" causal accounts, especially if there is pressure to separate scientific from ordinary explanations. But let alone laws hidden in causal stories, is there any reason to think that more detailed and precise laws are referred to by implication in some fairly approximate laws, such as the laws--presumably, on the "covering law" view, explanations--of classical, "macro" mechanics? Certainly pre-twentieth developers and utilizers of the laws of mechanics were not, in this activity, referring by implication to the laws covering more detailed regularities underlying mechanical processes, such as quantum mechanics, which were yet to be discovered.

It seems highly forced to claim that an account of a causal process is somehow a veiled reference to a bundle of equations. Why not instead, if one wants to insist upon further analysis, say that the causal process described is composed of more detailed causal activity? The difference in intelligibility is remarkable.

This notion that an explanation which does not mention laws might really be about hidden laws, helps show the way into a troubling thought about the deductive model, one which has already been remarked upon:

The logico-empiricist writers, once they present an example of an explanation, seem to then need to find the laws

that are pertinent in order to explain or clarify the explanation. The "laws" that they look for are "generalizations" under which what is to be explained can be, as it is often said, "subsumed." To subsume under a generalization only means that there is to be found in our knowledge a conditional proposition which describes circumstances under which a certain thing always occurs.

In the examples from the standard late Positivist literature which we have looked at, however, the thing mentioned or "covered" in the generalization which is turned up, is (apart from Carnap's example) not the thing-to-be-explained itself, but some related quantitative regularity on which the philosopher can attempt to pin the essence of the explanation, something taken from scientific knowledge of measurable properties of material and its interactions.

But apart from the pressure of a search for a meaning of "explanation" to supercede causality, it seems unnecessary to look for these laws, especially since, as it appears, this search does not manage to pin down the essence of any one explanation. The troubling thought about this quest is this: Suppose that to find and cite an appropriate conditional proposition from out of a broader knowledge of such propositions really does accomplish explanation. Why not, then, cite the exactly appropriate one: "If a mercury thermometer is stuck in hot water, the mercury level will dip briefly," or, "If ice which is floating in water melts, the water level

will not change as a result." With these conditionals, one is sure of hitting the mark.

The trouble is, of course, that these are not laws which are put to use in any science, hence will not do for an account of "scientific explanation." Another problem, equally obvious, is that these statements just describe the very thing-to-be-explained, they do not explain it. The impulse is therefore to seek more general facts of Nature--hopefully, quantitative laws in use by science--from which these specific observed phenomena can be derived. If this can be done, (the thinking goes) it will maintain the sense that what explains is something other than the thing explained, and also the sense that one is explaining by scientific laws.

So one looks for the general laws which are pertinent, which might seem essential to the observed phenomenon. But: Why should one think that these general laws, conditional propositions "subsuming" more specific laws, are any more of an explanation of those specific, quantitatively precise regularities in Nature which they designate, than was the conditional proposition which was merely a description of the phenomenon? Does not a law, rendered in prose form, only amount to a conditional proposition, and not to an explanation?

Moreover, laws can readily be seen as designating facts to be explained. I will take Archimedes's law as an example.

In Hempel's text, there is surprise at the discovery

that the water level in a beaker does not rise as the ice in it melts, and this leads to a recollection of Archimedes's principle. Suppose instead that one day an amateur scientist who is ignorant of Archimedes's principle is tinkering with floating objects and with weight and volume measurements. Through "blind intuition" and more-or-less random tinkering, he hits upon Archimedes law: a floating object displaces an amount of water which has the same weight as itself. He might shout, "Eureka!" But is this because he explained something? He was not trying to explain anything, only tinkering in his shop. He would have discovered something "nifty," in a certain sense a thing of beauty, a way that mathematics connects with the world so that the symmetry and clarity of the mathematics can be attributed to nature.

He might well not feel he had discovered anything that needed explaining, and this may be in part because he is pleased with it. However, one might have in this way discovered this mathematical relationship, or just heard about it and understood what one heard, and still not have an explanation of it--that is, one could still entertain the question why it is the case. What would seeing why it is the case involve?

One would think about floating objects, and the fact that some things float and some do not, and that certain things float in some liquids and not in others. A liquid will support an object if it has at least as much density as

the object, just as objects lighter than air will rise. In supporting the object the liquid resists the force of gravity. The density of the object and the density of the liquid are in mechanical struggle. When the object wins, it sinks; when there is equilibrium, the object is suspended within the liquid; and when the liquid wins, it floats. Among floaters there is variation in buoyancy--some things float high in the liquid and some float low, mostly underwater. Low floaters obviously displace more of the liquid.

Thinking about this struggle of forces, with density as the deciding factor, could lead not only to understanding why Archimedes's law holds, but possibly even to the first intuition, formulation, and testing of it. If an object floats, there is an exact equality between two forces: the weight of the object, and the upward pressure of the liquid upon it. What might be the relation between this upward force and the amount of liquid giving way to the object? Could the force be equal to this amount by weight?

The equality of weights is a fact about certain conditions, something one can either reason toward, or discover and subsequently come to understand why it is so. Thus the law itself is not something that explains, unless an artificial sense of "to explain" can be derived from its intrinsic interest as a precise piece of knowledge, its tidiness, or even its utility. Much less problematically, the law signifies a fact with a causal-mechanical explanation.

The Primitivity of the Non-Initiate

The drive toward a model of explanation which is both specially scientific and acausal, leads this tradition of thought to another kind of extreme. Acausalism in philosophy of science (which calls itself logical empiricism or logico-empiricism) will readily assert that a commonsense or pre-scientific understanding is no understanding at all. Ernest Nagel, in a comprehensive work having much to do with the deductive model of explanation,¹⁷ says,

A marked feature of much information acquired in the course of ordinary experience is that, although this information may be accurate enough within certain limits, it is seldom accompanied by any explanation as to why the facts are as alleged. Thus societies which have discovered the use of the wheel usually know nothing of frictional forces, nor of any reasons why goods loaded on vehicles with wheels are easier to move than goods dragged on the ground.

Prescientific people who use wheels, Nagel seems to be saying, are lacking an explanation why wheels make things easier to move. What is it about their experience or "information" that they do not understand? Do they attribute the success of the wheel, prescientifically, to spiritual agencies? It is hard to imagine anyone at any stage of culture saying, "I can see that you are having an easier time pulling your cart with wheels than I am dragging my load on the ground. Perhaps some day I will learn why this is." From earlier examples we can recognize the idiosyncratic claim

that directly understanding such a simple mechanism is not "scientific explanation" and hence is not really understanding at all, is devoid of the unique illumination that science provides. True scientific knowledge is enshrined away from any straightforward description of events.

It also seems wrong to say that the prescientific wheel-using people have no knowledge of frictional forces. Surely the designers and builders of carts and chariots would have had a fairly good knowledge of frictional forces. They would be able to explain to an apprentice why a wheel and axle should be constructed in such and such a way and out of such and such materials, to make it work smoothly and efficiently and wear longer. If one wanted to say that if they have a notion of "force" it is necessarily primitive and obscure, it could be retorted that this is notoriously true of the scientific use of the term as well.

What we can grant Nagel is that these people might not have at their disposal the principles regarding frictional force which modern science makes use of. For example, they might not know that the amount of friction is a function of the pressure between the surfaces, but is independent of the area of surface contact. And they would most likely have no formulae for these relationships. This means they have limits to their sophistication, both in methods of inquiry and in technology. But it in no way suggests that they do not understand why a wheel makes their lives easier.

Arthur Pap gives an example in which someone finds that the milk in the refrigerator has gone sour. The person is at first puzzled, then remembers that she defrosted the refrigerator that morning and forgot to turn it on again. Thus she has an explanation for why the milk has gone sour. Pap then says, "This explanation is, to be sure, superficial, since it is based on observation of an event sequence that the nonscientist just accepts as an empirical regularity but¹⁸ does not really understand."

But her explanation was just that she left the refrigerator turned off. Scientific knowledge or the lack thereof had no direct bearing on the effectiveness of the explanation. Nor should we slight it for being impoverished compared with the depth of science.

This example of an explanation could be connected with "scientific explanation" in the following way: the experience could occasion the person to ask, "Why does milk go sour when it is not kept cold?" She could then research the matter in biochemistry textbooks, or ask a specialist (or someone who happens to know). She might "not understand" the episode in the sense that she seeks an explanation of this sort (though "not understand" does not quite fit the case; it is just a curiosity about some specifics).

But what Pap says, that the person only recognizes an empirical regularity which she does not really understand, seems to ascribe to her a peculiar blankness of mind about

it. (Actually, it may well be that the person has accumulated few if any observational instances of this regularity.) It seems clear, however, that the person knows and understands something quite definite here, namely a causal relationship. Upon hearing the chemical explanation, it would satisfy her curiosity, but would most likely not elicit any surprise about the kind of thing this explanation involves. The knowledge that she gains might only be of specific substances and specific chemical reactions. She knew it was something like what she learns in specifics.

Pap explains that by "really understand" he means not only that one knows about a well-confirmed regularity, "milk turns sour if you leave the refrigerator off," but that one knows why this regularity exists. This means one has an explanation for the facts cited in this explanation, and so forth, until ultimately one has in one's mind the whole system of science. So here is the rub for the "nonscientist": she has not been initiated into the special world of understanding of the scientist.

Unfortunately for this claim, we have seen that she not only understands the fitting scientific explanation, but also understands, apart from this, why her milk went sour.

Summary of the Critique

From the examples we have looked at, the following summary of logico-empiricism on explanation can be given.

First, by deciding to pursue an acausality about explanation, involving a description/explanation contrast, it sees explanation as both specially scientific and as the first dawning of any understanding, as the first illumination of mere disconnected (merely described) facts. This leads to a general devaluation of causal understandings. Second, it grasps unsuccessfully at the possibility that explanation can be so construed if it is tied to some cognitive equipment of science in the form of quantitative formulae, which are only about observations, and which fall under the heading of "laws." In the next chapter I will examine the historical origins of this kind of philosophical commitment.

My concern has been to test this view of explanation against examples of explanation which were taken from the logico-empiricist philosophers themselves. In the course of the discussion I have shown how a law-based theory of explanation fares philosophically from a standpoint that has not acquired, or has set aside, any predisposition to think that an understanding of modern scientific knowledge in terms of causality is outdated and mistaken. The result: from this standpoint, acausal explanation theory appears implausible and fairly pointless. To reach this conclusion, this chapter began with an exposition of the fact that laws can be formulated independently of causal explanations, a fact that gives acausal theory of explanation, focussing on laws, a possible constructive course. Laws are statements of facts of obser-

vation refined into quantitative terms. From the standpoint that explanation of natural phenomena through science is causal explanation (and it is important to maintain a critical perspective on any reasons why it should not be), these facts of observation, in either their crude or their refined form, are "explananda," things which might well call for explanation.

Arguments for logico-empiricism as a general theory of the whole of science have two major drawbacks.

The first of these I have already mentioned. To attempt a formal model of explanation which does not involve description or adducement of causes in a way reflects the character of physics, in which branch of science, peculiarly, there come to be exclusively mathematical treatments of certain observations. But other branches of science are certainly not acausal. Consider geology. Plate tectonics is a very contemporary scientific achievement. It explains by telling, for example, about pieces of the Earth's crust grinding past each other, building up local stresses that are suddenly released in violent jolts. Geology is a mechanistic (and entirely scientific) science.

The literature of philosophy of science does not take examples exclusively from physics, but tries to cover all branches of science, in keeping with its name. Nevertheless, what is called the "standard" view holds that to explain some natural occurrence is not to talk about its causes, but

rather to cite a proposition stating that the occurrence has always been observed under similar conditions, or some related lawlike propositions. To the innocent ear, this seems patently absurd as a characterization of scientific knowledge. Science explains what stars and galaxies are, while ordinary experience and speculation, it might be said, could not. Here is some specially scientific explanation. What could any conditional propositions have to do with it? Certainly none would come into play in an actual explanation of what is seen in the night sky to someone who for some reason was ignorant about it.

There is one explanation for this odd generalization of the image of specialized physics over science per se. It is an idea which is sometimes called "theory reduction," and it is related to the Positivist picture of science as a single, complete system or structure. It is thought that since, for example, the deeper explanation of biology is chemistry, and the deeper explanation of chemistry is physics, all scientific knowledge reduces to physics.¹⁹ If physics is acausal, then in the last analysis, so is science in general.

But for this conclusion to follow, it must at least be shown that physics is permanently an acausal science, that is, that there can be no causal interpretation of its ultimate observations. My arguments aim to show in the long run that this is not demonstrated, but rather, that it is perfectly possible and cogent to simply say that physics is

anomalous among the sciences in not providing naturalistic explanations for its domain, which is not even to say that such explanations are impossible.

A second major problem with logico-empiricism as a general theory of science has also come to light in the discussion. In point of fact it seems to have been clear to some extent even to the proponents of the covering law model that an individual law covering a phenomenon does not by itself explain it. What answers this problem for the logico-empiricist is that the explanation by covering law refers and defers to more general laws which cover these laws by organizing them together with other "phenomenological" laws, and so on through higher orders of inclusion until the whole superstructure of science comes into view. Nancy Cartwright²⁰ calls this the "generic-specific" account, and it is well²¹ developed in Ernest Nagel's book, The Structure of Science. It is supposed to put the weight of the whole of science behind each individual explanation.

But the more general laws work exactly the same way as the phenomenological laws, with the latter taking the place of the specifics of observation, such as particular measured quantities, which are correlated in each phenomenological law. So again these more general laws can only signify highly integrated facts of observation. Therefore there is nothing in this kind of account which can finally come to an explanation where the specific law could not, unless perhaps

we can in following out each case get a glimpse of the whole structure. And what could even this be on the acausal model except an ultimately inclusive law?

The major conclusion for the purpose of the present work is that the demand for productionistic explanation throughout science is undiminished by the logico-empiricist objection to this demand. A law-based model of explanation does not have the kind of independent success in supplying a meaning to "explanation" which can be a challenge to the preeminence of causality for the explanation of phenomena, let alone supplanting the causal format thought to be a thing of the past. Moreover, the regularities signified by laws easily seem themselves to call for explanation in terms of the action of causes. Therefore, even if knowledge in the domain of physics were permanently confined to mastery of regularities, it is doubtful that a Humean view, either of science in general or of specific scientific explanations, is supportable.

Notes

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CHAPTER 2

PHILOSOPHICAL SOURCES OF ACAUSALISM

The last chapter examined a late Positivist and very influential constructive strategy for supplying a meaning to "scientific explanation" when causal stories have been consigned to a pre-scientific and dependent status as explanation. An important preparation for this traditional project is the adoption of the Humean analysis of causality. This chapter will seek out some of the direct arguments in earlier Positivism for the appropriation of this analysis.

"The Humean analysis of causality" designates what I regard as a kind of disease of thinking, one which, as I mentioned in the Introduction, would characterize events in a series as having contiguous relations. This analysis pre-establishes for itself the event and the process of which it is a part as blank extension in space and time. In this way an event or series of events is reduced (usually beneath awareness) to the occupation of a region of four-dimensional space, with time rendered as a dimension of space. This mode of analysis and its destructive consequences is investigated in this chapter and also in Chapter 4 as part of the discussion of Wesley Salmon.

The result of the Humean analysis is actually to col-

lapse the very character that any event has, if it is to be extended spatially or temporally or in any combined sense. Such an event has not only continuity, but shape, the differentiation of its beginning, middle, and ending stages. There is only continuous occurrence where there is continuous development. This means that there is continuous differentiating transition both internally to an event and throughout its production of a successor event. Thus, even before the genetic succession model of causality provides any novel successes at understanding, it already proves to follow from the decision to preserve both causal connection (against the contiguity picture) and what is in any case meant by an "event" for purposes of causal explanation.

This chapter examines arguments leading to the position that causality can only mean the regular succession of discrete events, at most contiguous. The critique of these arguments has the short-term aim of defending, as a general possibility, serial relations which involve neither contiguity nor separation, and this serves the broader aim of supporting the reality of causal connections.

The Positivist Doctrine About Causes

I will not attempt to provide a comprehensive history of the Empiricist/skeptical/acausal tradition. The primary aim is to test those arguments which occur in the contemporary period of science, which sometimes make use of recent devel-

opments in physics, and which might be operating to support the predominance of an acausal (neo-Humean) view of science. But some tracing of the more recent ideas studied in the previous chapter into their Positivist origin is in order.

Around the time that the famous paper of Hempel and Oppenheim appeared, philosophers of science were setting the stage for making laws the centerpiece of explanation, by reinforcing the idea of a sharp distinction between description and explanation which came from earlier Positivism. That distinction grew out of a complex philosophical aim, that of "reconstructing" scientific knowledge. This meant displaying and clarifying science as a complete or completable system of empirical propositions. Herbert Feigl argued in 1948, the year of the Hempel-Oppenheim paper, that an explicit description/explanation distinction was needed as a way of furthering the Positivist program.¹ Description would be about observed phenomena, while explanation of these phenomena would not be description of natural events bringing them about, but a situating of the "observation sentences" about them within a superstructure of such observation sentences, this superstructure being, once fully realized, the "unified" system of science.

The background rationale for this was that philosophers of this tradition wanted to display science as knowledge which is in their sense indubitable, that is, free of any reality claims for what is outside observation. This meant

that science, properly purified, had to be an organized system of propositions, all of which were such that they were subject to verification at any time by simply making an observation. The propositions to be retained as genuine science had to describe these observations and nothing more. A philosophical standard of "certainty" for knowledge, in keeping with a long tradition, was to be tied to observed events, and should not allow inference to causal entities which they regarded as both conjectured and in the nature of the case hidden from the confirmatory glance.

An additional impetus for this project was the influence of Copenhagenism in atomic physics, new and dramatic at the time of Positivism, which said, in essence, that physical theory in this area could only be about the observable, a kind of Empiricism adopted as a research philosophy in physics itself.

What the Positivists were faced with in their program was the fact that modern physics is, despite Copenhagenism, substantially concerned with "unobservable entities." To reject claims about the actuality of whatever is named by "electron," "photon," "magnetic field," etc., on the grounds that such claims are not verifiable by seeing the thing named, meant casting out of genuine knowledge the whole of what is on the face of it a fundamental category of natural occurrences encountered in the course of science, virtually the whole natural domain of contemporary physics.

Nevertheless the Positivist program amounted to just such an intellectual housecleaning, or, as they called it, a "clarification of scientific concepts." They did after all have some important thinkers of the past supporting them. Hume had concluded that the very idea of "causation" could only be a purely mental construct which we form on the basis of the "constant conjunction" of pairs of individual inputs through the senses, the "cause" and the "effect," when these same pairs of data occur in the same successive order on repeated occasions. Causal connection, on this account, only means the "determination of the mind" such that, when a cause-event is perceived or thought, the mind will "pass from the idea of [this] object to that of its usual attendant,"² the effect-event. On this view there can be nothing in our knowledge of the world (independent of mind) corresponding to the idea of the production or bringing about of one event by another event.

The Positivist appropriation of Hume's view for a theory of science combined this view with an existing tradition of scientific antirealism. Experimental observations had a role analogous to the Humean "sense impressions." For Hume, causal connections--which for him would have to occur between separate inputs of perception which always occur in immediate succession--cannot be a part of our knowledge of nature. In Positivist antirealism, any unobservable entities supposed to transmit a connection from prepared and precise experimental

conditions to their precisely measured outcomes--namely, some variety or other of causal transmissions which now are most predominantly given the term "particles"--do not have a place in the final form of unified science.

In this way the critique of causal connection could be alloyed with antirealism about unobservables. This chapter is concerned with the Humean component of this acausalist alloy. But a brief survey of the sources of antirealism will help to establish a critical perspective on Positivism and prepare for the next chapter.

In adopting scientific antirealism Positivists claimed descendancy from the physicist/philosopher Ernst Mach, whose rejection of Newtonian absolute space and time (another kind of "unobservables") seemed to be born out in Einstein's theories. Mach had argued that explanations in terms of fields and atoms implicated physics in beliefs about an occult world of events, and that no one such explanation of a set of phenomena had more claim to truth than others had for the same set of phenomena.³

A comparison of the cases of two antirealist physicists, Mach and Wilhelm Ostwald, is instructive. Ostwald held out staunchly against the theory that material substances consist of ultimate, discrete parts: the "atomic-molecular hypothesis." Eventually Jean Perrin conducted a long series of experiments, all using independent procedures, which were specifically designed, it might be said, to detect atoms.

The results of this experimentation, amounting to a considerable quantity of independent bits of evidence,⁴ won out over this skeptical view, so that Ostwald finally admitted that there are atoms and molecules.

Especially if it serves as a goad for this kind of brilliant experimentation, this skeptical reservation about causal explanations can be seen as a healthy influence, even though in this case it meant someone had to admit to a life-long error. But if in this case the skepticism was defeated not by learning how to see atoms, but through the detection of their effects, it would seem that a certain prejudice about the perceptual basis of knowledge had also been defeated. It might also be said that the physicist in Ostwald finally won out over the Empiricist philosopher. Mach, however, remained stubbornly antirealist about atoms. And it was Mach who, because of these very views, was made a hero of Positivist philosophy of science.

As I indicated in the previous chapter, the grounds for general antirealism about the unobservable can have little to do with the reasons why hypotheses in science are considered as yet unconfirmed, where this is a matter of the process of scientific discovery in textbook accounts.

Schlick's Humean Arguments

A connection can be made from the later formal model of explanation based on the lawlike proposition, to some origin-

al acausalist arguments of Positivism, through a text of Herbert Feigl's. As I mentioned, Feigl helped to prepare for law-based explanation theory by promoting a description/explanation contrast. He can also be credited with a remark summarizing the Positivist treatment of causality. It occurs⁵ at the beginning of his essay "Notes on Causality":

"The clarified (purified) concept of causation is defined in terms of predictability according to a law (or, more adequately, according to a set of laws)."

Feigl's subsequent discussion of the matter tends to be ragged, and there is scarcely an argument for this position. The attitude in the writing is one of, "By now (in the Positivist tradition) we have it right about causality. Let's go on." But he does provide references to writings from the works of the earlier Positivists, which he takes to represent the reasoning by which this view was settled upon.

One of the works which Feigl claims for his antecedence in his pronouncement about causality is Moritz Schlick's⁶ "Causality in Everyday Life and in Recent Science." It is worth treating at length. Schlick raises the problem of the "propter hoc/post hoc" distinction, that is, how we tell whether an event is the cause of another or just happens to precede it. He claims that this distinction marks our causal judgements, for example, when we consider which among several preceding factors is the cause of our state of depression, or when we try to determine whether a certain medicine is effec-

tive against a disease.

The first part of his article, the "everyday life" part, is concerned with how in fact one would ordinarily go about establishing that a causal connection exists. In his language, he is considering how a causal judgement is "verified." This term should be understood in light of what was outlined above, that it was a thesis of the Positivism of his time--for which he was importantly responsible--that meaningfulness depends on verifiability, so that in seeking the meaning of a word such as "causes" we have to look for how propositions containing it have their truth ascertained by an observation.

Schlick is seeking to explicate how talk of causes touches down, so to speak, to our observations. He presents some examples meant to illustrate the process of looking for and determining causes and establishing causal connections through observation; but in addition, in accordance with his philosophical bent, these examples are intended to show how from these observations we gain a meaning for the very concept of "cause." What he calls the "causal judgement" is not only a determination about a particular cause of something; he thinks that the causal mode of understanding itself, its first possibility, springs from these and similar sets of observations.

Schlick tries a few different possibilities for sources of our concept of "cause." The first condition that he comes

up with for a source of the concept is one which is closely related to Feigl's "predictability according to a law," and one associated with Hume: that of observed regularity of sequence. Schlick says about this, "If C is regularly fol-
⁷lowed by E, then C is the cause of E." To take his example, if in a sufficient number of observations the use of a particular medicine is followed by the recovery from a particular disease, one will, he says, speak of cause and effect. He will end up claiming, in an echo of Hume, that such regularity in observation is the sole source of the notion of a "cause."

About this sketch of an example, the treatment of disease by a drug, he claims emphatically that,

since...the observation of the regularity was, in this case, the only thing that was done, it was necessarily the only reason for speaking of cause and effect, it was the sufficient reason. The word cause, as used in everyday life, implies nothing but regularity of sequence, because nothing else is used to verify the proposition in which it occurs.

But there seems to be sparse basis in this example for these impassioned claims.

The truth in what Schlick says is that given only this much of a case, we might well have sufficient reason (in the sense of quite good reason) for thinking a causal connection exists between administration of the drug and remission of the disease. And the regularity could indeed be the only reason for so thinking, apart from a basic background of

medical knowledge. Suppose, for example, that a spontaneous recovery often occurred, but it had gone unnoticed for a time that the recovery was always preceded by the administration of a certain substance (for some other reason). "Come to think of it," the doctor might say, "these patients were always given this substance before recovery." It could be said about this that the idea of a causal connection was brought upon the doctor by a regular succession (and a subsequent realization).

But actually it may well be that a medical researcher looking for a cure has not built up his causal judgement by accumulating observations of a specific regularity at all. Normally, in fact, it would go like this: there would be some substantial reason to think that a certain drug will produce a beneficial result, and trials would confirm that it does. Because of this background support that the causal connection already has, one such success could support a specific causal judgement well enough to commit time and money to confirmatory tests.

To say that the word "cause" as it would be used in connection with his example, let alone in general use, "implies nothing but regularity of sequence," is surely not warranted on the basis of an example of this kind. This is not to say that it "implies" anything else that is not already clear in its unanalysed meaning. But if what Schlick is after is what is meant by saying there is a causal connec-

tion, one might well want to say that more is meant than regularity of succession, especially since one might even quite reasonably think after only one observation that a certain drug has a curative effect. There is certainly room for more to be meant by "causes," at this point in the argument.

But we soon learn from Schlick's discussion why he thinks it is impossible that causation could mean anything more than regularity of succession. It seems to him that if we say it is more than mere chance that E follows upon C, we must be saying that E will always follow upon C. The alternative to chance succession must be necessary succession, so that an instance of E not following upon C would force one to revoke the causal connection. But, he reasons, this "always" is unverifiable, because only a finite number of instances of E following C can ever be observed. Therefore, according to the verifiability requirement for meaning, there can be no meaning for "cause" beyond the fact that there is a regularity in past experience which can be extrapolated into one's fulfillable expectations, if one assumes that this additional or alternative meaning would be necessary connection, which means, the absolutely inevitable succession of C by E.

Schlick reminds the reader of the case in which E does not follow upon C due to an intervening cause. There one does not revoke the causal connection, though there is an exception to the regular succession. If in one out of a

hundred cases the cure fails, the physician will not decide the other ninety-nine cures were coincidences; rather, he will think that in this one case, there was a counteracting cause. This kind of case helps Schlick dispose of the idea of a "necessary connection."

But, thinks Schlick, the situation is the same with this intervening cause as it is with the original cause. One must be able to first discover, then verify, the intervening cause in order for a causal judgement about it to have meaning. It would seem that on the regularity view, one has to be able to accumulate some observations of the medicine failing to cure under repeated circumstances, in order to have acquired a "causal judgement." But then this example does not fulfill the basic need of the regularity view, that there be a specific, repeatedly observed succession, since in this observation the counteracting cause, the first member of this particular succession, is not observed. Its existence is, however, strongly surmised, which means the concept of cause is applied.

What is being called a counteracting or intervening cause here might be, for instance, that the disease symptoms in this case have a different cause, or that the disease-causing organism has a resistant strain, or that there is something about this patient's physiology that prevents the drug from working efficiently. Three things should be noted about this case. First, a judgement was made that there was

a counteracting cause after only one observation, that of the medicine not curing the patient in this highly exceptional case. Some amount of research might have been launched on the basis of this alone to find out just what the cause of failure was. Second, there was no observation of the counteracting cause, but only of its effect: no cure. Third, no number of occurrences of merely this same observation--failure of cure succeeding administration of the drug--would add to or support a general causal understanding, or help identify a specific cause. It was in fact the exceptionalness of the case, far from an accumulation of like cases, that made a causal intuition about it so strong.

But what Schlick specifies as the observations which on his account are what give rise to our knowing and speaking of an intervening cause, are not the observed successions: medicine administered--no cure. He refers, rather, to the observations which go on "in the laboratory," rather than in the clinic, in the investigation of the intervening cause. Let us look carefully at the consequences of this for the regularity view.

Some research into the counteracting cause is carried out. It is found to be a resistant strain of the agent of the disease. The microorganism taken from the body of the one hundredth patient is not destroyed by the chemical in petri dishes (let's say). This "regularly occurs," i.e., a few experiments are done to make sure. This allows the

researcher to securely surmise the specific intervening cause.

However, if causal connection arises purely from observed succession, the observed successions which take place in these experiments could not be the source of a causal judgement about the action of this cause preventing the action of the medicine in curing the disease in the patient. It would only give us cases of the microorganism surviving the drug, giving rise at most to a causal judgement about a process in a petri dish. The succession: medicine given--no cure, which is the one about which there is a judgement of a counteracting cause, takes place in a different setting, and has quite different antecedents and consequents. So far there is no observed regularity which could give rise to the causal judgement about the clinical observation that there is an obstructive action of one cause upon another.

In order to confirm the finding about the cause, disease organisms are taken from the body of the one hundredth patient and injected into rats, and after they become diseased, the drug is tried out on them. Cure does not result. The judgement of a particular cause is confirmed and supported. Does this arise from an observed succession, say, those observations which are made in the course of this complex series of actions? But this is not a causal process, only a deliberate order in experimentation. How on the regularity view would we make the connection between this experiment and

the clinical observations? What possible regular succession could supply this connection? So far this view is leaving out some important judgements of causal connection.

In fact, causal understanding permeated such clinical and laboratory activity from start to finish. The view that causal judgment comes exclusively from regularity seems out of touch with these examples. Surely the judgement of an intervening cause in this one hundredth case is not only such that "cause" already has meaning, but is also a judgment of considerable substance, prior to the launching of an investigation to establish it. Perhaps there is a more resistant form of the disease; here there is certainly something to research, and probably something to discover.

What actually would be the case in this example is that one would make a fairly well-founded judgement that a cause has intervened, without the need even arising for repeated observations. And another observation of the failure of the medicine to cure, or any number of such observations, would not help establish the first as something with a cause, or help establish what the intervening cause is, or engender the concept of cause.

It is wrong to think that in this research activity causal understandings originate even in part from individual observed successions which one happens somehow to accumulate. Rather, causality forms a background to the whole activity, and experiments which are deliberately designed on the basis

of this background are carried out to determine and confirm specific causes.

To summarize the case, Schlick admits that a cause is recognized where it is unobserved, and his theory cannot account for this recognition. As will be shown in succeeding chapters, the failure (in different ways) to account for interacting causes is a benchmark of acausalism in philosophy of science.

There is an important assumption, which I have already alluded to in Schlick's article, which is traditionally put to use in arguing for the regularist view about causes. This is the assumption that any other meaning of "cause" beyond consistent regularity would have to be the invariable occurrence of the specific effect, given the cause. On the Positivist view, to say that a certain effect will always occur barring intervention, is shattered against the demand for verifiability. "Will always" cannot be tested. Setting aside the question whether in general causes have to, or can, necessitate their effects, the traditional claim, also found in Hume, seems to be that this is the only alternative as a possible meaning for "cause," supposing that one were to try dropping the claim that regularity in observations is the only meaning.

There are some reasons apart from the verifiability criterion of the Positivists for rejecting the necessitation view of causality. Immediately there is the question of what

exactly it means. Is it logical, or somehow physical, necessitation? What could this "physical necessitation" mean other than "bringing about," or in other words, causing? If it were really necessitation, it might have to be some monstrous, unchallengable cause, the unstoppable force. And there cannot be any logical necessity connecting one event with a separate event. To imagine them occurring independently in some possible world refutes the idea of a logical implication from one event to a different event.

Allowing that necessitation is not workable as the alternative to observed regularity for an analysis of causality, the question remains: might there be some other alternative? Schlick is aware of this possibility. He considers and rejects the logical entailment of the effect in the cause, and then presses on to test further possibilities for a basis for causal judgements.⁸ I will retrace this whole path in his considerations.

Schlick gives an example that he thinks will show the source of the view that causation is more than regularity,⁹ whether logical entailment or something else:

If a surgeon amputates a man's leg, he will know beforehand that the man will be one-legged afterwards. Nobody thinks that we have to wait for a long series of experiments in order to know that amputation results in the loss of a limb. We feel we "understand" the whole process and therefore know its result without having experienced it.

Schlick spots where the idea that causation is logical

entailment might come from in this example. In fact, he claims, the logic has "nothing to do with causation," but only with what we mean by "amputation." The tautology in the statement "amputation causes the loss of a limb," he says, creates the illusion that here "cause" means logical entailment.

Then Schlick says something more, and something very curious, about this example. He generously tries to trace this causal understanding to something different than regularity, something called our "comprehension of the process" (emphasis added):

We usually believe we understand this connection, because we think we comprehend the process, say, of a saw cutting through a bone; the hard particles of the steel are in immediate contact with the soft particles of the bone, and the latter somehow must give way to the former. Here again we have contiguity in space and time, which appears to flatter our imagination, but apart from that we have again nothing but a sequence of events which we have often observed to happen in a similar way and which we therefore expect to happen again.

Here Schlick is again exploring possible alternatives to his view by considering the possibility that "we comprehend [causally] a process," apart from a claim about logical entailment. His description of the process here is a strange microanalysis that I cannot imagine anyone giving as an expression of their understanding of it, and which it seems doubtful that even a scientist would give, for all their concern with mini-micro dimensions. The point is to show the

process as a sequence of detailed events, with the aim of throwing us back on the regular succession analysis.

He claims to understand the psychology of the opposition here: because we have this fineness of detail, so that seemingly all the gaps in the causal process are filled in, we think we have a special comprehension of causality. But in fact, he claims, the events in the causal chain are at most merely contiguous and no more show connections than would any other regular succession. His argument is flawed by his strange treatment of the example; this sequence of events, particles making contact with particles, does not appear to constitute our understanding of the process.

Schlick did not live up very satisfactorily to his intention of considering, through this example, a possible understanding of a causal process which is neither a matter of logical entailment nor one of accumulated regularity in observations. Should we not, by way of expressing such an understanding, say something like: "A sharp saw wielded by a strong human arm will cut through flesh and bone"? But Schlick will want to say, as he does after the very passage quoted, that this assertion would merely be a summation of past experience, whether or not we had ever witnessed or undergone the operation: "For aught we know we might some day come across a bone that would resist any saw and that no human power would be able to cut in two."

This is the verification criterion at its best. He

speaks here of the old contingency that kept us from being able to verify the "will always." Here it serves not only, as a logical possibility, to ward off the logical entailment view of causality. It is also, apparently, supposed to speak against our "comprehension of the process" as a basis for knowledge of causality. But if I were to say I understand the mechanism involved here, this imaginary logical possibility does not speak against me at all, since I understand it mechanically as well: in this imaginary case the bone is too hard for the saw, so that the usual mechanism cannot proceed.

Schlick is trying to make the claim that any such understanding of a mechanism is really only our belief that we have arrived at a series of events which has its full detail unfolded, but that in such a series of events we nevertheless can never find anything but regularity of sequence. It was seen that the event sequence he spoke of was a strange contrivance. Can his case be made with a more recognizable event sequence? The cutting action of each tooth of the saw might be a more appropriate microanalysis. But this shows no more capability to decide between the regularist or the productionist view than does any other mechanistic description. So far it might be about discrete and separate events, or it might be about connected events.

He does at this point bring in another example to support the regularist claim, the old case of the drug curing the disease. He portrays his philosophical opponent as

thinking that there is a difference in the concept of "cause" between an understanding based merely on having observed that cures follow upon the use of the drug, and understanding the whole physiological process by which the drug has its effect, where the latter is a genuine causal understanding. His opponent may not want to accept this portrayal; coming to know just how the treatment works should not have much bearing on what we mean by "causes" in this case. In a scientific context, it would be known fairly well what a detailed understanding of the process would look like before it was known just what it is. But we are interested in what Schlick has to say about our understanding of the detailed process.

He provides a physiological sketch of a cure process, with the aim of showing it to be a mere sequence of contiguous events, and he therefore carefully leaves out any mechanistic terms such as "affects," "acts on," "produces change in," etc., which might be found in such an account if it were informally given. This caution in language makes his story¹⁰ sound slightly peculiar (emphases added):

The drug, e.g., is injected into the veins, we know it comes into immediate contact with the blood particles, we know that these will then undergo a certain chemical change, they will travel through the body, they will come into contact with a certain organ, this organ will be changed in a particular way, and so on.

And from this series of events, perhaps contiguous, but in any case merely successive as Schlick describes it, we can

derive no different meaning for "causality" than we could from observing numerous instances of use of the drug followed by recovery of the patient; the detailing of the process just gives us more cases of regularity of succession which are known from "experiences in the laboratory" (presumably experiments with tissue samples, etc.).

It is important to see what Schlick is trying to get us to think when he describes this causal process as a series of distinct events, simply following upon one another. On the one hand he is suggesting that there is an illusion here that we understand causality in a way which is something more than regularity of succession, and that this illusion comes from the contiguity, or at least the nearness, of the events in space and time. But, he wants to argue, even if the events are in fact contiguous, this implies no more of a connection between them than their side-by-side succession, i.e., his claim is that any connection between them that we might speak of is entirely based on observing them (in the laboratory) following a certain succession. Thus if we were to speak, as we well might, of the drug infusing some tissue and acting on it in such a way as to change its chemistry, we would only mean that the drug moved into the tissue and that the tissue subsequently changed. "To act on" designates an illusion of connection which cannot actually be found in an analysis of a process.

A causal "chain" as he sees it is like some bricks lined

up end to end, perhaps actually in contact, but capable of no interconnection. Each event in the "chain," like a brick, is a quite discrete particular. "Connection" between them might at most mean that they are touching, contiguous. Either there is only one event, as when there is one long, unbroken brick, or if there are more than one, then the closest possible intimacy between them which can hold up to analysis is that of contiguity, being directly adjacent to one another.

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If some of the events...

...are still separated, we have to look for new events between them, and so on, until all the gaps are filled out and the chain has become perfectly continuous in space and time. But evidently we can go no further, and it would be nonsense to expect more of us. If we look for the causal link that links two events together, we cannot find anything but another event (or perhaps several). Whatever can be observed and shown in the causal chain will be the links, but it would be nonsense to look for the linkage.

I do not dispute with Schlick that whatever we find to fill in the causal chain will be events. What I want to do is examine whether there is any reason to conclude that our understanding of successive events (in general) and their relations in space and time is constrained to picturing them as like bricks, so that the farthest analysis of a causal process can only find them lying end to end, contiguous. I will name this the "mosaic theory of events." It says that events can only compose into a more extended process the way objects combine into larger objects, e.g., the way bricks

compose a house. It can also be called an object-picture of events. (Interestingly, Schlick's own metaphor is unfortunate for his argument. Links do not compose a length of chain contiguously, but overlappingly.)

The consequence of the object or mosaic theory of events and their relations is that in talking about a process such as the physiological action of a medicine against a disease, terms as "affects," "brings about a change in," "acts on," etc., have as their meaning only a chimera which dissipates with correct thinking. These mechanistic terms, taken in an ordinary sense, speak of event-connections. My concern is that the philosophical attack upon the connectedness understood in this language may involve an uncritically accepted theory about events.

Hume generally refers to the members of the cause-effect relation as "objects," and to their relation as one of contiguity in space and time. The following passage summarizes his analysis of causation and contains his concluding definition of "cause":
12

When I examine with the utmost accuracy those objects, which are commonly denominated causes and effects, I find, in considering a single instance, that the one object is precedent and contiguous to the other; and in enlarging my view to consider several instances, I find only, that like objects are constantly plac'd in like relations of succession and contiguity.

In view of Hume's fateful language of "objects," the mosaic theory seems well embedded in the empiricist tradi-

tion. It involves the assumption that an analysis of a causal process into its constituent events will ultimately come to a stop at a mosaic of (at best) contiguous parts. That we are not bound to this assumption might be shown if we can apply a genuinely alternative picture of event relations.

If we are trying to conceive how in general events might compose into a causal process, and we want to supply these composing parts with genuine connections, we might try thinking that a causal process is analogous to a spectrum of colors, with distinct regions of color representing its constituent event-parts. Different colors in the spectrum are connected by continuous transitions. This analogy would support the supposition that language such as "brings about" means the genuine connection of one event with another. It shows that connected difference is a possibility.

Suppose we make use of the analogy and try to analyze a spectrum further, just as Schlick suggests we analyze further the causal process of a drug remedying a disease. A spectrum has the obvious analysis into a series of distinct colors, so that further analysis will be interested in the transitional regions joining these successively ordered regions. Though it would normally not be the case in an actual spectrum of colors, these transitional regions might, in some case of spectral variation, be themselves organized spectrally. Therefore what the further analysis will find will be either (a) more distinct and connected regions (a more detailed

spectrum) hence more transition points for further analysis, or (b) if the analysis has proceeded to the level at which there are no longer more or less unvarying segments (particular colors), there will be regions of continuous variation. Clearly, there can be no ultimate analysis of a spectrum into contiguous, mosaic-like parts.

How might it be decided whether one or the other mode of analysis of a causal process, into discrete parts or into connected parts, is correct? On the side of the former, it seems one can speak about the causal examples in terms of separate, successive events, as in Schlick's description of the disease treatment process, without using causal language such as "makes," "produces," "acts on," etc. We can, in other words, leave out any mention of causal connections and still have a detailed description. But this is the case with a spectrum of colors just as it is with a mosaic of objects; one can speak of the ordered succession of distinct colors without mentioning the transitions connecting them. Thus the analogy with a spectrum would allow the discrete treatment without the connections being undiscoverable. It would allow causal connections to belong to events and still show why the traditional empiricist or phenomenalist treatment is a possibility: simply because in spite of genuine connectedness, what are connected are distinct and separately identifiable things, and so can be treated as discrete by ignoring part of the example.

It would be helpful to have an example of two events with their connection clearly displayed, rather than relying solely on the color spectrum analogy. A hammer driving a nail is a case of a productive process which is given by Salmon (cited in Chapter 4). Suppose we analyze a single hammer blow into two events: the motion of the hammer and the penetration of the wood by the nail--two distinct object-motions. The first can be said to bring about the second. Is the productive connection on display in the example? Pre-reflectively, one would certainly attribute "making" and "causing" to this case.

It is clear that if the hammer blow is so analyzed, the two events are not merely contiguous. The motion of the hammer is the only one that even occurs independently of the other; when the nail moves into the wood, the hammer moves along with it. Thus there is a region of overlap or intersection of these events in space and time, which includes the whole of the nail motion, but is only a part of the hammer motion.

This terminal penetration of the wood by the nail with the hammer following it can be seen as the product of the antecedent hammer motion. This account of overlapping events is compatible with saying, "the impact of the hammer makes the nail penetrate the wood." This language is also compatible with describing the hammer blow as a single event, process and product.

But instead of either speaking about it with the ordinary causal language, or talking about connected events, Schlick or Hume might want to give this analysis: there are two merely contiguous events, first, the hammer proceeds up to the head of the nail, then, as a separate event, nail and hammer proceed together beyond this point-instant of contact with the nail penetrating the wood. Thus no productive connection is actually to be found in the example. So far as this complete analysis is concerned, there are only these two side-by-side events; causation, the bringing-about of one by the other, is only our mental contribution, from experience and expectation, to this bare succession.

If this traditionally empiricist analysis of causal process, making use of the mosaic picture of event relations, is possible in this causal example, then surely it could be opted for as an analysis of any succession of events, with some minor forcing of the description. But what is important to recognize here is that it does involve deciding upon one of two alternatives. One option has individual events which merely lie there in the spatio-temporal segment which they occupy, with at most a common boundary. The other option is that of a connected process, with one part bringing about the other. Both are possible, and both make use of an analysis into event-parts.

What comparative strengths do these possibilities have? One allows us to talk about something produced, the embedding

of a nail, by an antecedent process, the swing of a hammer--a case of perfectly real, connected events in productive relation. This is what makes sense in turn of the carpenter's action of making and building. The mosaic view excludes this and makes the origin and sense of the causal language as a language of connections mysterious, perhaps placing it in the mind rather than in the physical events, as Hume did.

On the side of the mosaic view one can say that it gives in a sense a clearer picture in that the events have a distinct boundary. This view might say of the other possibility, "Connection is supposed to be represented in this description by overlap of events. But what does overlap of events mean? Why should it mean causal connection? It looks like just an indistinctness in this particular analysis, so that the parts blur into one another." This indistinctness of boundary is precisely what is needed, according to analogy with the parts of a color spectrum, for connection between differentiated things. But it seems one can provide no proof, against this objection, that the overlap or interconnection between hammer motion and nail motion represents causal bringing about. Perhaps this just shows that our understanding of production here is solidly pre-reflective, so that the connection which is more than contiguity is renderable in the abstract only as a pair of events having a sort of Venn diagram description of overlapping regions. But even this mode of analysis thwarts the claim that nothing

that can be specifically designated "connection" is to be found in such cases.

The clarity (or rather, tidiness) which comes from picturing events as if they were discrete objects seems to have no advantage here in terms of accuracy or completeness in description. And it is bought at the expense of the ordinary way of talking about the example. Furthermore, the analysis into hammer motion event--nail motion event, though rough, gives a more natural description of the case than that which would divide the parts at an instantaneous slice of space and time.

I will now offer an example of different events which are indisputably connected though not causally. Most symphonic compositions have major sections, movements, which are separated by silence. Some symphonies, however, have movements which are joined by segue, in which the music does not stop. A chord or a note sustains a transition into a different mood, and perhaps a different key. This serves as a counterexample to the claim that different events can only be either separated by a gap or contiguous.

Positivists On Indeterminism

In the second part of Schlick's article, dealing with causality in "recent science," he makes use of some developments in physics which were new in his time, to make a different argument for the regularist view about causality. He

takes as his focus the apparent indeterminism which is observed in detailed experiments with light and other radiation.

This feature of experiments concerns the specific atomic locations at which radiation interacts with matter, or the exact timing of such interactions. There are only equations which yield probabilities for specific sites or times of interactions, given the antecedent conditions; there are not equations which predict determinate sites or times for the events. As a result of extended investigation and analysis, it is generally accepted by physicists that this is a genuine indeterminism, unlike the randomness in the outcomes of a deterministic process such as coin tossing, in which the randomness is due to limitations in knowledge of details about the particular occurrence. I will limit my discussion to indeterminism about atomic locations of interactions.

Schlick's analysis of the everyday notion of causality had reached the conclusion that all this notion could mean is that certain regularities are observed which can be formulated into laws expressing our expectations and our applicable knowledge. In the second part of his article he claims to find "striking confirmation" of this view in quantum theory (in which the probabilistic equations occur). Here is¹³ how he begins his discussion of this.

As is well known, the quantum theory in its present form asserts that a strictly deterministic description of nature is impossible; in other words, that

physics has to abandon the Principle of Causality.

The Principle of Causality is just the principle of determinism, that everything that happens has an antecedent cause. Insofar as this principle is taken to be something that leads a scientist to look for a cause for why an individual interaction in a radiation experiment occurred, e.g., just where it did and not at any other place, it is indeed true that physics has abandoned the principle. Giving up determinism in just such points of detail in scientifically studied and measured events is what Schlick can cogently mean in saying that a "strictly" deterministic science is no longer possible.

But then it seems rhetorically heavy-handed to call this the abandonment of determinism by physics. Schlick seems to realize this:

...for all ordinary purposes of science and everyday life the deterministic attitude not only remains justified but is the only one compatible with our knowledge of nature.

In spite of this recognition on his part, in order to make his case that the discovery of indeterminism in atomic experiments has impact on the general meaning of "causality" in such a way as to support his analysis of the concept, he does need to be making the claim that because of this discovery, physics abandons "causality" as it is normally applied in science, that is, unaffected by the Humean critique. His

words immediately following the last quotation are these:

"What does physics mean when it thus denies causality?" The Principle of Causality--determinism--is identified by Schlick with causality. When physics gives up on the expectation of "strict" determinism it "abandons" determinism, and furthermore, "denies" causality. This is a very fast slide of logic. It needs slowing down and examining.

It has already been noted what Schlick himself seemed to admit, that the highly specific unpredictability in experiments easily allows that for nearly all practical and explanatory purposes the world is deterministic. What therefore could be meant by saying that because of this indeterminism causality should be "denied"?

If there were some kind of general indeterminism, that is, if to some degree regularities were not reliable or there were no regularities in the world, it might be said that to get things right would be to deny causality. But one can avoid this murky imagining, because there is a concrete case of indeterminism at hand (if one adopts the generally accepted view in physics). What does indeterminism as a feature of certain effects say about causality as applied specifically to those observed events bearing this indeterministic detail?

Supposing that a realist view about the unobservables of physics is sustained, then these effects are produced by the kind of transmission that is called radiation. The effects are individual localized interactions whose sites in detec-

tion material appear to be outside all possibility of prediction. "Denying causality" here could only mean determining that there is no discoverable cause for why the radiation produces an effect at one specific site rather than another.

But a summary description of this case would be that within a certain context of the causal production of events there are specific features of the events which appear to have no cause: their exact locations. "Denying causality" is thus a wildly inappropriate characterization. What is appropriate is to say that a certain presupposition about causality is being challenged: the presupposition that it¹⁴ must always involve a thorough determinism.

The clearest point Schlick is making with his "denial of causality" is just that there are no mathematical models to predict sites of interaction, and that if the existence of specific causes is denied where predictive knowledge is lacking, this at least fits with the claim that causality is nothing more than predictability, as Feigl said. Schlick writes:

What is the real reason [for rejecting determinism]? None other but that it is found impossible to predict phenomena with perfect accuracy. Within certain well-defined limits it is impossible to construct functions which can be used for extrapolation. [This] proves that the physicist in his actual proceeding has adopted just that view of causality which we have been advocating.

And later:

We found that science confirmed the view that causality must be understood as "possibility of extrapolation," because we found that this was exactly the sense in which the word is used in quantum physics.

But it was not physics which said that the discovery of indeterminism by quantum physics meant the denial and abandonment of causality (on the naive productionist understanding), but Schlick. It was noted above that a productionist reading of the case is possible.

What is true of physics is the absence of the possibility of non-probabilistic prediction in certain cases. But if "possibility of extrapolation" means the existence of useful predictive mathematics, then this possibility did not fail quantum physics. Quantum theory is a brilliantly successful mathematical equipment, with many important applications, though its use involves merely probabilistic and not deterministic prediction. So there is really no failure of the "possibility of extrapolation," but rather highly successful extrapolation which is not fully deterministic. Of course physics did not suddenly find itself in the midst of senselessness and absence of regularity, rather, there is no lack of quantifiable and utilizable regularity.

In Schlick's thinking, causality and determinism are inseparably wed, in contrast to my suggestion above that a challenge to this presupposition is present in the facts:

Lack of determination means pure chance, randomness; the alternative "either determination or

chance" is a logical one, there is no escape from it, no third possibility.

The claim here implies that there is no room for a causal process with certain statistical outcomes, such as our commonsense assumption--that the observed interactions are causally produced--would like to see in these cases of indeterminism. But it should be recalled that Schlick thinks that the only viable meaning for "causality" is "consistently observed succession," in other words, what can be formulated by science into laws. And there are probabilistic laws at the very center of this discussion. So really Schlick should recognize a meaning for probabilistic or statistical causality, even on his analysis of the concept--namely, the statistical regularities designated by the laws of quantum theory.

Schlick cannot see that there may be certain causal processes (however this is understood) with detailed facts about their outcomes following only statistical laws. He is oblivious to the possibility of statistical regularity within a causal and deterministic context.

Once the possibility of statistical regularity is admitted, there can still be two views of causality: that it means nothing but regularity in observation (in these cases statistical), or that there are connecting causal processes, with some purely statistical outcomes, underlying these regularities. The existence of indeterminism does not argue for one or the other of these positions. There are evidently

such things as indeterministic regularities (the laws of quantum theory show this), and there may be causal production with indeterministic features. If there are no clear signs of an available description of such a process which maintains continuity in events, this is not already a logical constraint against such a description, since it may only mean a new stage of inquiry is in the offing.

Philipp Frank provides more circumspect discussions of the relations between causation, predictive mathematics, and indeterminism, both in a work already cited¹⁵, and in a contribution to the ambitious Positivist tract International Encyclopedia of Unified Science.¹⁶ For Frank the Positivist outlook, a combination of the regularity view of causation and antirealism about scientifically "postulated" entities, is taken as a matter of course. Frank defines the "law of causality" in this way: If all the variables in the initial state of a system are known, the values of these variables in a future state of a system can be predicted.¹⁷ Based on this definition, he claims¹⁸ that, "the question of whether the law of causality has survived in twentieth-century physics cannot be answered by a simple 'yes' or 'no.'"

His reasoning is based on the fact that, on the one hand, physicists talk about the computational devices of quantum mechanics as though they referred to a set of mysterious physical variables of a system which evolves deterministically, though on the other hand, as regards observations

these devices predict only probability distributions and not site of individual interactions. The predictive mathematics, on a predominant interpretation by physicists, designates a "probability wave" which, as if it were a propagation in the conditions antecedent to measurement, evolves deterministically.

In the earlier work, the contribution to the Encyclopedia, Frank does not know what to make of what quantum physicists call "state variables," since they do not appear to correspond to Newtonian masses. Thus, he could see no clear way to compare quantum mechanics with deterministic Newtonian mechanics in order to decide whether the "strictly" applied law of causality--determinism--still applies in this domain. In the later work he simply claims, as quoted above, that this question has an ambiguous answer.

The approach of the present work is not inclined to ascribe any physical interpretation to "the evolution of a probability wave," hence will give it no voice in favor of "strict causality" in physical events. Instead it will view the equations of quantum mechanics (together with these "state variable" models) in the same instrumentalistic way as other mathematical theory. This avoids any confusion, as though one were hedging, about whether there is indeterminism in the detailed effects of radiation. The possibility is allowed that there is, within a context of productive physical process, indeterministic events covered by probabilistic

laws.

My suggestion about indeterminism could be put in the terms of Frank's formulation of the law of causality: the law would be amended to allow the possibility of an ingredient of intrinsically unpredictable detail, whose causal traces in events do not extend all the way back into the given antecedent state of the system, within an otherwise deterministic context.

Hobart's Defense of Hume

Another work whose conclusions Feigl claimed to be representing with his pronouncement about the purification of the concept of causation, is R. E. Hobart's article, "Hume¹⁹ Without Skepticism." This article brings up for criticism a view about causality which is different from its analysis as a "habit of mind" built up through accumulated observations, and also different from the view that causes logically necessitate (or otherwise necessitate) their effects. But it also is something beyond my prephilosophical, "productionist" assumption. This is the view of A. N. Whitehead that "causal efficacy" was something like a primitive feeling that comes with our organism, arising from our bodily encounter with the world. It is not an a priori condition of perception, but a form of perception, though different from sense data.

Hobart's discussion focusses on a text of Whitehead's in which he disputes Hume. The expression "presentational im-

mediacy" is Whitehead's term of art for the usual sense data which, in Empiricist philosophy, are the foundation of knowledge. Here Whitehead presents his idea that the feeling of causal efficacy is a different mode of perception, one overlooked by Hume.²⁰

In the dark, the electric light is suddenly turned on and the man's eyes blink...The philosophy of organism [Whitehead's philosophy] accepts the man's statement that the flash made him blink...[Hume] first points out that in the mode of presentational immediacy there is no percept of the flash making the man blink...Hume refuses to admit the man's protestation, that the compulsion to blink is just what he did feel. The refusal is based on the dogma that all percepts are in the mode of presentational immediacy.

Hobart, defending Hume and positivistic Empiricism, disputes Whitehead's alternative view as it is sketched in this example. Hobart's subsequent defense of Hume against Whitehead is highly turgid, but his clearer thoughts can be found scattered in the text. He tries to argue that²¹
Hume...

...is not denying causation or derivation, or what we mean by the word "made," he is analyzing it... he says that the real production of one event by another, in the common human sense of these words, may be proved to consist in what he specifies.

We do not begin to learn what this is until the next page:

Hume says that the flash makes us blink in that, given the circumstances, the blinking does inescapably follow upon it; that to make a thing happen means to occasion it, or be the occasion of it, unconditionally--without chance of another

sequel--and could not intelligibly mean anything else. The flash makes us blink, that is, when it occurs we blink forthwith as a matter of course and cannot help blinking...The flash calls the blinking into existence; that is, when it calls, the blinking comes into existence.

This is a Herculean effort to portray both pure necessity and pure regularity of succession, and at the same time be claiming to explicate our ordinary idea of "to make happen." Hobart is correctly representing Hume as thinking that causation in its ordinary meaning is necessitation or "necessary connection," and also we again see the caution about language which tries to eliminate causal action terms and to portray or suggest bare succession. Real murkiness and confusion results here from a complex and deliberate warping of language.

Hobart does not give Whitehead's example an accurate or adequate treatment. For one thing, Hobart ignores Whitehead's point about the feeling of compulsion as a candidate for a "percept." Leaving aside this claim of Whitehead's, this example of causation, if that is what it is, is clearly not the necessitation of one event by another.

In this example, when the man says, "the flash made me blink," or "it caused me to blink," or "I felt the compulsion to blink," he does not mean that it was impossible for him not to have blinked. Suppose a friend of his had a camera, and wanted to obtain a candid shot of the man groping about in the dark--an artistic technique, let us say. The friend

says, "Now the flash will surprise you, but whatever you do, do not blink." On Hobart's view, either the man cannot comply with his friend's request, or if he can, then the usual mechanism, producing the compulsion, is now somehow gone, so that there is nothing for the man to overcome.

One might say that the flash-blink mechanism is a kind of reflex, or near-reflex. But even the paradigmatic knee-tap reflex is not always something one cannot overcome. When the compulsion to blink is overcome, there is the flash, but not this particular effect of blinking. To overcome the impulse or compulsion to blink does not eliminate the causal action of the flash. That one has to exert control to prevent the effect shows that something happens to one, something arising directly out of the light flash.

It is important to notice what it is about this discussion of causal examples that suggests abandoning the usual Empiricist approach. Notably, these reflex-like examples encourage the avoidance of thinking in terms of necessity. And the discussion shows how, though the Empiricist's description in terms of mere regular succession, at most contiguous, is always possible, it also might well be leaving something out of these cases. Making sense of these examples favors the un-"purified" application of causal language and its implication of event connections--for a productionist view of causality.

What is conspicuous in the flash-blink example, and I

will have occasion to make use of this again, is that a certain causal process could have one of its usual effects not happen, and still be the same particular process, the same otherwise causally efficacious actual occurrence, that it normally is. Necessity of succession is thus ruled out, and causality is attributable to the case despite the absence of the usual succession. I do not use the example to support Whitehead's general theory of causality, but as an excellent illustration of interacting causes, which, as we have seen and will continue to see, are cases about which the acausalist tradition is highly vulnerable.

Is "Productionism" Anthropomorphism?

A type of objection in the Humean tradition to my preferring a "productionist" alternative to the regularist view must be addressed. The objection may be either that this word says nothing more than that something which is designated a "cause" has the power to produce something, or that it is derived from the "feeling" of action or muscular exertion; in either case, the objection would claim, the ordinary language of causation is anthropomorphic.²²

My answer is that "production" does not necessarily imply an attribute of power had by something, nor is it necessarily derived from human action.

One might conceivably say that a certain drug has a power to cure a certain disease (as a loose figure of

speech). But if antecedent events are what is designated "causal" and adduced in explanation, namely, the mechanical flows and interactions of chemicals leading up to recovery, then neither this event-sequence as a whole nor any of its part-events is anything about which one would say it has a power. An antecedent causal event is not the kind of thing that can "have a power"; the relevant "attribute" is simply the fact of production. The causal event produces by happening. What is there here about which one can say it has a power to do something? Causality in events is not a priori a matter of latent properties of causal objects.

Efficacy through action by humans and animals is only a special sense of "production." This word is really just part of a complex of more or less interchangeable causal terms, such as "bringing about," "making happen," and "giving rise to." As in Hobart's usage, "production" can be used to speak in the indefinite case about a causal sequence of events. On my view, causal language just bespeaks the fact that transitional connections exist joining events, so that human action would be only a particular context for such connections. "Power," on the other hand, is most likely the projection of human skills, capacities, and capabilities upon a cause--and perhaps, in a way objectionably loose for philosophical purposes, for the identification and description of a cause.

My own critique of the contemporary use of "powers" and similar attributions in reference to unobservables in phys-

ics, a critique which is close in its conclusion to Hume's charge of anthropomorphism, is given in the next chapter.

Conclusion

What I claim to have accomplished in this chapter is this: I have shown that the philosophical conclusion about causality formed by the tradition of Hume and Positivism is not without a viable alternative. The two main props of the Humean view are not sufficient to deny that causal connections belong to the actuality of natural events.

One of the props is the idea that causation, if it means more than regularity of succession, must mean some kind of necessitation. This had very little support in terms of what can be clearly and cogently meant by "necessitation," and in any case, this alternative meaning of "cause" was defeated by counterexamples. The necessitation view seems to be traditionally used as a foil for arguments toward the regularity view, in that once set up as the only other option, it falls rather easily.

The other prop is the assumption that the analysis of a process into its parts can only turn up a disconnected, at most contiguous, series of events. There is the alternative to this picture that a causal process may be analogous to a spectrum of variation, in which case though the process has distinct parts, the analysis into just these discrete, successive parts must in the nature of the case be incomplete.

There are also graduated transitions. A look at the causal example of a hammer blow to a nail showed that an analysis into contiguous events, far from being the only option, seemed not to be the best analysis.

The viability of productionistic explanation is upheld against the philosophical critique of causal connection.

Notes

1. Herbert Feigl, "Some Remarks on the Meaning of Scientific Explanation," Readings in Philosophical Analysis, ed. Feigl and Wilfrid Sellars (New York: Appleton-Century-Crofts, Inc., 1949) 510.
2. David Hume, A Treatise of Human Nature, (Oxford: Oxford U P, 1978) 167.
3. Ernst Mach, History and Root of the Principle of the Conservation of Energy, trans. Philip E. B. Jourdain, MA (Chicago: The Open Court, 1911) esp. p. 57.
4. A good account of this history can be found in Wesley Salmon, Scientific Explanation and the Causal Structure of the World (Princeton: Princeton U P, 1984) 214-217.
5. Herbert Feigl, "Notes on Causality," Feigl and May Brodeck, eds., Readings in the Philosophy of Science (New York: Appleton-Century-Crofts, 1953) 408.
6. Moritz Schlick, "Causality in Everyday Life and in Recent Science," Feigl and Sellars, Readings, 515-533.
7. Schlick, 516.
8. Schlick, 518.
9. Schlick, 520-521.
10. Schlick, 521.
11. Schlick, 522.
12. Hume, Treatise, 170.
13. Schlick, 530-533.
14. Wittgenstein tried to present this same suggestion in a different philosophical context. The controversial and difficult passage occurs as entries 903-906 in Remarks on the Philosophy of Psychology, ed. G. E. M. Anscombe and G. H. von Wright, vol 1 (Chicago: U of Chicago, 1980).
15. Philipp Frank, Philosophy of Science: The Link Between Science and Philosophy (Englewood Cliffs: Prentice-Hall, 1957)

16. Philipp Frank, Foundations of Physics, Section 47, International Encyclopedia of Unified Science, vol. 1, pt. 2 (Chicago: U of Chicago P, 1938) 478-479.
17. Frank, Philosophy of Science, 287.
18. Frank, Philosophy of Science, 348.
19. R. E. Hobart, "Hume Without Skepticism," Mind XXXIX (1930): 273-301
20. Alfred North Whitehead, Process and Reality (New York: Macmillan, 1929) 266.
21. Hobart, 280.
22. Rudolf Carnap, Philosophical Foundations of Physics (New York: Basic Books Inc., 1966) 189.

CHAPTER 3

THE REALITY OF UNOBSERVABLE CAUSAL EVENTS

This chapter and the next will be concerned with a diverse group of contemporary philosophers who fall in the category of "causalists" or "causal realists," though their philosophical positions do not, in my terms, amount to productionist points of view. Their goals, nevertheless, include one or more of the following: (a) to show that the Humean account of causality is incomplete; (b) to defend the reality of the unobservable causal entities of physics, such as forces of attraction or "particle" transmissions, against those who claim that these are merely useful constructs of science; (c) to furnish the latter entities with some kind of general characterization.

My interest in these writers lies in showing how attempts to speak constructively about unobservable causes in physics land philosophers in forms of acausalism, even when they take positions opposed to the traditional forms of acausalism, such as the Humean analysis of causality or antirealism about unobservable entities. This phenomenon is one effect of a conventional assumption about the role of philosophy of science. The assumption is that philosophy can do nothing more than serve to clarify or interpret concepts

and laws in application by physics. The consequence of this assumption here displayed is that causal realism is largely adrift of its proper course: the furtherance of production-istic explanation.

The causal realist arguments to be studied are divided into two categories. Those which put forward some positive philosophical conception of causal occurrences outside observation in an attempt to further their understanding, I will name "constructive realisms." Where philosophers steer clear of such substantive suggestions as to ways to talk about these occurrences, and instead give arguments limited to attacking antirealism in defense simply of their reality, I will call the arguments "critical realisms." Nancy Cartwright gives both kinds of arguments; her work will be treated under both aspects.

One mode of constructive realism is discussed in this chapter: the popular "tendency" and "propensity" analysis of the unobservable. The final chapter will be concerned entirely with constructive realism. The conclusion is that whatever value any of these ontological attempts may have, they do not speak unambiguously of real events, as is essential if the aim is to supply or clarify causal explanation. All contemporary realisms which intend to apply some descriptive concept or other to this special class--unobservable causality in the domain of physics--labor under some form of this same limitation. They fall short of clearly attributing

to the "entity" causal actuality. Because of this they remain a sort of satellite system around acausalism, kept in motion by a vague yearning.

This problem does not occur with the critical realist focus in texts of Nancy Cartwright¹ and Ian Hacking.² These arguments claim that the life-activity of experimental and engineering science, working with the "entities" in question, involves the recognition that modern experiments and technology detect and put to use what Cartwright calls "concrete causal processes,"³ which are named and differentiated by the familiar "particle" terminology of physics. The upshot of critical realist arguments is simply to point out that, apart from any philosophical reasoning, and also apart from any theoretical/mathematical invention, scientists ongoingly presume that their experimental work has to do, e.g., with real transmissions of charge (electrons) and real electromagnetic transmission (photons). Cartwright and Hacking show that causal attributions can belong to the context of the activity, and need not be a product of theorizing. This critical realism will be discussed further in the concluding portion of this chapter.

A major strategy of critical realism is to keep in mind the activity of applied physics, rather than fixating upon mathematical theory (laws) in the traditional way. Cartwright's critique of entity-antirealisms based on consideration of theories leads her into attacks on what can be called

"theory-realism"--roughly, the claim that the mathematical predictive models which physics invents and adopts not only "describe" the world, but are "true" as well. Her theory-antirealism takes the form succinctly expressed by the title of her book, How the Laws of Physics Lie. Her claim about the "untruth" of certain laws turns out, at least in the case I will presently examine, not to be only a skepticism which somehow attaches to a formula, but to be an antirealism about the forces whose quantities certain laws specify as a function of other measured quantities.

Whether laws are true or false, or whether this is even an appropriate question considering the kind of thing a law is, will not be a concern of this work. In the first section I will consider one of Cartwright's arguments for the falsity of certain laws, because of its significance for questions about causality and entity-realism. The argument of Cartwright's which I have chosen to examine is found in her discussion of the composition or interaction of causes. Her claims about an example of such bring on an instructive dispute with Rom Harré.

Composition of Causes

There is an inverse-square law quantifying the gravitational force exerted by bodies upon each other, and there is a corresponding inverse-square law for the force of electrical attraction. In cases such as the moon's attraction toward

the Earth, the electrical force is negligible, and in the case of electrons and protons in atoms, gravity is negligible. But for the absolutely precise quantification of the actual force exerted by a body upon another, the two laws must always be applied in combination. It seems therefore that that one can never observe the pure forces of gravity and electrical attraction individually at work.

Cartwright concludes from this that these two laws taken separately do not designate "real, occurrent forces."⁴ For her a force is something that really occurs only with "the actual behavior of objects."⁵ She thus takes "force," in Rom Harré's words, as "an alternative term for mass acceleration."⁶ If an object is accelerated, there is a force. The precise measurement of acceleration is the precise measurement of a force. Taking the observations of forces upon bodies to ultimate exactitude, and considering exclusively these observable motions, each law mentions by its term "force" only an ideal acceleration of a body in which only this force and no other is operating. With this approach, what is thus mentioned or referred to in the individual law is non-occurrent. It is very important for understanding the course of Cartwright's thinking to see that, for her, observable motion is the sole reality with which the concept of force connects.

She expresses her claim as a difference with Lewis Creary. Creary thinks component forces are real as causal

influences (transmitted, in some sense, from one object to another) and add up to a consequent motion.⁷ Cartwright thinks there is only a real resultant force, given by an actual motion, and quantified by the vector addition of two functions whose individual terms of "force" signify...well, perhaps something. What might it be?

She suggests an answer, though it is one with which she herself is not in the end satisfied. Perhaps the component forces are the "causal powers that bodies have."⁸ Here she attempts, for the sake of argument, to make use of a distinction which she acknowledges to have been declared illicit by Hume: that between power and the exercise of it. Powers "exercized" (she suggests) can be the "occurrent" forces shown in mass accelerations, whereas the forces individually signified by the laws of gravitational and electrical attraction are the "unexercized" powers had by bodies.

"Unexercized" must mean that such a power is an unactualized potential. But then how could it be something contributing to a resultant force? Would it not be active and hence actual in such a role? Cartwright claims that this suggestion about the powers of bodies "makes sense of the story about vector addition."⁹ But potentials which remain unactualized cannot be the realities represented by vectors which are added into an actually occurring force. What occurrence would their addition represent? With merely an unexercized power, nothing occurs; there is no individual

action, hence no interaction, of causes. The resultant occurrence would have to arise spontaneously from inactive potentials, rather than being produced by something. This is not to give a causal meaning to individual laws. It is not to say that, let alone how, two forces bring about a resultant motion.

This turns out to be just what Cartwright wants, due to her zeal to show laws "lying." To say that a law "describes" only a power had by an object which is not put to effect is to say that the law "describes" no actual occurrence. The procedure of adding vectors to obtain a resultant force, the "vector addition law," however, does "describe reality," since the force yielded by this compound algorithm really occurs as the motion of a body.

In this argument she is taking issue with Creary's "causal influences," which, on his view, are evidenced in the force of gravity and the force of electrical attraction. She thinks that a realist about a particular law has the burden of pointing to the specific "effect" that the law "produces," by which she means an exact, observable predic-
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 tion derivable from it alone. She says that Creary's influences belong to

a class of suggestions that tries to keep the separate causal laws in something like their original form, and simultaneously to defend their facticity by postulating some intermediate effect which they produce, such as a force due to gravity, a gravitational potential, or a field.

Creary's idea appears to her to be an effect thought up to
 fill the bill:¹¹ "Creary's influences seem to me just to be
 shadow occurrences which stand in for the effect we would
 like to see but cannot in fact find."

She thinks gravitational and electrical forces are
 stand-in hypotheses with no concrete evidence. But her anti-
 realism about these influences does not arise from tradition-
 al epistemology. She does not disbelieve in them because one
 could never in principle watch such an influence, whatever it
 is, happening, could never catch it in the act of transmis-
 sion. In fact, for her some unobservable "intermediate in-
 fluences" are evidently real things:¹²

I see no reason to think that these intermediate
 influences can always be found. I am not opposed
 to them because of any general objection to theo-
 retical entities, but rather because I think
 every new theoretical entity which is admitted
 should be grounded in experimentation, which
 shows up its causal structure in detail.

Her position seems related to the practice in physics of
 considering gravity radiation hypothetical, strictly speak-
 ing, until such time as it is experimentally detected in the
 same manner that other forms of propagating radiation ("par-
 ticles") are detected. Her position is that a force should
 not be believed in merely because it is a term in a law
 quantifying it under given conditions. And in the end she
 does not seem to be happy with a realism about laws based on
 causal powers.

Thus she ends up with a kind of antirealism about certain individual laws, as the title of her book indicates, and this turns out to mean an antirealism about the unobservable causes giving rise to the forces mentioned in the laws. And from the point of view of particle physics, one might take her to be disbelieving in the graviton. The question remains open how her philosophical skepticism is related to a vestigial scientific agnosticism about whether such a cause would show itself in a different experimental context yet to be produced.

Rom Harré takes on Cartwright and makes the opposite claim about composition of causes.¹³ For him the composite forces are realities. He wants to say they are real in that they are "tendencies," "propensities," or "dispositions," which only manifest themselves insofar as there are no countervailing "tendencies" which nullify their effects. By this, he thinks, an actual effect (meaning an observed effect) can be said to be the result of interacting influences--the tendencies--yet neither influence has just the observable effect it would have if it acted alone, just as a tendency may be not manifested or only partially manifested.

This "tendency" analysis is popular with philosophers of science and originated with physicists themselves,¹⁴ but in fact, though "tendency" may be in some way a useful designation, it has intrinsic limitations when used in a constructive realism, as I will be showing in these two chapters.

Harre's "tendency" description of individual forces has two basic flaws. First, it does not quite manage to engage Cartwright's argument. Either a tendency is activated or it is not. Cartwright's argument would apply the same way to tendencies as to Creary's influences: even if what is thought to be interacting are tendencies, her argument is that neither are activated in any real case. The component force as a tendency would still be no real occurrence. Is a tendency in any case a real occurrence?

What is more important for my own claims is a point I will make presently and in the following chapter, that tendencies, propensities, and dispositions are not what is needed for a causal ontology for contemporary physics.

Ontology of Tendencies, Powers, and Capacities

Harre seems to realize that a tendency by itself is not a thing, so that a subject is needed to have the tendency:¹⁵

Tendencies are the dispositions of individuals. This cannonball has a tendency to fall, this electron has a tendency to accelerate toward an electrically charged plate, and so on.

This sounds a lot like Cartwright's suggestion about causal powers. In the following passage, Harre adopts "tendencies" as what are "described" by laws. He does agree with Cartwright that individual laws do not "describe" actual events. But he disagrees with Cartwright in that he thinks her anti-realism about forces concluded from this¹⁶

is the fallacy of 'actualism.' It prejudices the way that the laws of nature engage physical reality. According to the arguments of this chapter, laws of nature do not describe sequences of actual events but rather the tendencies that produce them. Tendencies are almost never exactly realized because of countervailing tendencies.

Tendencies, for Harré, are what allow one to be a realist about laws of nature--that is, about the forces they designate. And, from the quotation preceding the last, they are properties of the objects which exhibit motion due to forces. But from his way of speaking in this last quotation it is as though tendencies are to be taken as active yet somehow non-actual productive forces which bring about "actual" events--presumably observable motions. Harré wants to advocate a "modest" causal realism. But about these "modestly" real things, the forces of attraction signified in laws: are they latent powers of objects, or are they actual productive events? "Modest realism" seems to designate an unresolved ambivalence.

"This cannonball has a tendency to fall." What could this be saying? Well, it will fall if we let go of it. Should we say it has a tendency to fall when we let go of it? No. When we let go of it, it falls.

It weighs quite heavy in the hand. Is its weight or heaviness as it is held, its tendency to fall? Weight is mentioned by Harré¹⁷ as an example of a tendency. But why call it a tendency? It just weighs so much in Earth's gravitational field. Does it have the tendency no matter where it

is or what it is doing, say, if it is floating around in space remote from other objects? It must, if it is a property of the cannonball that manifests itself under certain conditions. But now weight appears to be a manifestation, not a tendency. "It has a tendency" suggests that it occasionally exhibits weight where there are no other objects.

Suppose the cannonball is on display in a science exhibit. It is atop a slender iron pedestal. A guide explains to onlookers, "Here we have an illustration of the force of gravity, in that this cannonball has a tendency to fall."

Some onlookers are puzzled. "Is something wrong with the pedestal?" "What are we supposed to be looking at?" One of them thinks hard and says, "Is the pedestal deliberately designed so that the cannonball tends to fall off and illustrate the force of gravity?"

"No," the guide patiently explains, "the exhibit is that this cannonball, just as you see it, has a tendency downward."

"Oh. You mean the thing weighs quite a bit," says one onlooker.

"I can believe that," says another. "And you had better be careful standing around it if it has a tendency to fall off."

Why should one say that the electron and the cannonball have tendencies, just because they respond to certain environmental conditions? They do respond; they do not tend to

respond.

Could one say that the cannonball on the pedestal represents a power in check? A tendency in check? The "in check" implies an opposing force, so that "power" and "tendency" become synonyms for "force." In this usage they become, in other words, not latent properties, but active and actual occurrences, "exerted" forces.

"Tendency" (as a latent property) does not find a fitting application in forces, because no one has ever known a case in which two masses failed to attract each other in the expected way according to the law of gravity (e.g.), regardless of other forces at work. The cannonball can fall off of things, or be dropped, and it has measureable weight. This is all that can be meant.

Suppose then that "tendency" refers not to properties of objects, but to properties of unobservable causal transmission through the space between objects, and thereby also of what is called the field, as one might hope from a venturesome contemporary realism. This is the way that the philosophical physicists Werner Heisenberg and Henry Stapp have used the terms "potentia" and "tendency." Can "tendency" satisfy any or all requirements for a concept of unobservable causes in physics?

We have already heard Harré admit that there are not tendencies by themselves, but that there needs to be something which has them. The next step, then, must be to try

thinking of a tendency as an attribute of an unobservable causal event, rather than of an object. A problem with this is already apparent.

Suppose there is an event or process that brings about something observed. What could be gained by way of an explanation of the observation, by supplying this event or process with a tendency? Is not its causal action all that is needed for explanation? If it has a tendency to do just the thing attributed to it, then it is the activation of the tendency that is the causal-explanatory event. Citing this causal event would be what explains. What role could the tendency as a latent property play?

In any case, it is difficult to conceive how a latent property can be attributed to an event (as opposed to an object) in the first place. What stage or part of the event is the latent property? If it is some stage or part of an event, then it is itself an occurrence, and not a potential for an occurrence.

If a cause has or is a tendency, then (roughly) at some times a certain thing will be brought about, and at other (comparable) times it will not. This is how the concept of tendency seems to serve Harré: sometimes gravity is active, i.e., it "prevails" in producing motion, while sometimes a different force is responsible for the motion, and this is not the motion that gravity would produce in this circumstance. Each force, when its effect is not the significant

one, is thought of as lying latent, awaiting a different sort of circumstance in which it would be activated or manifested.

However, if there is only an unactivated tendency, then there is no causal event at all, just where Harré wanted to claim that in the example taken from Cartwright there are two real interacting causes producing a resultant force. Cartwright and Harré were both motivated to talk about powers and tendencies, due to consciously or unconsciously confining the class of real, active events to observable motions. I will again take up the critique of this strategy for thinking about interacting causes, after bringing to light a different source of "tendency"-type analysis.

In a paper co-authored with John Dupré, Cartwright makes a claim (this time unflinchingly) for causal "capacities," an idea which seems indistinguishable from causal powers, except that the "capacity" seems to be attributed to unobservable¹⁸ events rather than objects. This paper is an earnest, creative, and contemporary attempt to establish the reality of causal unobservables in physics in a way that is directed against the Humean reduction of causality to regular succession. "Capacities" are what there is in addition to regularity in observation:

The Humean tradition downplays capacities, and conceives of them as no more than misleading ways of referring to lawlike regularities. We want to reverse this idea: it is better to think of lawlike regularities as misleading ways of referring to the exercise of capacities. If we try to tailor our causal claims to match the regularities we see

in nature, we will miss a good deal of the causal structure.

One of these assertions calls for criticism right off. Lawlike regularities are occurrent facts; they are not ways of referring, misleading or not. A law might refer to something, namely, a regularity. But what could be misleading about it? It is just a formulation of a factual relationship found by observation.

But the authors's aim is familiar enough: to supplement the account of causality with what Hume left out. It was seen in Chapter 2 that Whitehead tried to specify what Hume left out by talking about a primitive feeling. Rom Harré would fill in the causal picture at one level of explanation with causal mechanisms, and at another (beyond the observable)¹⁹ with powers, tendencies, and dispositions. The ideas of Cartwright and Harré in this regard are attempts at contra-Humean causal realism about physics. My own analysis has indicated that the Humean view leaves out event-connections due to the unwarranted presupposition that events in succession necessarily have the character of discrete objects in a row.

What is a "capacity" for Dupré and Cartwright? Ordinarily one would say that something has a capacity of some sort. Normally, in fact, the capacity would be attributed to a person. The definition of "capacity" in Webster's Dictionary which is relevant to these authors' usage is, "legal qualifi-

cation, competency, power, or fitness." One might, though, try to think of a capacity as an attribute of material things; for example, a quantity of explosive might have the capacity to move a certain mass of rock if skillfully placed (though this may stretch the term), or an acre of forest might have a certain capacity for atmospheric restoration.

This is approaching their meaning. For Dupré and Cartwright, capacities are properties of properties of events.

The authors characterize the Humean view as follows:

Causality is supposed to be about a relation which holds between events, a relation which holds in virtue of the empirically distinguishable properties that events have; the relation consists in, or at least is marked by, the regular association of these properties.

But this is inadequate, in their view, because it would imply that

the right sort of connections between capacities do not exist. Capacities are carried by properties. That is, you cannot have the capacity without having one of the right properties. But the same property can carry mixed capacities, and so the true complexity of the problem cannot be revealed by the associations of properties.

At the beginning of the article they say,

A basic assumption of this paper is that things and events have causal capacities: in virtue of properties they possess, they have the power to bring about other events or states.

As quoted before, they talk about the "exercise of

capacities." Apparently, then, the point is to say that a property of causal events may or may not exercise some particular capacity which it possesses. But this idea has the same philosophical problem as Harre's "tendencies." If the exercise of a capacity is the causal event that explains an observation, then what is the explanatory use of the capacity itself? A capacity "just sits there" in whatever has it, it is not itself an occurrence whose adducement explains. Only a power or capacity put to effect can be a causal event, and to say it has a power or capacity cannot further the explanation of what is thereby produced. And I have made the point above and in Chapter 2 (in the section titled "Is 'Productionism' Anthropomorphism?") that events per se do not "have capacities," or produce "by way of" capacities; they just do what they do. They happen.

In the dispute about composition of causes, the motivation for talking about tendencies and powers was to account for causes interacting with one another while tacitly limiting the category of actual events to observable motions. The motivation of Dupré and Cartwright for talking about capacities and powers is not this, but instead, what is mentioned in the title of the paper: Probability and Causality: Why Hume and Indeterminism Don't Mix. Their motivation is the apparent fact of indeterminism in experiment, where (a) laws make only probabilistic predictions, and (b) causal events do not "act like" material motions--particles or wave motions,

about which in any case it is hard to imagine them not being deterministic--and hence where, it seems, a concept of causality is needed which is (in terms derived from this kind of regularity and law) fundamentally probabilistic.

The idea in talking about "probabilistic causality" in terms of "capacities," is that a capacity might sometimes be exercised and sometimes not, thus accounting for fundamental indeterminism in observed events (and "tendency" and "power" can be put to this same use). I will call this the "capricious causality" interpretation of indeterminism. It is modelled after things about the behavior of persons, who may or may not activate or exercise their tendencies and capacities. Its essential claim is that there are causal agencies or factors which at times are active and at times lie latent.²⁰ One cannot predict with certainty when one or the other is going to act. Moreover, it is in the latent condition that they are the causal realities that underlie observed phenomena. It is as though to ask about the nature of their actualizing activity is not to the point. But the question, What happens to bring about...?--is not answered by the latent condition.

The present work proposes a contrasting provisional interpretation of indeterminism, one which avoids anthropomorphic concepts, and also does not have the fatal limitation as causal explanation which has been seen in tendencies and capacities. It is to say that within a context of causal

production, of transmission and interaction, there are certain attributes of terminal or interaction events, such as their exact location or timing, which have no specific causal correlate in the antecedent conditions. This implies that there is precisely no explanatory factor for these attributes present in those conditions, let alone a latent and capricious one. It implies instead causeless detail in observed events. In one kind of case, as discussed in Chapter 2, these causeless features of events would be the exact sites among atoms of material at which individual units of radiation interact. (The aggregate effects of such interactions are fully deterministic, so that an experimental set-up of a certain design will always produce a certain pattern of effect with an accumulation of these unit effects.)

There is strong intellectual resistance to the idea of a causeless event. Does it arise out of nothing? But actually I have not claimed about the cases at hand that there are causeless events. The interaction events are caused; they are the products of antecedent transmissions. What are causeless are particular facts about these events, namely (in cases I have discussed) their particular locations within detection material. (In the case of radioactive decay, the timing of events is indeterministic.)

Once the possibility of causeless features of produced events is accepted, the assigning of attributes of capriciousness to antecedent events appears to be the introduction

of a quasi-cause, when what is meant by "indeterminism" is that there is an absence of specific causes.

The other kind of motivation for talk of "tendencies" and "powers" came from cases of two causes interacting in a single observed event. Cartwright's and Harré's views on this turned out really to be the same. Both think that if two causes interact to produce an effect which is not that which would be produced by either cause acting alone, then there are not these individual causes as any kind of actual occurrence (there are at most tendencies or powers). If a causal process does not bring about just what it is supposed to bring about or normally brings about, then (the thinking goes) it is not that very causal process. But then what does the interacting in the case of the composition of two causes into a single effect?

Harré gives the cause a "modest" quasi-reality as a "tendency," but this has been shown untenable. Cartwright in How the Laws of Physics Lie does not care much for the "powers" interpretation of these cases.

Their position (partially unwitting in the case of Harré) against the actuality of the individual causes, based on the absence of their "pure" effects, is an unwarranted conclusion. Consider the example from the previous chapter. I felt a compulsion to blink from a bright flash, but I overcame it and did not blink. It certainly did have its usual effect on me, or else I would not have done anything in

overcoming it. The non-existence of a specific terminal effect does not negate the reality of the cause at all.

The thinking of Cartwright and Harré is in a sense Empiricist, in that in the last analysis it limits real events (as opposed to tendencies) either to events that can be observed, or to those unobservable events whose pure effects, unmitigated by other causes, can be observed. Apart from arguments toward skepticism about any unobservable process, there is no valid argument from the non-occurrence of a specific terminal effect in observable phenomena to the non-existence of antecedent causal events. If causal process A is a sequence of events, then the prevention of a particular consequent event by another cause B does not annul the existence of antecedent events of A, especially since both causes are adduced in this description.

Henry P. Stapp is a physicist at Lawrence Berkeley Laboratory and a well-known figure among those physicists who pursue explanations in the "quantum" domain outside the mainstream Copenhagenist positions. In personal correspondence, he has criticized my attacks on tendency ontology. Mr. Stapp feels that "tendency" appropriately characterizes cases of purely statistical regularity. It captures the fact that observed effects are not purely "helter-skelter," that there is regularity. At the same time it is fitting where there is not deterministic causation. A specific effect will not necessarily occur given its antecedent conditions, fully

specified, but it will have a tendency to occur, a tendency which is given an exact quantitative value by a probabilistic equation. He gives this example:

Consider a device that can be described...as a particle gun (accelerator): it periodically ejects a "particle." The gun is aimed at a set of three detectors, and quantum theory predicts that the probabilities for detection are, say, $1/10$, $2/10$, and $3/10$ in the three detectors, respectively. In this case the "particle" is conceived as a "tendency" for the various alternative possible detection events. One does not say [as I have--D. A.] that the tendency is activated. The tendency is present and it either produces an actual event [in its assigned detector] or it does not produce an actual event. The law describes the tendency for an actual event to occur: it is the essence of indeterminism that there can be tendencies for an event to occur without the event actually occurring. If every tendency actually produced the unique corresponding event then it would not be a "tendency" but rather a deterministic cause.

This is a very instructive presentation of how the idea of "tendencies" takes hold in the minds of physicists, from whom it is adopted by philosophers of science. It is strikingly close to what Harre' says in a passage quoted above: "laws of nature do not describe sequences of actual events, but rather the tendencies that produce them."

There are two important things to notice about Stapp's presentation of his thinking.

(1) He says that "the tendency is present and it either produces an actual event or it does not produce an actual event." Though the tendency is "present," something (as it were) embedded in the experimental circumstance, it is not,

or is not quite, an actual event, and hence is radically set apart from being an observed event. Here again actual events seem to be equated with observed or observable events. However, the insistence on a tendency is an insistence on something existing and present which is inherently antecedent to a measurement and hence cannot be observed. Stapp is not skeptical about the existence of something on grounds of unobservability. But what the existing thing is that is not an actual event (or an object), is hard to conceive.

It might be best to think of Stapp's "tendency," not fully occurrent but also not pure, non-actual potentiality, as something quasi-real. In earlier correspondence he speaks of the events preceding localizing interactions as "more tentative (or more like propensity) than the terminal event, which seems more actual" (my emphasis). This shows the origin of the tendency idea in physicist's experience, physicists such as the original quantum theorists with whom the idea originated. They are faced with an object of investigation (radiation, or "particles") which does not have the combinations of measurable attributes that an ordinary object has. There is not a bit of matter in motion with a definite course toward a terminal site. In the words of Maurice Merleau-Ponty, the causal event is apparently not "constrained to a unique and fixed location, to an absolute density of being."²¹

Such an object of study is, in short, outside the scien-

tific ontology in which fully localized objects and spatial localities are fundamental. Hence the suitability of thinking of it as somehow not quite fully real. The thought is that prior to the observable and hence fully real interaction is the potential for, or the becoming of, this determinate reality. It should be noted that this view of the matter does not involve an epistemological prohibition against claims to knowledge beyond the observable. It rather asserts the existence of quasi-actual or fuzzily actual things. Neils Bohr famously held the view that there is no reality to be comprehended in the "quantum" experiment apart from what is "described" by the mathematical models. But again, it is the encounter with something outside the working ontology--or should it rather be said, on its outer fringes--that leads to these conclusions, and not a general philosophical Empiricism.

(2) The idea of a tendency is directly tied to the mathematics of probability. The "particle" is a "set of tendencies," one for each of the quantitative probabilities for an event in each of the detectors. "Tendency" is a direct interpretation of the meaning of a probabilistic equation, whose solution in a given case is a quantity of probability. The equation (law) "describes" the tendency, something waiting in the wings of actuality, its likelihood of causal action subject to statistical calculation--thus the tendency is signified and identified by a quantity.

"Tendency" is a usefully appropriate word for situations with uncertain outcomes which have a certain degree of predictability: "My car has a tendency to stall in intersections." Hence it may serve as a convenient linguistic response to the encounter with statistical effects in experiments. But the purposes served by these linguistic adoptions of physicists should be sharply distinguished from the aim of productionistic explanation in this area. This aim, as I see it, is that of producing accurate narrative understandings of natural occurrences such as radiant propagation and its production of characteristic interactions with matter. "Tendency," where this refers to a state of affairs antecedent to an interaction, is not something that can serve in this descriptive project, because, as stated earlier, it is not itself a causal event; it does not have the form "such-and-such happens."

The important distinction is between the need for a ready-to-hand utility of language and a concern simply for a genuine explanation. The goal of the latter should not be an apt characterization of the observed facts--statistical regularity--but a description of a causal context for indeterminism, one which makes its existence fully coherent by giving it a place in our understanding of natural processes.

Mr. Stapp has since indicated that he considers the concept "tendency" of transitory value and not a permanent success at scientific description of nature.

The Success of Critical Realism

The focus of realism/antirealism is usually the established "particles" of physics, which are varieties of radiant transmission which connect "preparations," emitting devices, with detection or measurement devices. These are apparently the subjects of the contemporary antirealist view of Bas Van Fraassen. My aim in considering this important antirealist view is to show, using it as a foil, how Cartwright and Hacking support the reality of the unobservable by calling attention to the thoroughly causal context of experimental activity.

The following passage shows how Van Fraassen's antirealist arguments are framed in terms of what can and cannot be seen and in terms of particles (taking this technical term of physics uncritically as though it referred to material objects.) He is discussing the tiny luminous streaks which can be seen in a device called a Wilson cloud chamber, a kind of
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detection device for radiation.

The theory says that if a charged particle traverses a chamber filled with saturated vapour, some atoms in the neighborhood of its path are ionized. If this vapour is decompressed, and hence becomes supersaturated, it condenses in droplets on the ions, thus marking the path of the particle. The resulting silver-grey line is similar...to the vapour trail left in the sky when a jet passes. Suppose I point to such a trail and say: 'Look, there is a jet'; might you not say: 'I see the vapour trail, but where is the jet?' Then I would answer, 'Look just a bit ahead of the trail... there! Do you see it?' Now, in the case of the cloud chamber this response is not possible. So

while the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the article's being observed.

That something is in the nature of the case unobservable means, for Van Fraassen, that its reality cannot be established as a matter of scientific discovery, a Positivistic view.

The thrust of Cartwright's and Hacking's critiques of the observability criterion for reality is that they respond to its claim from the standpoint of the activities of observation and experimentation, prior to any epistemological reflection.

Ian Hacking repositions our standpoint on examples from physics, as against Van Fraassen. He asks us to take the standpoint of the experimental physicist, not merely an observer of phenomena but having himself a role in the a pro-²⁴
duction of effects to be observed.

Hacking supplies an example from his own experience in which as an experimental researcher he is trying to detect a certain entity which the mathematical models predict to exist. The procedure is to make use of a device which emits a certain kind of known particle. By aiming the emission at an object made of a certain material, he hopes to produce effects which would indicate the presence of the hypothetical entity. Given this experimental activity, if someone now says, about that which is emitted by his instrument and which

is thus itself a tool of research, "It may be nothing at all," this should bring on a question whether this remark arose from a mentally sound condition. Hacking's slogan, "If I can spray them [particles], they are real," despite the misleadingly picturesque image suggesting a shotgun barrage, contrasts strongly with the style of Van Fraassen's reflections about a conjectured and theoretical causal object, where scepticism seemed an acceptable and even likely response.

Hacking's experimentalist standpoint on antirealism is much like the case I made use of in Chapter 2, of a carpenter wielding a hammer. It is about as cogent in either case to claim that the activity is not in all of its stages a case of causal production. An experimenter who has set up in his emitter just the species of radiation he needs, and directs a beam of it at an object to look for specific effects, can only insist, in the face of antirealism about unobservables, that he is putting to use a real causal process.

Of course, the antirealist is likely to say about all this, "What stuff emitted by the instrument? To talk about particles being sprayed is to prejudge the whole issue." But the real force of Hacking's argument comes from placing the question of realism in the light of the relevant real life context, instead of supplying, as in Van Fraassen's text, only the simple framework of an observation plus a causal conjecture. I would add to Hacking's point, in keeping with

literally true." The suggestion in the word "unobservable" which Van Fraassen exploits, that of a subvisible object, might be counteracted by using the substitute term "pre-observational." This says that only at special termini do such events interact with conditions requisite for observations, rather than suggesting that they are hidden beyond the range of microscopes.

Cartwright in her anti-antirealist arguments does not go so far as to take the standpoint of the experimental producer of effects. She simply calls upon the prereflective realism that the scientist has about experimental observations, and calls attention, for instance, to some experiences with the designing of lasers. About the the cloud chamber example²⁷ treated by Van Fraassen, she says this:

In explaining the track by the particle, I am saying that the particle causes the track, and that explanation...has no sense unless one is asserting that the particle in motion brings about, causes, makes, produces, that very track.

²⁸
And: "...if there are no electrons in the cloud chamber, I do not know why the tracks are there."

Her claim is simply that for specific observed events under closely specified conditions, there are the characteristic causes named by the term "electron" bringing them about. Her language of "particle in motion" might suggest that she pictures this cause as a subvisible object, as does Van Fraassen. But her claim does not require this picture.

A causal realism need only involve the claim that some specific kind of process, agency, or entity, named "electron" by physics, produces the track. It is identified by its exact context: it makes this kind of track in this kind of medium and has this kind of preparation or emitter as its source. There is no need to picture the individual electron in any particular way whatever, and this holds not only for a causal realist argument but also for science exploring the causal possibilities of the electron and making use of them.

The significant difference in Cartwright's treatment of the cloud chamber example over Van Fraassen's is that Cartwright keeps in view the real context of technological activity, a context of developing knowledge involving cloud chambers and other equipment. As part of this scientific legacy there is a linguistic tradition of designating, in a technical language, causal agencies and sequences of events presumed to lie behind the observations.

Van Fraassen, on the other hand, takes this linguistic tradition designating causal events to be a product of the scientific imagination, a concocted world of the inherently imperceptible and hence unknowable. He ignores the historical experience which the various forms of invisible process--light waves, electricity, "particles"--came to be identified.

He presents his example as though scientific observations occur without context. In the case of the cloud chamber, one makes an observation out of the blue, of a peculiar

track within it which looks like a jet trail. About this distinctive datum, he thinks, one might make an ad hoc supposition, as a convenient explanation, that something moved through the chamber and made the track. It sure looks as though something did. And the account of the matter by physicists, "theory," certainly suggests this. But physics also says that it is physically impossible ever to see it. What gives us the warrant, then, to take this causal account--something moving through the chamber--as more than a convenient fiction, a parable metaphorizing the track, which perhaps can be used to communicate the character of the occurrence to others: "It looks just like..."

"How do I know that this particle, this individual causal agency, exists?" thinks Van Fraassen, "I am not as certain of it as I am that just now my eye fell upon one of those little streaks. The particle must always remain a fable that physics tells about the streak."

In the theoretical account which Van Fraassen relates, which sounds all the way through like a causal story, there is more than the existence of a moving something. There are the processes of ionization and condensation of which he informs us. But let's focus for a moment on the claim that antirealism is most interested in, namely that (as the skeptical view might put it) an imaginary object of science lies hidden in the occurrence, or (more cautiously) that there is a "moving charge" which, whatever it is, propagates indepen-

dently of the events it brings about. Is there any such entity which, arriving into and incorporating itself into the experiment, passes through and leaves a trace within the chamber?

What Van Fraassen recounts as the theoretical story is a technical account, not a description of events in ordinary language. Certainly we do not know what "electron" or "particle" designate as matters of ordinary understanding. But Van Fraassen writes as if it is completely clear what it is we are to be skeptical about: an object, like a meteorite entering the atmosphere. His "unobservable in principle" implicitly takes the term "particle" as if it did mean what it does in ordinary language. "In principle" means for him that there is no physical means by which one could see the electron. But in fact, this has nothing to do with the physical limits of microscope technology. The very notion of seeing the electron in motion, as one sees a jet passing overhead, makes no more sense than the idea of seeing light waves.

So what he is saying amounts to this: Physics tells a story about an infinitesimal object, but no one can securely claim to know it exists. The trouble with this is that if one looks closely at physics, one finds that the impression that the electron is a tiny object is a superficial interpretation of the talk of physicists, and in fact must simply be wrong. For one thing, as an independent causal occurrence

it is a form of radiation with a wavelike characteristic. One should take this finding seriously.

Suppose one were to say to Van Fraassen, "Nancy Cartwright is convincing when she argues that something brings about these tracks, leaving aside any picture of the cause as a moving particle. Would you not be content to say that we do not know, perhaps cannot know, what it is, rather than saying that whatever it is, we do not know that it exists?"

"It is the same with any hidden cause, in my view."

"Your argument, then, has nothing to do with the seemingly intrinsic obscurity of the electron's nature. You make no use of the fact that as a "quantum" entity, its complex effects seem to put it beyond the reach of any ordinary understanding."

"That is correct. However it is understood or not understood, it belongs to a causal hypothesis constructed by science. I am not arguing for the abrogation of a causal understanding of subatomic interactions, as in Copenhagenist physics."

"I'm glad that is clear. And ionization also is an unobservable change in an atom, so that your skepticism must apply to this occurrence as well, and not only to the existence of what is called a moving charge."

"Yes."

"So your full-fledged claim is as follows: For all we really know, it may be that nothing (as opposed to something

inaccurately or incompletely understood) brings about ionization; it may be that nothing brings about condensation of droplets; and ditto for anything which as you say results in the silver-grey line which we see, but which cannot itself be seen."

Van Fraassen says that the success of science means only that theories are empirically adequate, not that they are true. The intent of this is the usual one of Empiricism, to argue that one cannot infer that an explanation is correct when other explanations exist or may exist which explain equally well, unless our favored explanation can be independently confirmed (which requires an observation). He uses the word "theory" to suggest that the target of his skepticism is some one clear, definite hypothesis about observations--something that could be set beside other, differing hypotheses. He leads the reader to think the case is much like the discussion, currently unsettlable, over the wide-ranging possible causes for some extraordinary features on the satellite Miranda of Uranus, features revealed by the Voyager spacecraft,²⁸ except that in the cloud chamber case one theory is settled upon for one reason such as simplicity, which is a reason which falls short of establishing the correctness of the explanation.

But as we have seen, what there really is for Van Fraassen to mean by "theory"--the identification of the electron--is not some one, distinct causal hypothesis, as opposed to

some other, about the cloud chamber track, but the working language and understanding, both background and immediate, of physicists, which is through and through causal. Cartwright simply insists upon this working, pre-epistemological realism of physicists. To say as Van Fraassen does that the electron is part of the "scientific image" of reality, instead of part of reality, is to tacitly read the physicist's term "particle" as if it were the ordinary use of this term in a concrete description. Causal realism about the case is in no way tied to the image of a particle, and in fact, as Hacking points out, no image of the electron exists which is any more than a useful model. The whole implication of a subvisible moving object disappears upon close scrutiny of physics.

The realist arguments of Hacking and Cartwright are remedies for thinking of physics as a mythology of objects too small to be seen, postulated to explain particular observation-data. When physics is seen instead as a real life activity of technical discovery and invention, the entities which it designates through the various particle names are recognized as context-specified species of causality which are investigated and put to use. Apart from their differentiation and technical exploitation, these causal entities operate incognito. They have no description in existing science.

While Cartwright tries out the dubious ontology of "capacities," Hacking is in the tradition of pragmatic philoso-

phy of physics in eschewing ontology. The upshot, then, of critical realism's success against antirealism is this: the idea of clear and unverifiable hypotheses about objects behind the scenes is replaced by the idea of perhaps permanently obscure real causes.

Causality and Forces

Setting aside all traces of Empiricist doctrine, either about the limitation of knowledge to the observable or about the primitive status of narrative natural science: Is a philosopher of Nature warranted in expecting that some real and perhaps comprehensible causal occurrence outside observation is designated by the terms "force" and "field"? If so, how would inquiry about the force or field itself be related to knowledge of the relevant law or laws?

One could observe that two objects of a certain type attract each other when they are placed in proximity, without having a clear idea at all about how the exact quantity of force might depend on the exact conditions. One would nevertheless say that a force is exerted, or that something exerts a force, on these occasions. Now suppose one diligently determines the law yielding the quantity of force as a function of given conditions. How would this affect what one is able to say about the force?

There would certainly be no guarantee in this determination that one had any more of an explanation of why the force

occurs or what produces it, though there may well be clues in this quantitative relationship. But it would just as certainly have no negative effect on one's ability to say that a force is exerted here or that something exerts a force. Primarily one would now have at hand a procedure for determining the exact quantity of force when other conditions are known.

Now suppose a long tradition has developed of making use of this law, as a component of a more inclusive mathematical theory, for making predictions and designing technologies. The force is cited to explain certain facts, and the law has become the main focus of scientific concern with the force. When the force is discussed in science books and science classes, the law is cited and explained. The things to learn about this force are an equation, how it was arrived at, and how it is applied. Even bright students may be unlikely to raise the question, "But what is this force? What causes it?" They would be likely to fear the mockery of their peers for raising such a question.

But a particularly thoughtful and independent student might raise this question. Indeed, there is no reason that the naive question about what sort of natural process could be at work in this force, though it may get lost in the constructive technology of algorithms, should be any less viable than it was at the time of the crude observation of a force of attraction. The success of mathematical treatment

is only the snowstorm of knowledge-activity in which the question gets lost.

But the question may meet another kind of obstacle. Suppose our force is the force of gravity. What sort of thing could possibly answer the question about its cause? One expects an inquiry about it to have the form Cartwright
30
describes:

...causes make their effects happen. We begin with a phenomenon which, relative to our other general beliefs, we think would not happen unless something peculiar brought it about...An effect needs something to bring it about, and the peculiar features of the effect depend on the peculiar features of the cause, so that--in so far (sic) as we think we have got it right--we are entitled to infer the character of the cause from the character of the effect.

What mechanism, even ignoring the requirement that it be completely unobservable, could have the characteristics to draw together two object which are in proximity? To say that something reaches out from one object and pulls the other toward it is only a metaphoric elaboration on the notion of a "force," and only transforms the question into, What reaches out? The situation is a more fundamental puzzlement than that in such a case as, "I can't seem to find the cause of your car not working, and I'm not getting any ideas." In fact, even though there is no definite reason to say that gravity has no unobservable cause (indeed, physicists now talk about it as a form of radiation) it is not clear just what the question about the cause or mechanism of gravity is

asking after, when there is nothing to go on except the observations and a law of functional dependence. The question is not well-oriented. We seem to lack what Cartwright calls a "background knowledge" for the question to have bearings. Our first response to the question may well be to throw up our hands univocally with Newton's hypotheses non fingo.

31

Harre' says this:

At the end of any explanatory regress we must perforce shift from causal mechanisms to causal powers...Gravity may...be a referent of last resort, explanatorily. To explain the behaviour of falling bodies by reference to gravitational potential may be to cite a basic causal power.

How about by reference to a gravitational field? His answer is no doubt the same. The last stand in the explanation of forces is a power or "disposition." But a point I have been making is that a power by itself does not explain anything, and this is true whether it is attributed to the objects responding to forces, or to intervening events, or is not given any specific subject. If explanation in such cases stops at a power, then basic phenomena are permanently unexplained.

There is one thing that can be said very roughly to be required of any genuine answer to the question about the cause of gravity. It is captured in the notion in physics of a field. This term is normally taken--and for most purposes perfectly adequately--to designate a vaguely understood phys-

ical entity signified by an equation or set of equations which are in use. It is today most unfamiliar to presume that one might ask, "What is it, apart from well-known patterns of quantitative effect? How can it be described as a physical occurrence, active prior to any measurement, and independently of observable consequences?" But it is just the approach of the present work to take such terms as "field" to be terms for physical actualities, and to venture the beginnings of a story about Nature in which they will be fully and concretely comprehended. Here is one such amorphous beginning: "Field" means that objects are embedded in an environment (in some sense) of activity (of some kind) through which they interact with one another in different ways, and this causal activity is structured with the object as its center or focus.

Though in textbook and practical science there is not to be found the "background knowledge" with which to ask after, in a well-oriented way, "characteristic causes" for forces of attraction, our intuitions--which find no adversary in what is known from physics--tell us that there exists natural activity which could be described by way of such causal explanations were this background to be supplied. My view that forces are actual and perhaps describable independently of the material objects that they observably affect is a view held by major scientists of the past, notably Newton and Michael Faraday.

Conclusion

From the dispute over composition of causes, it was seen that when a single observed event is the result of two interacting causes, this situation contains no warrant for saying that either cause is unreal or is inactive as a latent power or tendency. And it was made clear that in any case, for a causal explanation inactive potentialities cannot substitute for actual causal events.

The critical realists supplied the insight that a specific unobservable cause does not have to be a "theoretical entity," identified only as a permanently hypothetical explanation. All of this speaks against the ubiquitous idea that because the processes which are adduced as causes for observed phenomena go on in empty space or in the "subatomic realm" and are therefore not the sort of thing one could watch happening--in short, they are not object-motions or changes in objects--this is some reason to doubt their actuality. Causal composition of physical forces, for instance, only makes sense if some depth of antecedence is ascribed to the forces as occurrent causal activity. Such forces bring about motions (if not prevented from doing so); they are not identical with these motions.

What Harre¹ thinks in claiming to concur with Cartwright that laws do not describe sequences of actual events, represents an interesting confusion. It is true that a law of nature does not describe a sequence of events intervening

between correlated observations. It is not a causal narrative. Harré's statement thus goes down easily. However, the implication in his thinking (perhaps unwitting) is that when a law quantifies a force as a function of other measured values, no real causal occurrence corresponds to this designated "force" (it is only a "tendency"). This is quite different from saying that a law does not tell a story about this causal occurrence, that is, does not say just what it is. In this causal/descriptive sense, we are far from knowing what the basic forces of Nature actually are. This does not mean that the law of gravity does not identify by its variable "force" an actual occurrence apart from a consequent motion.

What I am proposing, against the grain of recent philosophy of science both realist and antirealist, is an extension of an old-fashioned realism about the natural domain of physics, an attitude which was alive in what Frank calls the "mechanistic" era, in which certain unobservable transmissions known about at the time were assumed to be potentially understandable in detail. This "realist" scientific tradition, I claim, can be extended with no definite limit into the domain of contemporary physics, in which unobservable transmission (radiation) and forces are not entities in separate categories, but species of pre-observational causal process.

The primary problem faced by the project of a produc-

tionist understanding of these causal processes is outlined in the final chapter. Chapter 4 examines an effort by a highly regarded philosopher of science to claim reality for unobservable transmission and to contribute to a general understanding of it through the analysis and application of causal concepts.

Notes

1. Nancy Cartwright, How the Laws of Physics Lie (Oxford: Clarendon Press, 1983).
2. Ian Hacking, Representing and Intervening (Evanston: Cambridge U P, 1983).
3. Cartwright, 128.
4. Cartwright, 60.
5. Cartwright, 59.
6. Rom Harré, Varieties of Realism: A Rationale for the Natural Sciences (Cambridge: Basil Blackwell, 1986) 287.
7. Lewis Creary, "Causal Explanation and the Reality of Natural Component Forces," Pacific Philosophical Quarterly 62 (1981) 153.
8. Cartwright, 61.
9. Cartwright, 61.
10. Cartwright, 62.
11. Cartwright, 67.
12. Cartwright, 66-67.
13. Harré, 291.
14. Werner Heisenberg, Physics and Philosophy (New York: Harper and Row, 1958) Chapter III; and Henry Stapp, "Transcending Newton's Legacy," invited talk at "Newton's Legacy: A Symposium on the Origins and Influence of Newtonian Science," Tulane University, November, 1987.
16. Harré, 282.
16. Harré, 291.
17. Harré, 291.
18. John Dupré and Nancy Cartwright, "Probability and Causality: Why Hume and Indeterminism Don't Mix," Nous 22 (1988), 521-536.

19. Rom Harré, "Modes of Explanation," Contemporary Science and Natural Explanation: Commonsense Conceptions of Causality, ed. Denis J. Hilton (New York: New York U P, 1988) 142.

20. An interpretation of indeterminism can also be found behind the use of the terms "tendency" and "potentia" by physicists. An additional motivation behind their use of these terms is that, though properties such as position and momentum could be measured, these properties do not appear to preexist the measurement events, so that there is some reason to talk about these properties potential in what is measured.

21. Maurice Merleau-Ponty, In Praise of Philosophy and Other Essays, (Evanston: Northwestern U P, 1963) 154.

22. Bas Van Fraassen, The Scientific Image (Oxford: Clarendon P, 1980) 17.

23. Ian Hacking, Representing and Intervening, Cambridge University Press, New York, 1983, p. 272.

24. Hacking, 27.

25. Hacking, 262-266.

26. Cartwright, 92.

27. Cartwright, 99.

28. Stephen P. Maran, "The Mysteries of Miranda," Natural History 97 (1988): 74.

29. Cartwright, 76.

CHAPTER 4

WESLEY SALMON ON CAUSAL PROCESSES

Wesley Salmon published a major work in 1984 entitled Scientific Explanation and the Causal Structure of the World¹. It has two major parts: (1) a theory of explanation in the tradition of logico-empiricism, and (2) a general theory covering two basic types of causal process with a direct bearing on physics--transmission and interaction.

The theory of explanation purports to improve on the deductive theory of Hempel and Oppenheim. According to their "standard" doctrine (Chapter 1), an explanation is the deduction of an event from a general law. Salmon's purported improvement on this model is to provide a place for probabilistic laws, such as those by which modern physics responds to the encounter with indeterministic processes, which could not yield certain explananda events deductively. I will not attempt to determine whether this project, within the locico-empiricist designs, is successful.

From the standpoint of the present work, the importance of Salmon is that he straddles, or tries to straddle, the acausalist and productionist positions. On the one hand, he believes the world has a "causal structure," and this means he thinks that in physics the gaps, so to speak, in sequences

of events subject to observation are filled in by intervening, continuous transmissions. On the other hand, he strives to further and perfect the traditional acausalist project of a law-based model of explanation, on the Humean assumption that scientific knowledge, including knowledge of causal connections if there are any such things, is built up through the exploratory disclosure and formalization of regular associations in observation, and he formulates the logic leading from this accumulated experience to a "statistical" (as opposed to a deductive) structure of explanation.

Within the present concern the relevant part of this combined project is Salmon's constructive realism: how he proposes to talk about pre-observational processes in such a way as to understand them as continuous processes. I will be showing that, true to the tradition of Hume in which Salmon explicitly places himself, but contrary to his avowed aim, his is an account of causality which unwittingly leaves out the relation of production between events. To leave out this relation, in the final analysis, has the usual result in the Empiricist tradition, of reducing knowledge of the domain of physics to quantitative knowledge of regularity in observed and discrete events, which is to exile pre-observational causal activity from explanations.

Salmon, then, wishes to combine two views about explanation: first, his "statistical" model in the tradition of the law-based model; and second, what he calls the "ontic concep-

tion" of explanation, according to which to explain a phenomenon, in a sense to be upheld as legitimate and important, is to talk about natural entities, goings-on in nature, rather than, say, laws and their logical force as conditional propositions.

The three sections in this chapter show, respectively, (a) that Salmon's attempt to affirm the reality of unobservable transmission does not succeed, and in its failure provides a stark illustration of the reduction of event relations to contiguity; (b) how Salmon exemplifies the traditional view, contrary to that of the present work, that theories of physics explain and that the role of philosophy of science is to analyze the meaning that "explanation" must have in these cases; and (c) how Salmon's attempt to think about "probabilistic" process ends in a "propensity" analysis, whose basic failing from a productionist point of view was made clear in the previous chapter.

The Theory of Transmission

A theory of causality with application to physics must take into account both transmission through space and the interactions between transmissions and matter. Salmon's attempt to explicate the causal processes of transmission and interaction and their relation rests on some purported categorical distinctions. One of these grounding distinctions he attempts to establish is that between propagation (or trans-

mission) and production (which he would apply exclusively to interaction). The distinction appears to be taken quite sharply, as if propagation could not be production and vice versa. The other distinction is made between "process" and "event," which he would apply exclusively to transmission or to interaction, respectively.

What he is trying to achieve with these special distinctions and usages is to form a conception of transmission as continuous process intervening between separate interactions. For Salmon (and it will be seen why this is) only a "process" can unproblematically be said to be continuous, while succession in "events" cannot. The propagation/production distinction apparently serves to reinforce the difference on this basis between transmission and interaction.

In the following passage he tries to use the propagation/production distinction to introduce this difference in forms of causality.²

There are, I believe, two fundamental causal concepts that need to be explicated, and if that can be achieved, we will be in a position to deal with the problems of causality in general. The two basic concepts are propagation and production, and both are familiar to common sense...When we say that the blow of a hammer drives a nail, we mean that the impact produces penetration of the nail into the wood. When we say that a horse pulls a cart, we mean that the force exerted by the horse produces motion in the cart. When we say that lightning ignites a forest, we mean that the electrical discharge produces a fire. When we say that a person's embarrassment was due to a thoughtless remark, we mean that an inappropriate comment produced psychological discomfort. Such examples of causal production occur frequently in everyday

contexts.

After thus identifying cases of "production" which he will later call "interaction," he then turns to cases of transmission.

Causal propagation (or transmission) is equally familiar. Experiences that we had earlier in our lives affect our current behavior. By means of memory, the influence of these past events is transmitted to the present (see Rosen, 1975). A sonic boom makes us aware of the passage of a jet airplane overhead; a disturbance in the air is propagated from the upper atmosphere to our location on the ground. Signals transmitted from a broadcasting station are received by the radio in our home. News or music reaches us because electromagnetic waves are propagated from the transmitter to the receiver. In 1775, some Massachusetts farmers--in initiating the American revolutionary war--"fired the shot heard 'round the world" (Emerson, 1836). As all of these examples show, what happens at one place and time can have significant influence upon what happens at other places and times. This is possible because causal influence can be propagated through time and space. Although causal production and causal propagation are intimately related to one another, we should, I believe, resist any temptation to reduce one to the other.

There are some nice examples here, leaving aside that the assertion about memory as transmission is quite problematic. In these two paragraphs Salmon provides some cases which can be described as causal production and causal propagation. But does his sorting-out of these examples really mark out these two as separate categories of occurrence, as he suggests? Not only are they "intimately related"; it would be hard to think of two words that come closer to

meaning the same thing, or at least, that are so generally overlapping in their examples. Take the cases of water surface waves and sound waves. Why are they commonly called propagation? Because antecedent stages of the process give rise to, produce, successive stages. That is how a ripple intrudes upon and activates waters previously still. It produces a disturbance on the advance. A wave is productive of itself; it is reproduction or propagation.

Or take one of Salmon's examples: electromagnetic waves, also commonly termed propagation. Today such "waves" are no longer thought of as vibration in material which is everywhere present. Still, their effects show frequency and phases, in some sense successive reiteration, despite the fate of the waves-in-the-ether conception. There is no reason at all to suppose, just because these are not oscillations in and of material, that this successive reiteration is not any kind of productive cycle, so that the use here of both the term "propagation" and the term "production," just as they can be applied to ordinary waves, would be inaccurate or inappropriate. They are still called "waves," and according to some basic experiments must in fact be wavelike.

If one wanted to differentiate the usages of "propagation" and "production," one might say something like this: Propagation is at least a kind of production which is reiterated through successive events in space and time (without treating the question of whether the events need all be of

the same type). A case of production which contrasted with ongoing propagation would then be one that terminated in an effect, which then produced nothing further (or nothing relevant to the description). I will now study as a possible case of such a terminal effect a favorite example of Salmon's.³

In a darkened Astrodome a spotlight shines on the ceiling. The beam of light (transmission) produces (at least as we would ordinarily describe it) the spot (interaction). Now set the spotlight turning: the spot moves around. This process of moving around does not produce any further effect apart from itself, as does the light transmission process, which produces a spot. Because of this Salmon wants to call this movement of the spot a "pseudo-process."⁴ This seems inappropriate. There is no reason not to call it a real process.

"The beam is now turning figure eights on the ceiling."

"Not really; it is just a pseudo-process."

I will not use this expression, but will take Salmon's intent to be that the movement of the spot is not a causal process, but a terminal effect. This seems acceptable; whether the spot moves or does not move, or in what manner it moves if it does, might make no difference to the Astrodome, or to the light beam, or to the spectator. Spot movement seems not to affect anything else.

Salmon defines the difference between causal and non-

causal or "pseudo" processes in general as follows. If one inserts a red filter into the beam, it will alter the beam, and the beam will pass on this alteration to the spot on the dome, so that it becomes red. Salmon calls this "mark transmission." The beam of light is altered "structurally," and this new structure is transmitted to the wall. But, he argues, the movement of the spot is not capable of transmitting a mark, since if one holds the filter up and rotates the beam through it and on past, the spot will momentarily change to red, but this change will not be passed on to successive positions of the spot. Since it cannot transmit such a change, it is a "pseudo-process."

But since the light transmission and the movement of the spot are quite different kinds of process, why should it be assumed that the very thing that leaves a mark on one kind through portions of the process successive to the marking, should similarly leave a mark on the other? To be free of this assumption, we should ask whether there is any way in which the spot movement process could transmit--pass on into further effects--some kind of modification of itself.

Suppose there is a computerized device in the Astrodome which is equipped with a light receptor. It is designed to read patterns of light movement reflected from the ceiling and translate them into outputs specific to those patterns. Different ways in which the spotlight is moved--ways of "marking" the spot movement process--produce different re-

sponses from the computer. This would be a kind of "mark transmission" appropriate to this kind of process, and within its capacity.

For that matter, what about a certain cycle of movement of the spot leaving in our eye a retinal image of a figure eight? Spot movement is marked by the way the spotlight is moved, and it in turn marks our eye.

Another one of Salmon's examples of a "pseudo-process" is a moving shadow.⁵ But imagine a situation right out of the movies, in which the shadow of a feared assailant betrays the fact that he is creeping around the door with a gun in his hand (by holding a gun in the air, he "marks" the shadow in an unmistakable way). Here a moving shadow has quite an impact on the possible victim. The shadow of an airplane passing overhead might startle us, and a passing cloud will produce definite effects in light meters and solar cells. A shadow moving is a physical, causal occurrence. The truth in what Salmon says is that this is not a case of propagation, since stages of the motion do not produce succeeding stages.

It is thus possible to view certain occurrences as termini of processes, without considering any further events to which they themselves give rise, and yet these same product-occurrences in the cases examined can play the role of transmission to further effects. Thus Salmon's "pseudo-processes" are not a category separate from causal processes, as he supposes. And from our discussion it looks even less

likely that there could be a productive process which is in its very nature not propagative, or vice versa. It does not therefore appear likely that the propagation/production distinction as Salmon conceives it can serve to explicate transmission and interaction as different kinds or aspects of causality.

Transmission and interaction do get clearly distinguished in a characteristically Empiricist conception which Salmon appropriates from Russell and Reichenbach and which is called the "at-at" theory. According to this theory there are some separate events--interactions--and there is their occurrence in succession, which is the whole meaning of "transmission." For example, there is an interaction at the site of a red filter inserted into the spotlight beam, and there is an interaction which is the spot on the dome, and their succession in time and space constitutes transmission.

Within the standpoint of the present work, this should be recognized as an acausality about transmissions as independent, connecting processes. It does support Salmon's propagation/production distinction: only the interactions are cases of production, and propagation just means they happen now at one spot--now at another. But an aproductive--i.e., only "propagative" according to Salmon's distinction--process cannot produce itself (really propagate), and it also cannot, as aproductive, do anything by way of what should be called its role in an interaction. Thus on this view there

can be no causal action of transmission producing an interaction event; there can only be an unproduced, hence causally unexplained, "interaction" event. This is the same symptom that Cartwright and Harre had in the discussion of composition of causes: excluding from the account (in one way or another) the independent activity of the individual, unobserved contributing causes.

Interestingly, Salmon sees that, especially from the point of view of physics and the history of experimentation, there is in the spotlight example "continuous intervening process" termed electromagnetic propagation.⁶ He holds this view in spite of the fact that in his philosophical treatment he succumbs explicitly to an acausality (more precisely an aproductivity) about radiation--the "at-at" theory--which seems built into his avowed traditional Empiricism. "Transmission" designates no independent productive activity. What, then, allows him to speak of "continuous intervening process"? This is where his process/event distinction comes in.

He thinks he can still claim reality for the intervening transmission by a special tailoring of the term "process" as against "event." This second basic distinction is pursued in the section immediately following the above lengthy quotation, a section entitled "Processes."⁷

One of the fundamental changes that I propose in approaching causality is to take processes rather than events as basic entities. I shall not attempt

any rigorous definition of processes; rather, I shall cite some examples and make some very informal remarks. The main difference between events and processes is that events are relatively localized in space and time, while processes have much greater temporal duration, and in many cases, much greater spatial extent. In space-time diagrams, events are represented by points, while processes are represented by lines. A baseball colliding with a window would count as an event; the baseball, travelling from the bat to the window, would constitute a process. The activation of a photocell by a pulse of light would be an event; the pulse of light, travelling, perhaps from a distant star, would be a process. A sneeze is an event. The shadow of a cloud moving across the landscape is a process...Among the physically important processes are waves and material objects that persist through time. As I shall use these terms, even a material object at rest will qualify as a process.

Salmon is trying to establish, for his own special purpose, that there are cases where one of these concepts--event or process--applies but the other definitely does not, and vice versa. But it seems clear that this is not established by these examples. Why can't a baseball colliding with a window be called a process? If one were studying the shattering properties of the glass, capturing the event on film in order to slow it down and examine it, one would be likely to call it a process.

A scientist could surely give a fairly detailed story about "the activation of a photocell by a pulse of light." The word "process" would not be inappropriate for what is described.

A sneeze could be called a process if it were a subject of physiological or aerodynamic study. And a cloud shadow

suddenly moving across a meadow might be called a process under some circumstance, but as it is it makes a perfectly good event.

The way we use these words it is hard to imagine a happening, which as such takes up an interval of time, being such that one could say, "That was certainly not an event," or, "I would not call that a 'process.'" To apply one term to a particular case but exclude the other seems highly artificial.

One can gain from these examples and discussion a sense for how these two words are differentiated in their usage. It is not something about the character of the occurrence itself, but depends on what aspect of it is relevant to the purpose at hand in talking about it. If one is viewing it as something that comes to pass at a particular time and place and is gone, one will call it an event. But if one is interested in the composition of the occurrence, what parts or stages it has and how it goes through them, one will refer to it as a process.

The non-overlapping separation in usage of "process" and "event" for different kinds of occurrences, aside from these fatal objections to it, serves Salmon's purposes well. His "at-at" theory leaves transmission without any description independent of interaction; it is just succession in interactions. It is itself no sequence of events, no productive activity. By separating concepts, he can nevertheless call

it a "process"--and thus, he thinks, have it be something, as an attempt toward a constructive realism.

In pursuing the reasons for this separation of concepts one is also led directly to a historical reason for this whole peculiar view: Hume. Here is Salmon in some closing remarks to a chapter:

8

Throughout the discussion of causality, in this chapter and the preceding one, I have laid particular stress upon the role of causal processes, and I have even suggested the abandonment of the so-called event ontology. It might be asked whether it would not be possible to carry through the same analysis, within the framework of an event ontology, by considering processes as continuous series of events. I see no reason for supposing that this program could not be carried through, but I would be inclined to ask why we should bother to do so. One important source of difficulty for Hume, if I understand him, is that he tried to account for causal connections between non-contiguous [N.B.] events by interpolating intervening events. This approach seemed only to raise precisely the same questions about causal connections between events, for one had to ask how the causal influence is transmitted from one event to another along the chain. As I argued in Chapter 5, the difficulty can be circumvented if we look to processes to provide the causal connections.

The thought is also expressed in closing remarks to an earlier chapter:

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It is tempting, of course, to try to reduce causal processes to chains of events; indeed, people frequently speak of causal chains. Such talk can be seriously misleading if it is taken to mean that causal processes are composed of discrete events that are serially ordered so that any given event has an immediate successor. If, however, the continuous character of causal processes is kept clearly in mind, I would not argue that it is philosophically incorrect to regard processes as collec-

tions [N.B.] of events. At the same time, it does seem heuristically disadvantageous to do so, for this practice seems almost inevitably to lead to the puzzle...of how these events, which make up a given process, are causally related to one another. The point of the "at-at" theory, it seems to me, is to show that no such question about the causal relations among the constituents of the process need arise--for the same reason that, aside from occupying intermediate positions at the appropriate times, there is no further question about how the flying arrow [of Zeno's paradox] gets from one place to another. With the aid of the "at-at" theory, we have a complete answer to Hume's penetrating question about the nature of causal connection. For this heuristic reason, then, I consider it advisable to resist the temptation always to return to formulations in terms of events.

To speak of transmissions as serially ordered events, he thinks, tends to make us think of them as broken up into discrete parts which do not have the continuity wanted from causal connection. To try to reestablish connections by means of intervening events, also discrete, is to obtain at most contiguity, not connection. We recognize here the mosaic theory of events, which says that events must compose in the way objects compose.

Salmon has helped to elevate this Humean problem to a new height of clarity. The belief here expressed is that if a process has parts, it must be discontinuous and disconnected. This means that in order for causal connection to obtain, the "process" must be completely uniform in space and time, with no differentiation into separately identifiable stages or parts. There is no "question about how [the transmission] gets from one place to another"; this restates the

view that transmission or propagation is not to be described, independently of interactions, as productive activity. In the background, Salmon almost seems to realize that his "process" can only be an empty shell of space and time, since without any differentiation there can be no real occurrence.

The central claim Salmon is making with all these distinctions turns out to be a stark one: there is only causal connection where there is complete uniformity and featurelessness in time and space along a route separating the events thought to be connected. But obviously he does not intend to be saying that there is nothing connecting the events. There is rather a real, featureless intervening "process."

Being featureless allows it no independent description, beyond saying that it occupies, in some special sense in which a process can be said to occupy, all points of a space-time interval. It can be called an object-picture of a process (with no event-parts), and it is easy to see how it is exemplified in a "material object at rest," occupying its space and the time of its enduring existence.

Concerning the spotlight example, Salmon can make a counterfactual assertion, saying that a "mark" imprinted on the process will be passed on by it, and this tells him that it is "really there," filling the space-time region. For example, if a white card is placed in the beam of light at any point between the red filter and the wall, "we will find

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the beam red at that point."

The transmission is thought of as occupying its temporal and spatial interval as a featureless object. Thus Salmon would uphold causal reality, and also attempt something like a description or a step toward description with his idea of "process." But this success is at the cost of actually eliminating and exiling from the account something that is known about radiation. Electromagnetic transmission has frequency of cyclic parts; it is propagative reiteration. It is a process composed of a sequence of parts or events. The claim that in order to be causal it must have no differentiated parts not only ignores, but denies, the one thing that science does know about the causal "structure" of this genus of transmission, denies its known physical character.

A general remark is in order about Salmon's appropriation of Hume's analysis of causality for application to pre-observational process in physics. Hume started with an epistemology of sense impressions, which we can understand as the mental reception of the images of objects. It is one thing to say, given this starting point, that there are no productive relationships to be discovered among these object-images. It is quite another thing to say that given anything whatever that we can call events in serial order, "productive connection" must already be without meaning, or is unavoidably barred from the description.

The appropriation of Hume seems especially injudicious

when what is at issue are occurrences such as light transmission, which are not subject to observation and can precisely never be "phenomenal," can be neither cause-events nor effect-events given a sense data epistemology. To uphold the reality of these causal events though unobserved and largely undescribed, and then to argue for a skepticism about their productive connections, would be an epistemological joke. What we really cannot know is why, or indeed whether, productive connections per se would be a problem.

It seems to me that the most potent criticism of the Empiricist view of pre-observational transmission is to simply point out, as I did in Chapter 2, that it is not a conceptual impossibility that things which are differentiated from one another might also be connected in any sense of the word one would like to see applied. The example of the color spectrum serves to establish the possibility. Especially for causal occurrences whose unknown character is the very descriptive problem that must ultimately be faced, it seems a serious and perhaps disastrous mistake to be ruling out connected differentiation in advance.

Explanation as a Theme: A Mark of Basic Acausalism

In his chapter section entitled "Explanation In Quantum Mechanics," Salmon gives an illuminating exposition of the philosophical problems faced by physicists. Besides this valuable exposition there are some vague positive suggestions

about particular problems, whose evaluation would require that we go into those problems. But the overall result is that he himself does not feel he has gone far at all in the matter. In the closing words of the book he cites a central¹¹ unresolved mystery of quantum physics:

In his address, "On the Present State of Quantum Mechanics," delivered at the 1982 meeting of the Philosophy of Science Association, Howard Stein remarked, "The problem of reduction of the wave packet...remains what it has always been: baffling." If he is correct--and I feel fairly confident that he is--in claiming that we still do not understand so fundamental an aspect of quantum theory, it is hardly surprising that nothing approaching an adequate theory of quantum mechanical explanation has yet been offered. In any case, to provide a satisfactory treatment of microphysical explanation constitutes a premier challenge to contemporary philosophy of science--one that lies beyond the scope of this book and beyond my present capabilities.

I want to make note here of how Salmon's language in this passage situates him squarely in traditional philosophy of science, and contrasts with the way I would speak of this same problem of understanding, namely as a problem of how to causally explain a phenomenon. He sees the problem as one of a "theory of quantum mechanical explanation" and a "treatment of microphysical explanation." Thus he represents the philosophical need here as something like understanding a case of explanation which already exists in and as a certain achievement of science. He is continuing the traditional issue or theme of explanation, in which theories of scientific explanation are propounded, and for which the Hempel and Oppenheim

paper is monumental. This tradition begins with the flat presumption, "Science explains," then asks the question, in view of contemporary acausal physics taken as a paradigm, "How does it do so?"

As I have asserted throughout this work, my view does not go along with this thematizing of explanation. I claim that science, in general, explains, and it does so with accounts of causal activity in Nature, except for atomic physics, which is a technological achievement in interaction with a domain of Nature, and is not in the main a science which explains.

The established results of the researches of quantum physics do not include what I would call explanations, but they do include are some quite "baffling" facts. The business of explaining these facts has been a peripheral aspect of the physics community: a small and diverse group of "quantum" ontologists, working outside the mainstream and in a purely explanatory exertion, so far without effect on practical research. Their specialty is that of quantum measurement theory. This field arose and is sustained out of dissatisfaction with the Copenhagenism associated with Neils Bohr, which is the most conspicuous basis in physics itself for the neo-Humean view of science. In other words, this field exists because the baffling character of the quantum measurement event, mentioned by Stein, is unaffected by the successes of quantum mechanics.

The absurdity of the position that the baffling result of physics that Stein mentions is a case of explanation which philosophers have the task of somehow explicating highlights another objection to Empiricist philosophy of science. Its logic would allow only Copenhagenism, which renounces description of the unobservable, as legitimate science in the area of quantum mechanics. Since legitimate science is assumed to be legitimate explanation, the philosopher then thinks he must discover how it is that existing quantum mechanics explains what it quite plainly does not explain.

In embracing Copenhagenism, Empiricism has already passed negative judgement on the attempts by major figures in physics, the anti-Copenhagenists, to explain what is baffling. Rejected out of hand as serious science are: David Bohm's implicate order theory; Hugh Everett's "many worlds" theory; the Idealistic theories of Wigner and Wheeler; Henry Stapp's suggestion to adopt a Whiteheadian cosmology (complete with teleology)¹²--in short, the entire area of quantum measurement theory. The neo-Humean picture of research in physics is quite impoverished compared with the reality.

It does not appear to me that any of the quantum ontologies meets the productionist criteria for an explanation. Generally they are explanations involving transcendent reality, Idealism, or teleology. Exceptions are Bohm's early neo-mechanistic theory, which is rejected by physics on solid grounds, and Everett's hypothesis, which redescribes the

problem in a manner more incomprehensible than the baffling observations themselves. But my view is allied with these attempts in holding that to supply an explanatory dimension to physics is the business of a pursuit which is independent of the mainstream technical/theoretical procedures of physics. A general strategy for a quantum ontology that productionism might adopt is suggested in Chapter 5.

"Probabilistic" Process

According to the above passage, Salmon does not think his project of constructive realism (or "ontic" explanation) has gotten very far with regard to the explanation problems of contemporary physics. However, he has offered a philosophical strategy which he apparently thinks is an achievement in this matter. Quantum physics encounters what appear to be irreducibly indeterministic effects. Hence certain mathematical models in use (which only correlate observed interactions and cannot, I believe, be called descriptions of connecting transmissions) yield probabilities as solutions. Salmon cogently demands that a contemporary theory of causality face this ineradicable indeterminism. He aims to connect his "statistical" model of explanation with the idea of a probabilistic causal process, and thereby support the idea of "causal structure" even for the strange "microcosmic world" of contemporary physics.

But his proposal is not only about the indeterministic

processes of physics. He wants to show that probabilistic processes are not something unique to this domain, but form a class including ordinary cases. This purports to be a general, probabilistic theory of causality.

He gives as an example the way a pond ripple affects a
¹³
 toy sailboat. He says about this,

...there is a certain probability that the boat will capsize when the wave reaches it. The probability will, of course, vary with the distance between the boat and the point of entry of the rock. For certain distances, the probability of capsizing will be unity [the boat will capsize] and for others it will be zero for all practical purposes, but for intermediate distances, the probability can reasonably be supposed to have intermediate values...

Ordinarily, when it is said that there is a certain probability that something will occur under given conditions, it is meant that given a number of cases of these same or similar conditions, the results show a variation which can be calculated as a statistical distribution. Though not in quantum mechanics, in ordinary cases such as this one it is assumed that this variation is due to variations in the conditions which were not accounted for in the description.

Thus to give more concrete meaning to this probability calculus about the sailboat, something like the following should be added to the example: A group of boys has designed a toy sailboat for racing, and made it particularly slender and with a large sail. They wonder how well it stays upright in waves. They experiment by dropping rocks of various sizes

into the water at varying distances from the boat.

After this they test a competing design in the same way. One appears to be better, by a small margin. They make a rough probabilistic judgement: this one is less likely to capsize. If one wanted to work into the example the calculation of specific probabilities for capsizing from, let's say, average pond ripples, one might try making the boys serious toy sailboat engineers with marketing ambitions. But the example is already fairly contrived. Calculation of probabilities does not seem to suit this example all that well.

But what Salmon wants to suggest by this probability of capsizing is something very different from this. For one thing, the probability is not something about the boat at all, but something about the wave. The probability is in fact treated by him as if it were a thing that either rides along with the wave, or is the wave itself. He says about this and some other examples,¹⁴

The basic causal mechanism, in my opinion, is a causal process that carries with it probability distributions for various types of interactions... In these cases, one process is propagating through space-time, and it carries probabilities for different sorts of interactions if it intersects with another process of a particular sort.

Continuing, Salmon's thinking displays the same propensity that was found in Harré:

It seems to me altogether appropriate to refer to these probabilities as propensities.

He uses this language to suggest some definite embedded content in the wave, just as Harré did in regard to objects. A subsequent remark is quite clear:

Causal processes transmit energy, among other things, but they also transmit propensities for various kinds of interactions under various specifiable circumstances.

Energy is a measurable attribute of a wave. Salmon clearly intends to be saying that the propensity is, like energy, a property of the wave, so that inasmuch as the wave is transmitted, so is the propensity. And a propensity is the same thing as a probability. Is a propensity something that can be transmitted from place to place? Is a probability such a thing? If we can agree that these are properties of waves, it would seem they could be transmitted, just as my propensity to absent-mindedness goes wherever I go.

But a wave on the pond cannot be said to have a propensity to overturn the sailboat. It either will or it will not, and the exact conditions obtaining determine whether it will or will not. To say it has a propensity would have to mean that identical waves under identical conditions at times capsize the sailboat, and at times, unpredictably, do not. But this is not the case. It will capsize the sailboat if it is large enough to do so. A certain design of sailboat, on the other hand, can have a tendency to fall over in waves.

What about the claim that the wave "carries probabilities"? The claim was derived from this talk: "There is a

certain probability that..." This talk can only be informing about a statistic, computed from a compilation of cases with waves of varying heights. One would only assign a probability to the event of capsizing if there were considerable unknowns among these conditions in the case at hand, say, if one threw a rock over one's shoulder into the vicinity of the boat. But given an actual wave under particular conditions, a probability for an effect is not a fact about that wave.

Probabilities and propensities are not things that can ride along with waves. And it will not help support causal explanation in physics to talk as if causal processes were often, or as a rule, probabilistic in their physical character (and not as a limitation in knowledge about them), since these ordinary cases outside of atomic physics are deterministic.

Conclusion

We have seen that the attempts Salmon has made toward an ontology of causal unobservables have basic inadequacies. Rom Harré, another constructive realist I have examined, is for his part quite clear and conscious about the limits of ontological possibilities as he sees them. He sees that unobservable transmissions in atomic experiments, which is where the real difficulties of understanding arise, cannot be described on the basis of an ontology of particles moving
 15
 within a reference grid. At the same time he says that he

is far from knowing how to procede toward a different ontology which might be adequate for these cases, and can only state an unargued preference for David Bohm's speculations about them.¹⁶ He sees the need for a fundamental shift in conceptions of physical events, advancing beyond the elements of particles and local reference space, but cannot locate and specify such a shift.

The constructive realisms of Harré and Salmon produce only some less-than-successful talk of tendencies and featureless process. We must conclude that, though what I have called constructive realism at least dimly recognizes that its objective must at some point be to produce a causal ontology adequate for the domain of physics, its own positive achievements in this regard are quite limited.

The position of the present work is that the aims of constructive realism should be reformulated in a more auspicious manner by adopting a radically alternative general point of view to that of Empiricism. Its claims would be (a) that contemporary physics is lacking an explanatory dimension; (b) that the missing explanations--descriptions of natural causal events--can be produced; and (c) that this requires a more adequate understanding of the causal relation.

Notes

1. Wesley Salmon, Scientific Explanation and the Causal Structure of the World, (Princeton: Princeton U P, 1984).
2. Salmon, 138-139.
3. Salmon, 141-142.
4. Salmon, 142.
5. Salmon, 143.
6. Salmon, 209.
7. Salmon, 139-140.
8. Salmon, 182-183.
9. Salmon, 156-157.
10. Salmon, 148.
11. Salmon, 279.
12. A thorough survey of the work of quantum ontologists can be found in Nick Herbert, Quantum Reality (New York: Doubleday, 1985).
13. Salmon, 202.
14. Salmon, 203.
15. Harré, Varieties of Realism, 264, 304, 308.
16. Harré, 315-316.

CHAPTER 5

PROBLEMS FOR PRODUCTIONISTIC EXPLANATION

This work has largely confined itself to the criticism of traditional philosophy of science. In connection with this criticism, it has sought to establish the possibility that causal narrative explanation, of the sort I have called "productionistic," is suitable and adequate for contemporary experimental knowledge.

As will be the first thought of anyone familiar with quantum physics, this project immediately faces a number of well established and notoriously perplexing results of scientific investigation. Of these I will discuss the most prominent, namely, that presented by Bell's Theorem and the experiments of John Clauser and Alain Aspect. The most that can be accomplished here is to acknowledge the difficulties that this problem presents for explanation on a productionist conception.

The productionist conception requires that observed correlations in events be accounted for by continuous, physical connections. This view seems to be rendered problematic by the "nonlocality" result of analysis and experiment based on quantum theory, which is illustrated by the following case: when two photons are paired with one another by a

specific procedure, then at a later time when these two photons have been spatially separated from one another (apparently to any extent), the result of measurement performed on one photon is shown to depend on how a similar measurement is performed on the other, without requiring any time to elapse between the two measurements. The interdependence apparently cannot be accounted for by the transmission of an influence with a finite velocity. If there is an occurrent, continuous, physical connection, as required on the productionist view, how could it be unaffected by spatial separation, regardless of the degree of separation? The productionist has the burden of showing how the connecting occurrence might be identified and characterized in some other way than as local motion or other local transmission through space.

I believe that nonlocal connections can eventually be accounted for on a productionist concept of explanation. My reasons for optimism are as follows. It seems to me that, strictly speaking, nonlocal connections are paradoxical within the traditional Cartesian ontology which continues to be the framework of description in natural science. The productionist strategy, then, would be to supercede this ontology with one more adequate for phenomena produced by radiation (e.g., light or photons), and thus, as a first step, to transform the problem of nonlocality from a paradox into a tractable problem for physical explanation.

The Cartesian ontology presupposes that any physical description has the setting of a reference system or "grid" of spatial position, and that the active elements in the description must consist of motions through successive positions within this system (it can be argued that the limit of this ontology is in any case evidenced by the breakdown in mechanistic causal models). An immutable connection between causality and locality is thus presupposed.

Alfred North Whitehead adopted an approach to physical explanation, conceived prior to developed quantum theory, which directly challenged this traditional ontology. His critique of "simple location" meant the denial that locality and motion of matter are fundamental elements in the description of natural events:¹

Thus primarily we must not think of events as in a given Time, a given Space, and consisting of changes in given persistent material.

Whitehead's proposal calls into question the doctrine that causality must consist of local transmission through space. Both the philosopher George R. Lucas, Jr.² and the physicist Henry Stapp³ have proposed the "rehabilitation" (Lucas) of Whitehead for help with problems arising from modern physics--specifically, with that presented by Bell's Theorem and nonlocal connections. Physicist David Bohm has proposed accounting for nonlocality by repudiating the doctrine that locally transmitted causality is essential to a

physical description⁴ (however, Bohm's theory is incompatible with productionism in that his new framework for description rejects causality altogether as a relation requiring differentiation among events).

The hope for productionistic physical explanation appears to lie with a transformation in natural ontology. The author regrets that the present work has the weakness that, as a work intended for a general philosophical audience, it has not penetrated further into the essential problems disclosed by physics.⁵

Notes

1. Alfred North Whitehead, An Inquiry Concerning the Principles of Natural Knowledge (New York: Dover, 1982) 26.
2. George R. Lucas, Jr., The Rehabilitation of Whitehead: An Analytic and Historical Assessment of Process Philosophy (Albany: State U of New York P, 1989) 183-199.
3. Henry P. Stapp, "Whiteheadian Approach to Quantum Theory and the Generalized Bell's Theorem," Foundations of Physics 9.1-2 (1979): 1-25.
4. David Bohm, Wholeness and the Implicate Order (New York: Routledge and Kegan Paul Ltd., 1980).
5. It is the author's view that the various "quantum" perplexities, such as nonlocality, are part of an array of unresolved problems for causal explanation which are aspects of the question, What is radiation? A theory of radiative process, its form of activity and its cosmological status, is under development by the author.

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