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SAVING THE PHENOMENA*

James Bogen

James Woodward

According to a widely shared view of science, scientific theories predict and explain facts about "observables": objects and properties which can be perceived by the senses, sometimes augmented by instruments. In the tradition associated with logical positivism, this view receives an influential formulation in terms of a supposed distinction between two kinds of vocabularies and an allied claim about the structure of theories. According to the positivists, facts about observables are reported by means of "observation-sentences," expressed in an "observational vocabulary." Explanation, prediction, and theory-testing involve the deduction of observation-sentences from other sentences, some of which may be formulated in a "theoretical" vocabulary, containing terms which do not signify observables. Such deductions are possible because theories also contain "correspondence rules" that systematically coordinate the terms of the theoretical vocabulary with terms from the observational vocabulary.[1]

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[1]For a characteristic statement of such a view see the passage quoted below from Ernest Nagel's *The Structure of Science* (New York, N.Y.: Harcourt, Brace and World, 1961). A similar picture is suggested by Rudolph Carnap in his *An Introduction to the Philosophy of Science*, ed. Martin Gardner (New York, N.Y.: Basic Books, 1974). According to Carnap, "[i]n addition to providing explanations of observed facts, the laws of science also provide a means for predicting new facts not yet observed" (p. 16). Elsewhere, Carnap writes that, "to a philosopher, 'observable' has a very narrow meaning. It applies to such properties as 'blue' and 'hard'. These are prop-

The positivist picture of the structure of scientific theories is now widely rejected. But the underlying idea that scientific theories are primarily designed to predict and explain claims about what we observe remains enormously influential, even among the sharpest critics of positivism.[2] Indeed, the characteristic move of post-positivist writers on observation has been to extend vastly the notion of observation and observability. These writers argue that many things (for example, electrons, water molecules, the interior of the sun) which the early positivists would have regarded as unobservable are observable after all. They also draw various important conclusions about the nature of science from these claims about observation. Consider, for example, recent attempts to cast doubt on the possibility of objective, non-circular tests of competing theories by appealing to the "theory-ladeness" of observation or to facts about "expectancy effects" or "set" in perception. These arguments presuppose a conception of science in which theories are tested by considering their predictions about what we observe.[3] Similarly, the common strategy of arguing for, say, the reality of electrons by trying to show that we can "observe" them would make little sense except against the background of a conception of science which accepts the (characteristically positivist)

erties directly perceived by the senses" (p. 225). Carnap goes on to mention an example of an "empirical law" which can be derived from theoretical laws with the help of correspondence rules: "If there is an electromagnetic oscillation of a specified frequency, then there is a visible greenish-blue color of a certain hue" (p. 233). Here we have the clear suggestion that scientific theories are such as to permit the deduction of sentences ascribing properties like greenish-blue.

[2]Not everyone accepts this idea. Notable exceptions include Nancy Cartwright, *How the Laws of Physics Lie* (Oxford, England: Oxford University Press, 1983); Ian Hacking, *Representing and Intervening* (Cambridge, England: Cambridge University Press, 1983); Ronald Laymon in a series of papers, of which "Idealizations and the Testing of Theories by Experimentation" in P. Achinstein and O. Hannaway, eds. *Observation, Experiment and Hypothesis in Modern Physical Science* (Cambridge, England: Cambridge University Press, 1985), pp. 146–173; and "Newton's Demonstration of Universal Gravitation and Philosophical Theories of Confirmation" in *Minnesota Studies in the Philosophy of Science*, volume XI, ed. J. Earman (Minneapolis, Minnesota: University of Minnesota Press, 1983), pp. 179–199, are representative.

[3]See the discussion of Kuhn and Hanson in Sections VIII, IX and X below.

premise that one argues for the reality or scientific legitimacy of an entity by showing that it is observable.[4]

Our aim in this paper is to show that this aspect of the traditional account is fundamentally incorrect: if, like most philosophers, we use "observe" to mean "perceive" or "detect by means of processes which can be usefully viewed as extensions of perception,"[5] then scientific theories typically do not predict and explain facts about what we observe. The facts for which typical scientific theories are expected to account are not, by and large, facts about observables. Indeed, we think that the whole category of observation (and, correlatively, of what is or is not observable), and the disputed questions about whether and in what respects observation is theory-laden are much less central to understanding science than many have supposed. Our argument turns on an important distinction which we think has been ignored in most traditional analyses of science: the distinction between data and phenomena. Data, which play the role of evidence for the existence of phenomena, for the most part can be straightforwardly observed. However, data typically cannot be predicted or systematically ex-

[4]See the discussion of Maxwell and van Fraassen in Section XI below.

[5]It is worth explicitly noting that scientists themselves often use "observe" in a different way: to mean something like "detect" or (even more vaguely) to indicate that their grounds for believing a claim about an entity are based in part on information obtained by means of a causal interaction of some appropriate sort with the entity, and are not based just on an inference from general theory. On this usage, there is no suggestion that the observed entity is perceived, or that the processes underlying its detection are in any very interesting respects analogous to those that underlie perception. For example, in his recent book *Story of the W and Z* (Cambridge, England: Cambridge University Press, 1986), Peter Watkins speaks at several points of the "observation" of W and Z bosons in experiments conducted at CERN. But Watkins also writes:

> We never observe [these] particles themselves, but only the results of their passage through material. . . . The particles we are dealing with are far too small to see (p. 109).

While the W and Z have been observed in the sense that they have been produced in the apparatus at CERN and successfully detected, Watkins does not think that they have been perceived or seen.

It should be clear in what follows that we have no objection to the use of "observe" to mean "detect" and no objection to the claim that in this sense of "observe," many of the facts that scientific theories explain are facts plained by theory. By contrast, well-developed scientific theories do predict and explain facts about phenomena. Phenomena are detected through the use of data, but in most cases are not observable in any interesting sense of that term. Examples of data include bubble chamber photographs, patterns of discharge in electronic particle detectors and records of reaction times and error rates in various psychological experiments. Examples of phenomena, for which the above data might provide evidence, include weak neutral currents, the decay of the proton, and chunking and recency effects in human memory.[6] (Some of these examples will be discussed in more detail below.)

Facts about phenomena may also serve as evidence, but typically such facts are evidence for the high-level general theories by which they are explained. For example, the existence of weak neutral currents is a crucial piece of evidence in favor of a theory devised by Weinberg and Salam, which unifies the weak and electromagnetic forces. With respect to their evidential role what distinguishes data from phenomena is *not* that only facts about data may serve as evidence, but rather that facts about data and facts about phenomena differ in what they serve as evidence *for* (claims about phenomena versus general theories).

Some versions of the claim that scientific theories explain facts about what we observe amount to the mistaken idea that theories explain facts about data. Other versions use the notion of observation in a way which systematically blurs the distinction between data and phenomena, with the result that important features of scientific practice are obscured.

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about what we observe. Our criticisms are rather directed at those who hold that scientific theories explain what we observe and who then go on to tie the relevant notion of observation rather closely to sensory perception. As we shall see below, this includes the majority of philosophers who discuss the role of observation in science.

[6]In his *Representing and Intervening*, Ian Hacking introduces a notion of phenomena which is similar in a number of respects to our notion. But Hacking contends (pp. 227–232) that phenomena rarely occur in nature and that most phenomena studied by physicists are manufactured in the laboratory. While this is certainly true of some phenomena, such as a number of those created in very high energy particle accelerators, we claim below that is not correct as a general characterization of phenomena. These features which Hacking ascribes to phenomena are more characteristic of data.

I.

There are some cases in which the traditional account is roughly correct. For example, early astronomers did produce theories which attempted to explain and predict what could be observed, with the naked eye or with the use of telescopes. But a scientific discipline which is marked by attempts to predict and explain what is observed is usually at a relatively naive and primitive stage of development. As the case of astronomy suggests, an important source of progress in science is the development of procedures for the systematic handling of observational and measurement error and procedures for data-analysis and data-reduction which obviate the need for a theory to account for what is literally seen.

To appreciate this point, consider a well-known passage from Ernest Nagel's *The Structure of Science*.[7] This passage illustrates the importance of distinguishing between data and phenomena, and also shows why claims about what is observed are usually not good candidates for systematic explanation. Nagel begins with some remarks which at least suggest the view we are concerned to criticize: that scientific theories typically explain and predict facts about what is observed. He writes:

> Scientific thought takes its ultimate point of departure from problems suggested by observing things. . . . [I]t aims to understand these observable things by discovering some systematic order in them; and its final test for laws that serve as instruments of explanation and prediction is their concordance with such observations. . . . Indeed, many laws in the sciences formulate relations between things or features of things . . . commonly said to be observable, whether with the unaided senses or with the help of special instruments of observation.

Nagel then goes on to offer illustrations of some laws about observables and to suggest that they are explained by means of theories that make reference to unobservable objects and features:

> The law that when water in an open container is heated it eventually evaporates is a law of this kind [that is, a law which formulates a relationship between observables], and so is the law that lead melts at 327 degrees C. . . . However . . . many laws employed in some of the most

[7]Ernest Nagel, *The Structure of Science*, p. 79.

> impressively comprehensive sciences are notoriously not about matters that would ordinarily be characterised as 'observable', even when the word 'observable' is used as broadly as in the examples [above]. . . . Thus when the evaporation of heated water is explained in terms of assumptions about the molecular constitution of water, laws of this latter sort appear among the explanatory premises.

Let us focus on Nagel's example of the melting point of lead. Despite what Nagel's remarks seem to suggest, one does *not* determine the melting point of lead by observing the result of a single thermometer reading. To determine the melting point one must make a series of measurements. Even when the equipment is in good working order, and sources of systematic error have been eliminated, the readings from repeated applications of the same thermometer to the same sample of lead will differ slightly from one another, providing a scatter of results. These constitute data. Given the absence of systematic error, a standard assumption is that the scatter of observed thermometer readings not only reflects the true melting point (the phenomenon in which we are interested), but also the operation of numerous other small causes of variation or "error," causes which cannot be controlled for and the details of which remain unknown. If one can make certain general assumptions about the character of these causes of variation (for example, that they operate independently, are roughly equal in magnitude, are as likely to be positive as negative, and have a cumulative effect which is additive), then it will follow that the distribution of measurement results will be roughly normal and that the mean of this distribution will be a good estimate of the true melting point. Standard scientific practice is to report this estimate along with the associated standard error, which is directly calculable from the variance of the distribution of measurement results.

There are several points we want to make about this example. Note first that Nagel appears to think that the sentence "lead melts at 327 degrees C" reports what is observed. But what we observe are the various particular thermometer readings—the scatter of individual data-points. The mean of these, on which the value for the melting point of lead reported by Nagel will be based, does not represent a property of any particular data-point. Indeed, there is no reason why *any* observed reading must exactly coincide with this mean value. Moreover, while the mean of the observed measurements has various properties which will (on assumptions like those described above) make it a good estimate of the true value of the melting point, it will not, unless we are lucky, coincide exactly with that value. (In fact, the standard error gives us a measure of the probability that an interval centered on the mean will contain the true value.) It is even more obvious that the standard error does not report what is observed or seen. So while the true melting point is certainly *inferred* or *estimated* from observed data, on the basis of a theory of statistical inference and various other assumptions, the sentence "lead melts at 327.5 ± 0.1 degrees C"—the form that a report of an experimental determination of the melting point of lead might take—does not literally describe what is perceived or observed.

Nagel is quite right to say that what a theorist will try to explain is why the true melting point of lead is 327 degrees C. But we need to distinguish, as Nagel does not, between this potential explanandum, which is a fact about a phenomenon on our usage, and the data which constitute evidence for this explanandum and which are observed, but which are not themselves potential objects of explanation. It is easy to see that a theory of molecular structure which explains why the melting point of lead is approximately 327 degrees could not possibly explain why the actual data-points occurred. The outcome of any given application of a thermometer to a lead sample depends not only on the melting point of lead, but also on its purity, on the workings of the thermometer, on the way in which it was applied and read, on interactions between the initial temperature of the thermometer and that of the sample, and a variety of other background conditions.[8] No single theory could

[8]To measure the temperature at which a single sample of lead melts, the observer must take a reading just as the melting begins. A standard method is to put a small amount of finely powdered metal in a thin capillary tube. A thermometer sensor (for example, a thin piece of wire connected to a thermocouple) is fixed to the outside of the tube. When the sample begins to melt, it vaporizes, changing the color of the tube. The observer records the thermometer reading as soon as he notices a color change. No matter how thin the capillary tube, there is always enough space between the lead and the sensor to guarantee a discrepancy between the temperature of the sample and the reading. No matter how attentive the observer may be, there is no guarantee that he will be able to tell precisely when the sample first begins to melt. Without going into additional detail, we leave it as an exercise to the reader to list the various

accurately predict or explain an outcome which depends upon the confluence of so many variable and transient factors.

One could, of course, *say* that calculating the mean (or variance) of a distribution of thermometer readings ought to be regarded as a case of "theory-laden" observation. But it is not clear what the point of such a stipulation would be, or why anyone should think it would be an illuminating thing to say in connection with the above example. The scientific and methodological problems that arise in connection with the example are problems of data-analysis and statistical inference. One wants to know, for example, what conditions have to be met if the true melting point is to be estimated reliably from the observed data, what sort of properties different estimating procedures have, how to determine the likely error in one's estimate, and so forth. The various arguments put forward by philosophers for and against conceptions of observation that would allow one to say that the melting point of lead is observed seem to shed no light on such matters. Suppose that we resolved all the traditional disputes in psychology and philosophy about what we *really* perceive, about the distinction between indirect and direct perception, and about whether there is a principled distinction between observational and theoretical predicates. Suppose also that we solved all the problems which presently occupy psychologists about the nature of affective and proximal sensory stimuli, and about where perceptual processes leave off and non-perceptual inferences begin. It seems to us that none of this would be relevant to understanding how experimentalists can justifiably move from data like thermometer readings to a conclusion about the melting point of lead. Interesting as such issues about observation may be in other contexts, they lead us away from what is analytically most central in connection with the melting point of lead. They divert our attention to such extraneous matters as whether an experimenter really sees that lead melts at 327 degrees or whether he merely sees instead that the top of the mercury column coincides with a certain marking on the thermometer.

This is not to deny, of course, that perceptual factors are sometimes decisive in settling disputes about the reliability of data. But,

factors which can influence a bit of data obtained through this procedure. We are indebted to Tad Backman for information concerning this example.

contrary to what one might suppose, standard philosophical discussions tend to be unilluminating even when the reliability of perception is the central issue. Consider, for example, the important experimental controversy between Rutherford in Cambridge and Petterson in Vienna in the 1920s concerning whether elements like carbon emit protons under bombardment by alpha particles. As Roger Steuwer describes in a recent paper,[9] both the group at Cambridge and the group at Vienna relied on observations (using a low-powered microscope) of scintillations—tiny flashes of light produced when a charged particle strikes a scintillation screen. Issues about the circumstances under which trained observers could reliably distinguish and count scintillations of varying brightness turned out to be quite crucial in resolving the controversy. As a result of empirical investigations of the characteristics of human observers, the Cambridge group showed that even trained observers could not reliably distinguish the flashes characteristic of protons from flashes characteristic of alpha particles.

Here, facts about visual perception do bear in a non-trivial way on the reliability of an inference from data (flashes on a scintillation screen, or perhaps reports of those flashes) to a phenomenon (emission of protons). But general arguments about whether observation is theory-laden, or about whether it is appropriate to talk of seeing protons and alpha particles or just the flashes they cause, do not help us to understand the role of observation in such a case. What is required instead is a detailed empirical investigation of the performance of the human visual system under various conditions.

Notice also the difference between the considerations relevant to establishing reliability in this case and the considerations relevant to establishing that the mean of the distribution of thermometer readings provides a reliable estimate of the melting point of lead. The latter considerations do not have anything to do with the characteristics of the human perceptual system. They are considerations of the sort discussed in textbooks on statistical inference

[9] Roger H. Stuewer, "Artificial Disintegration and the Cambridge-Vienna Controversy" in *Observation, Experiment and Hypothesis in Modern Physical Science*, ed. P. Achinstein and O. Hannaway (Cambridge, Mass.: The MIT Press, 1985).

and data-analysis. They are not what someone interested in sensory psychology or physiology might undertake to investigate. Important differences between the perceptual and non-perceptual considerations which bear on reliability are only obscured when every case in which phenomena are detected from observed data is described as a case in which that phenomenon is observed.

As our discussion suggests, when observational error is a serious possibility in actual practice, the kind of error to be corrected for will differ from case to case and will depend upon the effects of many different conditions peculiar to the subject under investigation, the experimental design, and the equipment used. We can begin to get an idea of how various these local factors can be (and how various are the methods needed to correct for the errors they produce) by comparing the Cambridge-Vienna controversy with the difficulties which plague a neuroanatomist studying a bit of stained tissue under a light microscope. Petterson's technicians had to count and discriminate flashes of differing degrees of brightness. The neuroanatomist must pick out parts of the structure of interest from a tangle of extraneous items visible at one focal length. He must then follow their connections to other bits of structure (equally well obscured by extraneous detail) as he changes the focus of the microscope. The techniques of slicing, staining, and mounting the tissue, without which the structure would not be visible, produce artifacts which can be confused with parts of nerve cells. Tiny details, crucial to the required discriminations may be partially obscured from sight. Instead of merely keeping a count, the neuroanatomist may be required to record what he sees in the form of drawings. In short, his observations are complicated by sources of error which Petterson never had to face.

Reflection on such examples suggests that the factors contributing to observational error, and the means for correcting for it in different cases, are far too various to be captured in an illuminating way in a single general account. In some cases, as with Petterson's technicians, the visual system is simply unable to make certain kinds of discriminations reliably. In other cases—for example in the notorious case of the supposed detection of non-existent N-rays by French physicists in the early twentieth century—observational errors are due, at least in part, to the observer's mental set. As Irving Klotz shows in his study "The N-Ray Affair,"[10] French nationalism and the discovery of other kinds of novel radiation (such as X-rays, and B-decay) predisposed those involved to accept that N-rays existed. In other cases—for example, in studies of the speech of urban black children—observation is known to be highly sensitive to cultural biases and expectations of various kinds. William Labov in his *Language in the Inner City*,[11] describes a number of cases in which the biases and background beliefs of educational psychologists studying the language abilities of urban black children infected their investigations with artifacts which led to many incorrect findings. Understanding the factors which can contribute to various kinds of observational error requires detailed empirical studies of specific cases for which the formal and highly general techniques of traditional philosophy of science are not suitable. Thus, standard philosophical discussions of observation tend not only to neglect the role of non-perceptual (for example, statistical) considerations in the assessment of the reliability of data, but also tend to be unhelpful even when perceptual factors are relevant.

In contrast to the difficulties and obstacles surrounding the standard philosophical issues regarding perception, observation, and theory-ladeness, it is often relatively easy, on a case-by-case basis, to identify the data from which a scientist works, the considerations which are relevant to the reliability of this data, and the phenomena for which the data is taken to be evidence. Detailed investigations of this sort can cast a great deal of light on what scientists actually do and on how reliable knowledge is acquired in science. Such investigations would show that many different kinds of problems arise in connection with inferring phenomena from data and that many disparate procedures are employed in various areas of scientific investigation for dealing with such problems. Many of these problems and procedures have little to do with observation and perception, as philosophers understand them. They deserve detailed investigation in their own right. Adopting our distinction between data and phenomena, and focusing on the dis-

[10] Irving Klotz, "The N-Ray Affair," *Scientific American* 242, no. 5 (1980), pp. 168–180.

[11] William Labov, *Language in the Inner City* (Philadelphia, Penn.: University of Pennsylvania Press, 1972), pp. 201–240.

parate factors which affect the reliability of data that serves as evidence for phenomena will, we believe, facilitate such investigation.[12]

Our general thesis, then, is that we need to distinguish what theories explain (phenomena or facts about phenomena) from what is uncontroversially observable (data). Traditional accounts of the role of observation in science blur this distinction and, because of this, neglect or misdescribe the details of the procedures by which scientists move from claims about data to claims about phenomena. In doing so, such accounts also overlook a number of considerations which bear on the reliability of scientific knowledge.

II.

Our discussion will proceed as follows. In Section III, we explore the distinction between data and phenomena in more detail. In Sections IV and V, we defend the claim that scientific theories explain facts about phenomena rather than data. In Sections VI, VII, and VIII, we extend this defense by attempting to show how a concern with whether data are reliable evidence is different from a concern with the explanation of data. In Section IX through XIII,

[12]Recent detailed historical studies of important experiments and experimental techniques in twentieth-century physics by Allan Franklin and Peter Galison are models of the sorts of detailed investigations we have in mind. A number of Franklin's papers are included in his recent book *The Neglect of Experiment* (Cambridge, England: Cambridge University Press, 1986). Representative papers by Galison include "How the First Neutral-Current Experiments Ended," *Reviews of Modern Physics* 55 (1983), pp. 477–509, and "Bubble Chambers and the Experimental Workplace," in *Observation, Experiment and Hypothesis in Modern Physical Science*, ed. P. Achinstein and O. Hannaway (Cambridge, Mass.: The MIT Press, 1985). A number of recent sociological studies, including Andrew Pickering, *Constructing Quarks* (Chicago, Ill.: University of Chicago Press, 1984) and Trevor Pinch, *Confronting Nature: The Sociology of Solar Neutrino Detection* (Dordrecht, Holland: D. Reidel Publishing Company, 1986) are also valuable and suggestive sources of empirical information about the disparate problems and procedures which are relevant to inferences from data to phenomena in various experimental contexts. In most cases this empirical information can be readily separated from the relativist and constructivist conceptions of scientific knowledge which Pickering and Pinch defend.

we relate our framework to various familiar philosophical treatments of observation and observability. In the course of our discussion we will be concerned to rebut the following objections, which may have already occurred to the reader:

O1. What we say about data and phenomena merely repeats, in different language, familiar points about the theory-ladeness of observation. Claims about phenomena are just relatively theory-laden observational claims, and claims about data are simply less theory-laden observational claims. Our repy to this, elaborated in Sections IX–XII, is that it is a mistake to think of claims about phenomena as theory-laden observational claims.

O2. Contrary to what we say, theories do predict and explain claims about data. Indeed were this not the case, we would be in no position to assess the reliability of data as evidence. Our response to this in Section VI shows that many considerations which are relevant to reliability have little to do with the provision of explanations.

O3. There really is no principled distinction to be drawn between data and phenomena. Our distinction is no better off from the old discredited dichotomy of observational versus theoretical claims. Our reply to this, contained in Sections III and IV, is that although there are grey areas and borderline cases, data and phenomena typically differ in clear and important respects.

III.

How do phenomena differ from data? It will be useful to begin with some examples which illustrate the distinction we have in mind.

1. In experiments conducted at CERN and, independently, at NAL, physicists successfully detected the phenomenon of weak neutral currents in 1973. (These experiments will be discussed in more detail below.) The data obtained at CERN consisted of approximately 290,000 bubble chamber photographs of which roughly 100 were thought to provide evidence for the presence of neutral currents. The quite different data obtained at NAL consisted of records of patterns of discharge in electronic particle detectors. In what proved to be a crucial run of experiments, 8 of

approximately 330 records were interpreted as evidence for neutral currents.[13]

2. In a widely discussed experiment, a vat of cleaning fluid containing an isotope of chlorine was buried a mile under the Black Hills. Neutrinos emitted from interactions in the core of the sun struck the cleaning fluid and interacted with the chlorine to produce a radioactive isotope of argon. This was periodically flushed out of the tank by running helium through it. Geiger counters were then used to measure radiation from the argon. The data thus produced (flashes on a screen called "splodges") were analyzed to estimate the rate of neutrino emission from the sun. This rate is a phenomenon for which theories describing nuclear reactions occurring in the sun must account.[14]

3. The question of whether there is a distinctive psychological function carried out by the frontal lobes is of considerable interest to neurophysiologists. In an effort to isolate such a function, Brenda Millner and Hans-Lukas Teuber compared the performance of patients with frontal lobe damage due to surgical ablation (Millner) and gunshot wounds (Teuber) with normal controls on a variety of tests. The tests involved sorting cards, visual searches, and various orientation tasks. The data consisted of drawings made by surgeons showing areas of ablation (Millner), and X-ray photographs of the skull (Teuber), together with the test scores. Millner interpreted her data as indicating that damage to the frontal lobes impairs a subject's ability to give up unsuccessful problem-solving strategies and devise new ones. Teuber thought his data indicated an impairment of a certain kind of coordination between motor and sensory functions. If Millner was right, behavioral perseveration was the phenomenon her data in-

[13]For details of the experiments at CERN and NAL, see Peter Galison, "How the First Neutral-Current Experiments Ended"; and Andrew Pickering, "Against Putting the Phenomena First: The Discovery of the Weak Neutral Current," *Studies in the History and Philosophy of Science* 15, no. 2 (1984), pp. 85–117.

[14]For relevant discussion see Trevor Pinch, "Towards An Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics," *Social Studies of Science* 15 (1985), pp. 3–36. See also Dudley Shapere, "The Concept of Observation in Science and Philosophy," *Philosophy of Science* 49 (1982), pp. 485–525.

dicated. If Teuber was right, the phenomenon indicated was a kind of dysfunction of sensory processing.[15]

These examples illustrate a number of important points about data and phenomena. Instances of each of the phenomena described above can occur in a wide variety of different situations or contexts. This, in turn, is closely connected with the fact that the occurrence of these instances is (or is plausibly thought to be) the result of the interaction of some manageably small number of causal factors, instances of which can themselves occur in a variety of different kinds of situations. Thus neutral currents, which involve the exchange of a short-lived particle called the Z°, will occur in a wide variety of interactions involving the weak force, one of the four fundamental forces in nature. Neutral currents will be produced, at a small but calculable rate, whenever a neutrino strikes a nucleon and in many other natural processes as well—for example, in many kinds of radioactive decay. By contrast, many different sorts of causal factors play a role in the production of any given bit of data, and the characteristics of such items are heavily dependent on the peculiarities of the particular experimental design, detection device, or data-gathering procedures an investigator employs. Data are, as we shall say, idiosyncratic to particular experimental contexts, and typically cannot occur outside of those contexts. Indeed, the factors involved in the production of data will often be so disparate and numerous, and the details of their interactions so complex, that it will not be possible to construct a theory that would allow us to predict their occurrence or trace in detail how they combine to produce particular items of data. Phenomena, by contrast, are not idiosyncratic to specific experimental contexts. We expect phenomena to have stable, repeatable characteristics which will be detectable by means of a variety of different procedures, which may yield quite different kinds of data.[16]

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[15]Brenda Millner, "Some Effects of Frontal Lobectomy in Man," Hans-Lukas Teuber, "The Riddle of Frontal Lobe Function in Man," Chapters 15 and 20 in *The Frontal Granular Cortex and Behavior*, ed. J. M. Warren and K. Akert (New York, N.Y.: McGraw-Hill, 1964).

[16]For example, as remarked above, neutral currents were detected not only from data which consisted of photographs of bubble chamber tracks, but also, in a second experiment, from very different data produced in electronic particle detectors. Similarly, if the frontal lobes have a distinc-

Consider the experiments which detected weak neutral currents. While neutral currents themselves figure in interactions which occur in a wide range of circumstances, the data from bubble chambers or electronic particle detectors which constitutes evidence for neutral currents will occur only in a few laboratories, in which rare and highly specialized equipment is employed. Thus, the characteristics of the bubble chamber photographs that served as data in the experiments conducted at CERN, depended not just on the presence of neutral currents themselves, but on a complex variety of other factors, which were unique to the apparatus employed at CERN. These included the characteristics of the neutrino beam employed, the characteristics of the shielding that surrounded the bubble chamber (a matter of considerable concern, as we shall see below), the dimensions of the chamber itself and the liquid it contained, the causal factors underlying the production of the tracks, the details of the processes by which photographs were taken and developed, the details of the processes by which the impulses from the beam, the expansion of the chamber, and the taking of photographs were all coordinated, and many other factors as well. Whether, in any given case, a photograph was produced which was usable as evidence similarly depended upon a great many complex considerations. Sometimes relevant interactions occurred but were not recorded in photographs. Sometimes extraneous interactions left tracks which made a photograph too noisy for use as evidence. Other interactions, as we note in more detail below, can produce data which mimic the data produced by weak neutral currents, and, unless one controls for this possibility, the data actually produced by neutral currents will not constitute reliable evidence for the existence of neutral currents. Thus, an item of data which counts as reliable evidence for the occurrence of a weak neutral current will be the result of a highly complex and unusual coincidence of circumstances.

A similar point applies in connection with the other examples

tive psychological function, this ought to be detectable in a variety of different psychological tests and problem-solving tasks. By contrast, if an alleged phenomenon appears to be detectable only from one very specialized body of data, or if the characteristics of the phenomenon appear to vary greatly depending upon the details of the detection-procedure employed, this will raise the suspicion that the alleged phenomenon is spurious—an artifact, as one says, rather than a "real effect."

described above. While the melting of lead occurs whenever samples of lead are present at the appropriate temperature and pressure, and results from a characteristic change in the crystalline structure of this metal, the observed value of each thermometer reading depends not just on the temperature at which the lead began to melt, but on various perceptual and cognitive factors at work in the observer, on the various factors which determine the workings of the thermometer, on the mechanisms by which heat is transmitted from the sample to the thermometer, and no doubt on various other sources of random and systematic error as well.

Similarly, the psychological functions which Millner and Teuber ascribed to the frontal lobes ought to be exhibited in a wide variety of everyday behavior. But the data (drawings, photographs, and test scores) which Millner and Teuber appealed to as evidence for their claims were the result of many complex factors which were idiosyncratic to the particular techniques of measurement and detection they employed. The characteristics of this data were determined in part by such transient factors as whether Millner's surgeons drew pictures of ablations from their memories of what they saw while performing operations (and if so, what they were able to see) or from their memories of what they took themselves successfully to have ablated. Other relevant factors included the workings of Teuber's X-ray and photographic equipment, the peculiarities of the investigators who administered and scored the tests, the degree to which patients were cooperative and interested, other physiological and psychological features which could affect their performance, and of course the characteristics of the tests themselves. Once again, it takes a good measure of coincidence and a great deal of what is random relative to the predictive and explanatory resources of any available theory to produce any single bit of data.

These features of data—the fact that data are typically the result of complex interactions among a large number of disparate causal factors which are idiosyncratic to a particular experimental situation—are closely tied to the evidential role that data are expected to play in science. Often the characteristics which data must have to be useful as evidence can only be purchased at the cost of tolerating a great deal of complexity and idiosyncrasy in the causal processes which produce that data. For example, one requirement on data is that it must occur in a form which is accessible to our senses. Consider the detection of neutrinos in this light. Neutrinos are uncharged and subject only to the weak force. The overwhelming majority of neutrinos which strike an observer will not only pass right through the observer, but through the earth itself without interacting with either. These facts by themselves virtually guarantee that getting neutrinos to produce records which are accessible to the human sensory system will require a great deal of subtle contrivance and that the causal chain running from the neutrinos to such records will be long and complex. Thus, for example, in the neutral current experiments conducted at CERN, researchers followed the strategy of first getting the neutrinos to interact with matter to produce charged particles and then getting the charged particles to interact with a standard detector (in this case, a bubble chamber) in such a way as to produce records which were visually detectable.

Merely getting phenomena to produce data which is in principle so accessible is, however, only a small part of what is required to produce useful and reliable data. For one thing, matters must be arranged so that data is produced sufficiently frequently and in sufficiently large amounts that human beings can detect enough of it in reasonably short periods of time to support conclusions about the existence of phenomena. Here too, there is often a trade-off between this desideratum and complexity in the processes which produce data. In the neutral current experiments, for example, some investigators initially favored the use of interactions involving the scattering of an electron off a neutrino. This had the advantage that it is a rather straightforward interaction, which was thought to be well understood and which is relatively clear of background effects. It had the very serious disadvantage that it occurs very rarely, and thus is unlikely to yield enough data to support statistically reliable conclusions. For this reason, the investigators focused on interactions involving neutrinos and nucleons, which produce much more data, even though these interactions are in various ways much more complex and, as explained below, less well understood. Here complexity in the processes which produce data is an unavoidable by-product of the need to obtain sufficiently large amounts of new data.

Data must also be such that it is relatively easy to identify, classify, measure, aggregate, and analyze in ways that are reliable and reproducible by others. For example, it is because they wish to produce data having these features that Millner and Teuber employ standardized tests like the Wisconsin Card Sorting Test, which are easy to administer and produce standardized, unambiguous responses to various specified behavioral tasks. The use of such tests makes it possible for other investigators to administer the same tests and to classify the resulting data in similar ways and thus to check Millner's and Teuber's results. However, it also has the result that this data will be produced only in a highly specialized experimental context in which many causally relevant factors must come together in precise ways.

Moreover, data must be made to occur in a form which is tractable with respect to the demands of data-reduction and statistical analysis—a consideration which is crucially important in high energy physics (cf. Section VI). Data must also result from processes in which there has been adequate control for various kinds of experimental error (cf. Section VI). Here again, data having these desirable features rarely occurs naturally. Instead, it is typically the product of laborious, elaborate, and carefully planned contrivance on the part of the investigator. As before, scientists tolerate the resulting complexity and idiosyncrasy in order to get data with epistemologically desirable features.

We conclude this section with some remarks about the ontological status of phenomena. It should be clear that we think of particular phenomena as in the world, as belonging to the natural order itself and not just to the way we talk about or conceptualize that order. Beyond this, however, we are inclined to be ontologically non-committal. Phenomena seem to fall into many different traditional ontological categories—they include particular objects, objects with features, events, processes, and states. Perhaps some phenomena are best thought of as having a structure more like that traditionally ascribed to facts or states of affairs. Some of the phenomena we have discussed, such as the melting point of lead or the psychological deficits characteristic of patients with frontal lobe lesions, may not be readily classifiable in terms of any of the traditional ontological categories. We have not attempted to characterize a single ontological category to which all phenomena belong, both because we do not know how to provide an illuminating classification of this sort, and because doing so is not essential for the purposes we pursue in this paper. For our purposes, what matters most about phenomena is the distinctive role they play in connection with explanation and prediction, the general features they possess which suit them to this role, and the way in which they contrast in these respects with data. For our purposes, anything which can play this role and which has these general features can qualify as a phenomenon, and this is why (like the scientists whose activity we claim to be describing) we are inclined to be somewhat casual about matters of ontological classification.

IV.

The contrasting features of data and phenomena described above have a very important consequence which is central to our discussion. Typically, scientific theories are expected to provide (what we shall call) systematic explanations of facts about phenomena rather than facts about data. To show why this is the case, we will briefly describe in this section two features which typical systematic explanations possess.[17] The first has to do with the ex-

[17]A remark is in order here about the point and scope of the requirements introduced below. In ordinary usage, the identification of a single factor which played a causal role in the production of some outcome is often described as an explanation of that outcome, even when many other factors also played a role in producing the outcome in question, and even when the details of the mechanism by which the identified factor produced the outcome are left vague and unspecified. Let us call such explanations "singular causal explanations." Nothing in our discussion is meant to deny that it is sometimes appropriate to regard claims about phenomena as providing singular causal explanations of claims about data. Thus, for example, one might speak of the occurrence of neutral currents as providing an explanation of facts about the bubble chamber photographs which represent evidence for such occurrences, where this simply means that neutral currents were one of a large set of factors which played a role in the production of this data. Our point in introducing the requirements described below is *not* to challenge this usage. Rather our point is simply that such singular causal explanations differ in important ways from the kind of explanations we describe as "scientific" or "systematic," and which we intend our requirements to characterize. We claim that many scientific theories do provide such systematic explanations meeting our requirements, that such explanations are highly prized by scientists, and that facts about phenomena are more appropriate candidates for the explananda of explanations meeting these requirements than facts about data. From our point of view, what is important is whether these descriptive claims are correct, and not whether explanations of data meeting other requirements are possible.

hibition of detailed patterns of dependency. The second has to do with unification and systemization. We will then attempt to show that explanations of facts about data meeting such requirements often will be difficult, if not impossible, to construct and, moreover, that there will be little scientific point in doing so, even if this were possible.

To begin with the first requirement: according to the notion of explanation we are trying to characterize, it is not a satisfactory explanation of an outcome merely to assert that it is due to some general mechanism, where the details of the mechanism are left unspecified. Instead, a satisfactory systematic explanation must show how the features of the explanandum-phenomenon systematically depend upon the factors invoked in the explanans of that explanation. It is true, for example, that the behavior of many gases approximately conforms to the Charles-Boyle law because of facts about the interactions among the molecules that make up those gases. But merely to say this, without characterizing the relevant mechanical properties of the molecules in question and the laws governing their interaction and without showing how these give rise to the Charles-Boyle law, is not to provide an adequate systematic explanation. A satisfactory systematic explanation requires instead something like what Maxwell and Boltzmann provided—an account of how, given certain assumptions about the initial conditions of the molecules (for example, assumptions about the distribution of their velocities), and assumptions about the laws governing their interactions (for example, that the laws are those of Newtonian mechanics), one can derive some approximation to the Charles-Boyle law (for example, by solving the Boltzmann transport equation). Similarly, as Ian Hacking has recently claimed,[18] it is not a serious scientific explanation of the operation of an ordinary optical microscope simply to say that the image is produced (somehow) via the causal interaction of the light with the specimen. For genuine understanding one needs to know the optical laws governing the production of the image, and that, contrary to what was originally supposed, the image is produced as a result of the interference of transmitted and diffracted rays.

[18]Ian Hacking, "Do We See Through a Microscope?" *Pacific Philosophical Quarterly* 62 (1981), pp. 305–322.

In highly mathematical sciences like physics, portions of chemistry, population genetics, and micro-economics, dependency-relations are typically exhibited by solving systems of differential equations. Here one sees patterns of explanation approximating, at least in some important respects, the classical deductive-nomological model: explananda are literally derived from generalizations describing the behavior of a wide range of different systems, given appropriate assumptions about initial and boundary conditions. It is frequently the case that if such derivations are actually to be carried out, theorists must rely on idealizations, approximation, and simplifications of various kinds. Scientists make use of such devices in order to secure generality, or to avoid computational intractabilities, or when they do not know how to represent complex interactions mathematically. However, this merely illustrates the importance attached to the exhibition of detailed and systematic patterns of dependency. Theorists will employ premises that they know cannot be literally true or fully descriptively adequate rather than forego the construction of derivations that exhibit such patterns.[19]

In some areas (for example, portions of molecular biology and neurophysiology), the quantitative theories and mathematical techniques needed for explicit derivation of explananda may be unavailable. Here dependency-relations are exhibited by the detailed tracing of the causal mechanisms responsible for the facts to be explained. For example, there are no equations which systematically exhibit the connections between abnormalities in the substantia nigra and dopamine deficiencies in the caudate nucleus and putamen and the symptoms of Parkinson's disease they produce. Instead, one is given detailed accounts of the role played by the substantia nigra in the synthesis of dopamine required by the caudate and putamen, and somewhat less detailed causal stories

[19]The ubiquity of idealizations and approximations in explanations in the mathematical sciences and their role in securing generality and computational tractability is emphasized in the work of Nancy Cartwright and Ron Laymon. See, for example, Nancy Cartwright, *How the Laws of Physics Lie* and Ronald Laymon, "Scientific Realism and the Hierarchical Counterfactual Path from Data to Theory" in *PSA 1982*, ed. P. Asquith and T. Nickles (East Lansing, Mich.: Philosophy of Science Association, 1982).

about the roles of these parts of the brain in the regulation of bodily movement.[20] Causal accounts of this sort play much the same role as explicit derivations. Both show in detail how some fact to be explained depends in a systematic way on a set of explanatory factors and principles.

There is a second, related feature of systematic explanation in science which is emphasized both by scientists themselves and by philosophers like Michael Friedman, Philip Kitcher, and Clark Glymour.[21] This is the idea that good explanations in science should unify and connect a range of different kinds of explananda. A characteristic kind of advance in scientific understanding occurs when one sees how what previously seemed to be a number of independent, unrelated facts can be accounted for in terms of a small set of common mechanisms or laws. Nineteenth-century optical theories represented an important explanatory achievement because they provided a unified, systematic account of a wide range of optical phenomena involving reflection, refraction, diffraction, stellar aberration, and polarization in terms of a few basic assumptions regarding the transverse wave character of light. Similarly, Maxwell's theory provided a unified treatment of an apparently diverse set of electromagnetic phenomena. More recently, a similar drive toward unification of the four fundamental forces is a conspicuous feature of theorizing in contemporary high energy physics: thus the Weinberg-Salam theory, referred to above, represents an attempt to unify the electromagnetic and weak forces, and more recent grand unified theories attempt to unify the strong and electroweak forces.

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[20]For a brief introductory sketch, see E. R. Kandel and J. Schwartz, *Principles of Neural Science* (New York, N.Y.: Elsevier, 1981), pp. 352ff.

[21]Michael Friedman, "Explanation and Scientific Understanding," *Journal of Philosophy* 71 (1974), pp. 5–19; Philip Kitcher, "Explanatory Unification," *Philosophy of Science* 48 (1982), pp. 507–531; and Clark Glymour, "Explanation and Realism," in *Scientific Realism*, ed. Jarret Leplin, (Berkeley, Calif.: University of California Press, 1984). Among scientists the following remarks from Peter Watkins's *Story of the W and Z* are typical:

> A common aim of all science is to explain as many facts as possible with a few simple principles or ideas. This leads to efforts to relate apparently different phenomena wherever possible and in some cases a unification is achieved (p. 43).

V.

We have argued that data are far more idiosyncratic than phenomena, and furthermore, that their production depends upon highly irregular coincidences involving a great number of different factors. It follows that explanations of data, when they can be given at all, will be highly complex and closely tied to the details of particular experimental arrangements. As we vary the method used to detect some phenomenon, and other details of the experimental design, the explanation we must give of the data will also vary, often in rather fundamental ways. Thus, explanations of data will often lack generality and will fail to satisfy requirements having to do with theoretical unification. Moreover, the factors involved in the production of any given bit of data may be so disparate and so numerous, and their co-occurrence so rare, that the details of their interaction may be both epistemically inaccessible and difficult to model theoretically. Exhibitions of dependency-relations of the sort that would be achieved by explicit derivations or the tracing of specific causal mechanisms may prove impossible because of computational intractabilities. In short, it will often not be feasible to provide explanations of data satisfying the requirements on systematic explanation outlined above.

In undertaking to explain phenomena rather than data, a scientist can avoid having to tell an enormous number of independent, highly local, and idiosyncratic causal stories involving the (often inaccessible and intractable) details of specific experimental and observational contexts. He can focus instead on what is constant and stable across different contexts. This opens up the possibility of explaining a wide range of cases in terms of a few factors or general principles. It also facilitates derivability and the systematic exhibition of dependency-relations. In short, facts about phenomena are natural candidates for systematic scientific explanation in a way in which facts about data are not.

VI.

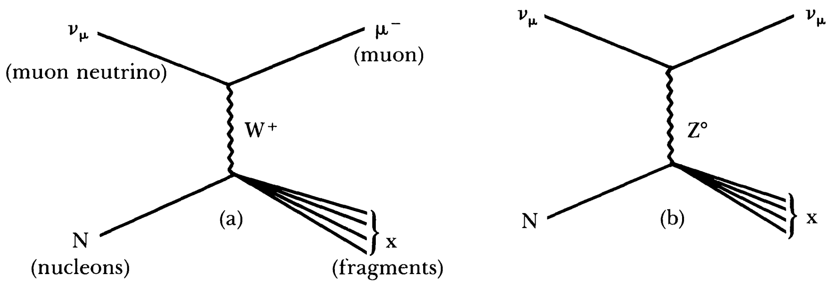
We turn now to an important objection, adumbrated above, to our claim that theories do not provide systematic explanations of facts about data (Cf. O2, Section II). The objection is that the only way to assess the reliability of data as evidence is to construct a theory which systematically explains that data or traces in detail the causal processes by which it is produced. According to this objection, one would have no justification for believing that bubble chamber photographs are reliable evidence for the existence of various subatomic particles unless one was in a position to construct a systematic explanation, in the sense described above, of facts about those photographs.

Our response to this objection is that it is simply false that an assessment of the reliability of data requires the construction of systematic explanations of facts about such data. To show this we will describe in detail several examples which illustrate some characteristic considerations relevant to the assessment of reliability.[22] As these examples suggest, the question of whether data constitute reliable evidence for some phenomenon turns (among other things) on such considerations as whether the data are replicable, whether various confounding factors and other sources of possible systematic error have been adequately controlled, on statistical arguments of various kinds, and on one's procedures for the analysis and reduction of data. In large measure, these considerations are distinct from a concern with explanation. They are also largely ignored in most traditional accounts of theory-testing.

1. *Control of possible confounding factors.* Confounding factors are factors which can produce data similar to that which would be produced by the phenomenon of interest and thus yield spurious

[22]The brief list of considerations which follows is certainly not meant to be exhaustive. Historical studies of experimentation, such as the studies included in Allan Franklin's *The Neglect of Experiment*, contain accounts of a number of other strategies for assessing reliability in the absence of systematic explanations of data, although they are typically not described in just these terms. For example, one important strategy which is not discussed above involves what Franklin calls "calibration." The fact that one's apparatus can reproduce familiar phenomena with known characteristics is often an important piece of evidence that the apparatus is functioning reliably and that various unwanted kinds of background noise and confounding factors are absent. Here too, there is an obvious difference between this strategy and attempting to ensure reliability by constructing a systematic explanation of the behavior, of one's apparatus, or by attempting to deal with possible background and confounding effects by derivation or calculation. Given the susceptibility of most pieces of complex apparatus to unpredicted and unexplained glitches and breakdowns, and the variety and complexity of possible background and confounding factors, the greater utility of the strategy of calibration is obvious.

candidates for that phenomenon. We also include under this heading factors which introduce so much noise into the data that it becomes unusable as evidence. Controlling for such factors is central to establishing reliability. A striking example is provided by the experiment at CERN, referred to above, in which neutral currents were detected.[23] The existence of neutral currents—weak interactions mediated by the neutral Z° particle—is a distinctive prediction of the Weinberg-Salam theory, which unifies the weak and electromagnetic forces. The detection of neutral currents was thus a matter of intense scientific interest. The reactions which were of primary interest to the group at CERN were:



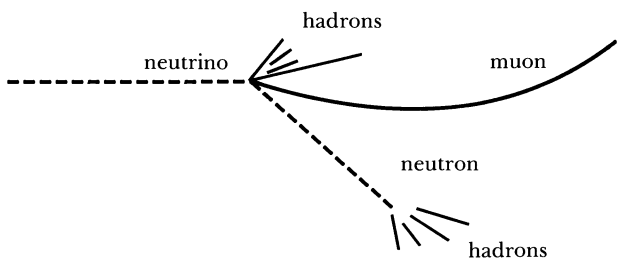
(a) is a charged current interaction. It involves a change in charge, since the charge of the incoming lepton (the neutrino), which is neutral, changes to negative (the muon). The reaction in (b) involves a neutral current, since a neutrino rather than a muon emerges and there is no charge change. In contrast to earlier theories, the Weinberg-Salam theory predicts that interactions of kind (b) as well as kind (a) should occur and, indeed, predicts a definite ratio for these two kinds of occurrences.

While charged particles like muons leave tracks in bubble chambers, electrically neutral particles like neutrinos do not. Thus for both charged and neutral current interactions, a photograph will not show the incoming neutrino. Instead it will show a short

[23]For our discussion of this example we have relied very heavily on Peter Galison, "How the First Neutral-Current Experiments Ended" and Andrew Pickering, "Against Putting the Phenomena First: The Case of the Weak Neutral Current."

shower of tracks left by the strongly interacting particles (X) produced when a neutrino strikes a nucleon. However, the charged current process in (a) exhibits, in addition to this shower, the long straight track of a high energy muon, while the outgoing neutrino in (b) leaves no such track. The identification of a weak neutral current interaction depends, accordingly, on the absence of a muon and upon one's ability to rule out various other possible causes of the interaction besides an incoming neutrino. Thus, detecting the presence or absence of muons is crucial to distinguishing neutral from charged currents.

This feature of the experiment raised in turn the following interpretive difficulty: it was known that when neutrinos from the incoming beam strike the chamber and the surrounding apparatus, they produce a large, but unknown, number of neutrons. If one of these hits a nucleon, the resulting shower of hadrons will mimic a neutral current event. No muon will be produced and, because it is chargeless, the neutron will leave no track.



Hadrons with no muon look like a neutral current event.[24]

The neutron background is a clear case of a confounding factor —a factor which mimics the data which would be produced by genuine neutral currents. To show that they had detected neutral currents, the CERN experimenters had to show that this background was not by itself large enough to account for all the apparent neutral current events. The magnitude of this background could not be derived with confidence from fundamental theoretical principles for several different reasons. First, the mag-

[24]Reproduced from Galison, "How the First Neutral-Current Experiments Ended," fig. 8, p. 484. Used with permission.

nitude of the background depended on characteristics of the incoming neutrino beam and the surrounding equipment which were neither directly measurable nor fully understood. Second, such a derivation would have required a theory of strong interactions which was not available at the time of the experiment.

The difficult and controversial nature of the background problem is suggested by the extensive disagreements and changes of mind among participants in the experiment about the best way of dealing with the problem. One general line of attack was to attempt to estimate the size of the background using various Monte Carlo simulations and thermodynamic arguments. Here the idea was to establish, via the convergence of different assumptions and estimating procedures, an upper bound on the size of the background. Doing this helped to persuade many (but not all) of the experimentalists that the neutron background could not be causing all the candidates for neutral currents they were detecting. A second kind of consideration focused instead on the location of putative neutral current events within the chamber. For theoretical reasons, it was expected that neutron-induced events would occur more frequently near the walls of the chamber. When plausible candidates for neutral currents began turning up in the center of the chamber (and indeed relatively uniformly through most of its volume), this was an additional consideration in favor of the claim that the experimenters were detecting genuine neutral currents.

There are several general remarks we want to make about this example. Note first that the concern about reliability posed by the problem of the neutron background is not dealt with by constructing a systematic explanation of that background. To construct such an explanation one would have required information, both about the passage of strongly interacting particles through matter and about various parameters characteristic of the equipment employed, which was simply not available. Instead the problem of reliability is dealt with by the use of various estimating procedures, which are not intended to provide explanations. Each of these is regarded as individually problematic, but they converge sufficiently to set an upper bound on the background. The problem of reliability was also dealt with by the exploitation of various physical features of the apparatus. To make use of the fact that neutrons are relatively unlikely to penetrate into the center of the chamber, one need not know what the size of the background is, let alone be able to explain it.

Second, the Monte Carlo simulations were designed to rule out the possibility that *all* candidates for neutral current events were due to the neutron background, or that the ratio of neutral to charged current events was significantly different from the expected ratio. To rule out these possibilities is not at all the same thing as to establish conclusively that a given piece of data—the 38th photograph in a series, say—does not represent a neutron-induced event. Nor is it the same thing as explaining the image in any particular photograph. This illustrates how the acceptability of the techniques used to deal with the neutron background did not depend to any significant extent upon the ability to explain individual pieces of data.

As another illustration of the difference between explaining data and insuring that it is reliable, consider the research on solar neutrinos described above. The isotope of argon that is used in the detection of solar neutrinos can be produced by all sorts of irrelevant background radiation of various kinds, including cosmic rays. Instead of attempting to deal with these potentially confounding causes by the construction of a detailed causal theory of the processes involved in the production of the data, the investigators conducted their experiment far enough beneath the surface of the earth to provide shielding from much of this background radiation. If the experiment had been conducted instead on the surface of the earth, then the irrelevant background radiation would have been so extensive that reliably correcting for its influence by calculation would have been a practical impossibility, even given a theoretical understanding in principle of the sources and character of such radiation. As this example illustrates, the best strategy for achieving reliability is often to rule out possible confounding factors by the design of one's apparatus, rather than by attempting to achieve a detailed theoretical understanding of their influence.

2. *Empirical investigation of equipment.* Another common strategy for ruling out artifacts is to investigate empirically the reliability of instruments or detection devices. This may make it look as though the establishment of reliability requires an explanation of how the instrument figures in the production of data. But in fact there are many cases in which one can investigate empirically the reliability of equipment without possessing a systematic explanation of how

the equipment functions or how data is produced. Ordinary visual perception furnishes an obvious illustration. The functioning of the human visual system is just beginning to be understood.[25] We certainly did not in the past possess a full explanatory theory of its operation. But no such explanatory theory is needed to investigate the conditions under which visual perception is reliable. In the example described in Section I involving the detection of protons and alpha particles by observation of scintillations, the researchers at Cambridge empirically investigated the performance of human observers under various conditions. They discovered that under laboratory conditions the observers employed at Vienna continued to report the scintillations thought to be characteristic of protons even when the supposed source was fully shielded.[26] This is dramatic evidence for unreliability. No detailed explanatory theory of the human visual system or of why the observers at Vienna made mistaken reports was needed to establish this conclusion.

A second illustration of the same point is provided by Ian Hacking in his recent paper "Do We See Through A Microscope?"[27] Prior to Ernest Abbe's work in 1873, it was not generally understood how an ordinary optical microscope works. Abbe showed that, contrary to what was commonly believed, diffraction plays an important role in the production of the image—that the image is due to interference of the transmitted and diffracted rays. However, as Hacking rightly remarks, the unavailability of a correct theory of how microscopes work did not undermine the reliability of observations made with microscopes prior to Abbe. One can obtain evidence that a microscope provides reliable evidence by checking to see if the same microscopic objects are detectable

[25] S. Coren, C. Porac, L. Ward, *Sensation and Perception* (Orlando, Fla.: Academic Press, 1984) provides a reliable summary of the present state of neurophysiological and psychological investigations of the senses. Crudely put, the anatomical and physiological stories are far from complete, crucial functions remain to be characterized adequately, and even the most promising work in cognitive psychology which attempts to fit the constraints of what neuroscientists have found tends to be inadequate to the evidence provided by empirical psychological studies. See, for example, the authors' assessment of David Marr's account of brightness constancy, p. 415; the difficulties they point to in squaring the account with the evidence are depressing, but far from unusual.

[26] Steuwer, "Artificial Disintegration and the Cambridge-Vienna Controversy," pp. 273ff.

[27] Hacking, *Representing and Intervening*, p. 209.

via different physical processes. One can also determine whether manipulation of a specimen produces the expected changes in the image. Similarly, if one is worried that an apparent feature of a specimen is only an artifact of a staining procedure, one can use different stains, or look for the feature in question on an unstained specimen, or observe whether the feature behaves in expected ways when one attempts to manipulate it. None of these strategies requires possession of a detailed explanatory theory of how the microscope or the chemistry of the staining procedure works.

3. *Data-Reduction and Statistical Analysis.* Procedures for data-reduction and statistical analysis are also crucial to the assessment of reliability, but have little to do with the construction of explanations. Bubble chambers, for example, produce an enormous amount of data, of which only a very small portion will be relevant to the phenomenon one is trying to detect. (Recall that out of 290,000 photographs, roughly 100 were regarded by the group at CERN as candidates for neutral current events.) Hence the need for fast and reliable procedures for searching through a mass of data for items of potential theoretical interest. To deal with this "data bottleneck," relatively untrained personnel are frequently employed to recognize potentially interesting photographs. In addition, it is increasingly common to employ computer programs to fit hypothetical trajectories to tracks appearing in those photographs and to make tentative identifications of interactions of interest. The extent to which such methods of data-reduction are independent of any concern with explanation is illustrated by the fact that the person or machine performing these tasks can carry them out without understanding either the theory which explains the interactions for which the photographs are evidence, or the physical principles by which the equipment works. Data-reduction aimed at isolating and analyzing relevant data does not require explanation of that data, even though it may be essential to establishing reliability.[28]

[28]For a fascinating discussion of the evólution of procedures for data-reduction in connection with bubble chambers, see Peter Galison, "Bubble Chambers and the Experimental Workplace." For a description of the procedures for data-reduction used in connection with the discovery of the W and Z particles, see Peter Watkins's *Story of the W and Z.*

A simple illustration of the use of statistical analysis in connection with reliability is provided by the case of determining the melting point of lead. The rationale for the use of the mean of the thermometer readings as an estimate of the melting point involves various general assumptions regarding the causal mechanisms underlying the production of the data—for example, that the sources of error operate randomly and independently. But the details of the operation of these causes will be both unknown and enormously complex, so that typically there will be no possibility of explaining why this or that individual data-point occurs. Moreover, one does not *need* to be able to do this to obtain a reliable estimate from the data. A similar point is true in many other cases in which statistical techniques are used to make inferences from noisy data.

We conclude this discussion of reliability with an observation which we hope to discuss in more detail elsewhere. It is a matter of historical fact that theories specifying fundamental explanations or basic causal mechanisms often shift dramatically over time. If the determination of whether bodies of data constitute reliable evidence for various phenomena depended upon our possession of correct and detailed accounts of the causal mechanisms which produce the data, claims about phenomena would be far more fragile than they actually are. Significant changes in explanatory theory would frequently undermine the evidential status of data, and the acceptability of procedures and assumptions used in data-analysis and experimental design. In fact, this seldom happens. Because our account treats the assessment of reliability as largely independent of the construction of systematic explanations, it can explain why inferences to the existence of phenomena, and the procedures used to analyze data and establish reliability, are often robust enough to endure through fundamental changes in explanatory theory. In this regard, our view fits the history of science and accounts for an important source of continuity and stability in science, better than views which conflate the determination of reliability with the explanation of data.[29]

[29]The discovery of the weak neutral current is an obvious example. Establishing the existence of this phenomenon did not require causal explanations of the data. Moreover, it is unlikely that subsequent theoretical developments will show that the experimental design and techniques of

VII.

We have been arguing that general theories in science are required to explain facts about phenomena rather than facts about data and that the assessment of the reliability of data as evidence does not require systematic explanation of facts about data. Further support for these contentions is provided by consideration of cases in which available data is not evidence for the existence of any phenomena. For example, parapsychologists have accumulated data which they claim constitutes good evidence for the existence of phenomena like telekinesis and telepathy. Critics have argued that the experimental designs employed by parapsychologists do not eliminate the possibility of certain kinds of fraud, or reliance on unconscious cuing, or other "natural" sources of information. They have also pointed to defective statistical techniques and to inadequate randomization procedures that do not eliminate chance-effects or experimenter bias. Many critics have emphasized that when rigorously controlled experiments are performed, parapsychologists are unable to replicate their results reliably. As the apparent sensitivity of the measuring procedure is increased, the "signal" they are trying to detect gets weaker.[30]

Such criticisms of the reliability of parapsychological data do *not* turn on the provision of detailed systematic explanations of the data. Typically, the critic is not in a position to establish that, for example, fraud or unconscious cuing have actually occurred, let alone to show in detail how they were accomplished in various particular experiments. Instead, the critic establishes that these and other possibilities have not been adequately controlled for or decisively ruled out. Similarly, criticisms which focus on statistical deficiencies do not establish that certain claimed effects are artifacts

data-analysis employed in these experiments were radically misconceived. Any subsequent theory of the weak interactions will need to account for the phenomena associated with weak neutral currents.

[30]Richard Feynman takes this to be one of the characteristic features of pseudo-science. See *Surely You're Joking Mr. Feynman* (New York, N.Y.: W. W. Norton, 1985), pp. 308ff. For related criticisms, see Martin Gardner *Science—Good, Bad and Bogus* (Buffalo, N.Y.: Prometheus Books, 1981); P. Diaconis, "Statistical Problems in ESP Research," *Science* 201 (1978), pp. 131–136; and Dael Wolfle, "Editorial on Extra-Sensory Perception," *Science* 123 (1956), p. 7.

which are due to chance. They merely establish that this possibility cannot be confidently rejected. Still less do they exhibit the particular constellation of factors which combined to cause this or that particular bit of data. Similarly, to show that the results of parapsychological experiments are not consistently replicable is not to explain those results or to explain why replicability fails.

The critics' failure to provide systematic explanations of the data in parapsychological experiments has been seized on by some who are sympathetic to such research. Writers like Trevor Pinch argue that while parapsychologists at least have candidates for unified, systematic explanations of their data, their critics offer no alternative, similarly unified explanation.[31] The critics are accused of postulating what are (at best) ad-hoc piecemeal explanations of parapsychological data in terms of a number of disparate causes (fraud, unconscious cuing, chance effects) for which there may be no independent evidence and of often failing to do even this. If the point of doing science was to explain the data, or if showing whether data are reliable required providing detailed explanations of the data, this defense of parapsychological research would have considerable plausibility. By contrast, our view is that until the existence of parapsychological phenomena is established through well-designed, replicable, and otherwise reliable experiments, there is nothing which requires systematic explanation. It is perfectly legitimate, methodologically, to claim that because of a wide variety of largely unrelated defects in experimental design and data-analysis, parapsychological experiments do not provide reliable evidence for parapsychological phenomena. The skeptic can make this claim while cheerfully admitting that he does not know the details of the causal processes by which the unreliable data was produced. Only phenomena require systematic explanations which are not piecemeal or ad-hoc. The parapsychologist's demand that his *data* be explained in this way is misguided. This example illustrates how a failure to distinguish data from phenomena, and the idea that theories must explain the data, can have pernicious methodological consequences.

[31]See, for example, Trevor Pinch, "Normal Explanations of the Paranormal: The Demarcation Problem and Fraud in Parapsychology," *Social Studies of Science* 9 (1979), pp. 329–348.

VIII.

Assuming that the claims made in the preceding section are correct, what are their implications for scientific realism and for the popular idea that inductive inference is a matter of inference to the best explanation? In this section we comment very briefly on these matters. Throughout our discussion, we have described phenomena (and data) in realistic-sounding language: as objects and processes which occur in nature, regarding which we can come to have justifiable and true beliefs. But although we are indeed realists about phenomena, we do not regard our discussion as an *argument* for realism, at least in any very direct way. Our discussion is rather intended as an empirical description of various features of scientific practice that have been overlooked or misdescribed in the philosophical literature. We describe those features within a realistic framework, but whether these features *must* be described or interpreted in this way, if we are to make sense of them, is very much an open question in our view.[32] For all we have shown, there may be versions of anti-realism which will allow the anti-realist to agree that scientific practice exhibits something like our distinction between data and phenomena, and then go on to show that this fact about scientific practice is ontologically non-committal; that it does not require or support realism about phenomena. Our claim is merely that anti-realists must either provide a plausible anti-realist gloss on the distinction between data and phenomena and the different role assigned to each or they must show that our claims about the role of this distinction in scientific practice are empirically mistaken.

The second issue we want to take up has to do with the connection between explanation and evidential support. We have argued that often one can assess the reliability of data even in the absence of a systematic explanation of why the data obtain. It might seem that this commits us to denying the common doctrine that all or at least a great deal of inductive inference can be represented as "in-

[32]We argue below (Section XIII) that one specific version of anti-realism—the version defended in Bas van Fraassen's *The Scientific Image* —does appear to be inconsistent with our empirical claims about the role of the distinction between data and phenomena in science.

ference to the best explanation." Whether this is so depends on exactly how the doctrine in question is understood. One version of the doctrine goes roughly as follows. The notion of explanation is interpreted in such a way that a claim that some single factor has played a casual role in the production of an outcome constitutes a potential explanation (a "singular causal explanation" cf. note 20) of that outcome, even when the other causal factors that played a role in the outcome are left unspecified. Given a number of competing causal claims of this kind, establishing that one of these is the "best explanation" of the outcome requires showing that the claim in question is true and that its competitors are false, or at least that the claim is more plausible than its competitors.

The first version of the doctrine of inference to the best explanation claims that if E is to be evidence for hypothesis H, it must be the case that H figures in the best explanation of E, where the notion of best explanation is understood along the lines just described. Thus it might be claimed, for example, that if certain bubble chamber photographs are evidence for the occurrence of neutral currents, the occurrence of neutral currents must figure in the explanation of (in this case, must be a causal factor in the production of) those photographs. Relatedly, it must also be the case that certain competing causal explanations of the photographs—for example, that they are caused by background neutrons—can be ruled out. We think that this version of the doctrine that there is a connection between evidential support and explanation is probably correct, and that nothing we say above is inconsistent with it.

This first version of the doctrine should be distinguished from a second version which is in many ways stronger and more ambitious. The second version takes the potential explanatory power of a hypothesis to be by itself a *reason*, independent of whatever other evidence one might possess, for accepting the hypothesis as true or as supported by a body of evidence. Unlike the first version, the second version thus requires the assessment of competing explanatory hypotheses according to criteria for explanatory goodness other than truth or falsity. These criteria will be prescribed by some antecedent theory of explanation. They will presumably include the sorts of criteria described in Section IV and perhaps others as well: generality, systematic unity, simplicity, detailed exhibition of dependency relations and so forth. According to this second version one begins with a body of evidence E and a set of alternative hypotheses $H_1 \ldots H_n$ which are potential explanations of E. (We assume that it is built into the notion of potential explanation that none of $H_1 \ldots H_n$ is known to be false or is significantly undermined by current evidence.) One wants to know which of these hypotheses is best supported by E, or which (if any) is worthy of belief as true, given E. One proceeds by asking which of these hypotheses would, *if true,* provide the best explanation of E, where the criteria for explanatory goodness take the form described above. If, say, H_1 would provide the best explanation of E, one takes this fact to provide a reason for belief in H_1 or to show that H_1 is the hypothesis which is best supported by E. One does not, as in the previous version, *first* provide reason to think that one of the hypotheses $H_1 \ldots H_n$ is true or plausible and that its competitors are false and *then* take this fact to show (or at least to be a necessary condition for showing) that this hypothesis provides the best explanation of E. Rather, the idea is that even in the absence of any other evidence that the hypothesis is true and its competitors false, one is justified in taking it as true or well supported, just because it scores so highly on other dimensions of explanatory assessment which are independent of truth. Clark Glymour captures this idea very nicely:

> [T]here are many, many cases where there is little or no external evidence for the claims that go into an explanation, and the claims are argued to be worthy of belief exactly because they explain so well. Because they are, indeed, so explainy.[33]

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[33]Clark Glymour, "Causal Inference and Causal Explanation" in *What? Where? Why? When?: Essays on Induction, Space and Time, Explanation,* ed. Robert McLaughlin (Dordrecht, Holland: D. Reidel Publishing Company, 1982), p. 186. For additional defenses of something like this notion of inference to the best explanation see Paul Thagard, "The Best Explanation: Criteria for Theory Choice," *Journal of Philosophy* 75 (1978), pp. 76–92 and John Watkins, *Science and Scepticism* (Princeton, N.J.: Princeton University Press, 1984).

A few additional remarks by way of clarification of the distinction between the two versions of inference to the best explanation may be helpful here. The first version is a thesis about the truth conditions for the claim that E is evidence for hypothesis H. As such the first version does not purport to specify a strategy or set of grounds for telling whether E is evidence for H or whether H figures in the explanation of E. The idea underlying the first version is merely that whatever the appropriate

The question of whether the second version of inference to the best explanation is ever a legitimate strategy for choosing among hypotheses or assessing evidential support is a controversial one. All that we wish to claim here is that in the examples described in Section VI, data are not shown to be reliable or unreliable evidence for claims about phenomena by means of the strategy described in the second version, at least not when the relevant criteria for explanatory goodness are understood along the lines of Section IV. The considerations which were taken by the experimentalists at CERN to show that certain bubble chamber photographs provide reliable evidence for the existence of neutral currents did not consist in the exhibition of a potential systematic explanation of those photographs, and an argument that this

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strategy or grounds may be, if it is indeed true that E is reliable evidence for H, it must also be the case that H figures in the "best explanation" (in the sense specified in the first version) of E. By contrast, the second version does purport to describe a distinctive inductive strategy or a distinctive set of grounds for assessing evidential support. The idea is that these grounds will have to do with whatever criteria, other than truth, which we think are relevant to the assessment of explanatory power. Someone who holds that all inductive inference is a matter of inference to the best explanation in the second sense rules out the possibility that one might justifiably come to believe that E is evidence for H on the basis of considerations that do not have to do with how well H would, if true, explain E. The belief that all inductive inference involves inference to the best explanation in the first sense has no such consequence.

The need to distinguish these two versions of inference to the best explanation also becomes apparent when one considers recent writing on the subject. First, several writers (most notably Nancy Cartwright in *How the Laws of Physics Lie* and Ian Hacking in *Representing and Intervening*) have expressed skepticism about whether inference to the best explanation is ever a legitimate inductive strategy. These writers, as we understand them, are attacking the second version of the doctrine: the idea that the fact that a hypothesis, if true, would explain well is an independent reason for belief in its truth. They do not attack the first version of the doctrine, according to which it is understood as a claim about truth conditions. Second, a number of writers have found the claim that all inductive inference is inference to the best explanation plausible. We think that these writers have typically had in mind the first, rather than the second version of inference to the best explanation. Those who seem to have in mind the second version of the doctrine typically have not found this claim plausible. Writers like Glymour or Thagard seem to claim at most that inference to the best explanation is *a* legitimate inductive strategy or provides one kind of legitimate ground for belief among several, and not that all grounds for belief must take this form.

potential explanation, would, if true, provide a better explanation of the photographs than any alternative. More generally, in the above examples many of one's grounds for belief in the evidential status of the data do not seem naturally to be represented as considerations which have to do with the assessment of competing systematic explanations of that data, although it may very well be true that (as remarked above) to come to believe, on the basis of those grounds, that such data are reliable evidence for some phenomenon is to commit oneself to the claim that the phenomenon must figure causally (typically in some very complex way, the details of which remain unknown) in the production of the data.[34] To the extent

[34]It is also worth noting that while it may be true that a necessary condition for E to be evidence for H is that a factor described in H figures as a cause in the production of E, this condition is clearly not sufficient. Even if H does genuinely figure in the explanation of E, other sources of systematic error may contribute to E in such a way as to make E unreliable as evidence. For example, a microscope may produce a distorted image of a specimen, even when the specimen figures causally in the production of the image. Similarly, even when H figures in the explanation of E, so much additional background noise or so many potential confounders may be present that E is unusable as evidence. It is also worth noting that even if it is correct that if E is evidence for H, then H must figure in the explanation of E, one need not know or rely on facts about the actual explanation of E in order to assess the evidential status of E. For example, as noted above, the absence of replicability and deficiencies in the design of many parapsychological experiments are enough to undermine the claim that the data from such experiments is evidence for parapsychological phenomena, even if one does not know a correct singular causal explanation of the parapsychological data. Similarly, as explained above (Section V), one can have good grounds for belief in the reliability or unreliability of instruments or detectors like microscopes, bubble chambers, or the human visual system, and in the data these produce, even if one holds seriously false beliefs about the causal operation of such devices. Finally, we should also note that many issues which arise in connection with the analysis and interpretation of data—for example, issues having to do with data-reduction, or with procedures for aggregating data, or with when it is appropriate to discard data—do not seem to be represented naturally as issues about the assessment of explanations. For all of these reasons, while there may well be a general connection between evidence and causal explanation of the sort described above, it seems to us that an account which focuses just on the fact of this connection is likely to overlook many of the considerations which are relevant to assessing whether a body of data provides reliable evidence, and to yield at best a partial description of this aspect of scientific practice. The unexceptionable general point that if certain bubble chamber photographs are to constitute evidence for neutral currents, the latter must figure causally in the

that the doctrine of inference to the best explanation is taken to mean that all legitimate inductive inference must take the second form described above, the doctrine seems mistaken.

IX.

The remaining objection to be considered from Section II was (O1) that the traditional account, supplemented by the last thirty years or so of literature on observation, does give an accurate characterization of what scientific theories explain and predict. Even if we are right to think that data cannot be systematically explained —the objection begins—saying that *observations* are explained need not commit one to the view we have argued against, that *data* are explained. Instead, we should recognize that phenomena, as well as data, can be observed. In light of this, it is entirely appropriate to think of scientific theories as explaining facts about what we observe. According to Kuhn, "when Aristotle and Galileo looked at swinging stones, the first saw constrained fall, the second, a pendulum." If this were true, then Galileo's observational claims would have reported features of the motion of a pendulum, while Aristotle's would have reported features of forced motion. According to Hanson, a microbiologist looking at a specimen through a microscope may make an observation-report about ". . . a cell organ, a Golgi body." Feyerabend believes that theory-laden observation-sentences may report the relative velocity of an observer and a source which emits light of a certain wavelength. Dudley Shapere and Ian Hacking think we can observe the inside of the sun.[35]

The important feature of these examples is not the idea that

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production of the former tells one surprisingly little about the considerations which are relevant to assessing whether such data is good evidence for various claims about neutral currents or about the features which such data must possess.

[35]Thomas Kuhn, *The Structure of Scientific Revolutions*, second edition (Chicago, Ill.: University of Chicago Press, 1970), p. 121; Norwood Hanson, *Patterns of Discovery* (Cambridge, England: Cambridge University Press, 1958), pp. 4, 17; Paul Feyerabend, *Realism, Rationalism, and Scientific Method*, Vol. I (Cambridge, England: Cambridge University Press, 1985), p. 29; Dudley Shapere, "The Concept of Observation in Science and Philosophy," p. 488; Ian Hacking, *Representing and Intervening*, p. 182.

conflicting observational claims can be made from the standpoint of conflicting theories. Rather, what is important is that Kuhn et al. think of observational claims as reporting facts about phenomena—the very sorts of facts we think that scientific theories do predict and explain. And so—the objection concludes—if what we say about the traditional account is meant to apply to those who believe observational claims are theory-laden, then what we say is either incorrect or trifling. If our view is based on the assumption that only data can be reported by observational claims, we are wrong because the assumption is false. On the other hand, if our position merely represents a preference for calling what scientific theories explain "phenomena" rather than "observations," our rejection of the traditional picture is just a matter of terminology.

Our reply to this objection is that if "observation," "observation-sentence," and related terms are given a definite enough interpretation to make the traditional view a substantive characterization of scientific activity, then phenomena for the most part cannot be observed and cannot be reported by observational claims. In order to support the contention that phenomena are observed, terms like "observation" and "observation-sentence" must be used too vaguely to say anything informative about science.

X.

In most philosophical discussions, the notion of observation is closely tied to perception by the human sensory system. This is obvious enough in the case of the early positivists, for whom the subjects of observational reports were either sense-data or familiar, medium-sized physical objects like thermometers and ravens. But it is equally true of more recent writers like Kuhn, Feyerabend, and Hanson who stress the theory-ladeness of observation: they too emphasize the influence of theoretical expectations or preconceptions on perception and the close analogy between the processes underlying the operation of the human perceptual system and the processes underlying the operation of instruments like telescopes and microscopes.

Most accounts of observation-sentences as theory-laden construe what is perceived along the lines of "New Look" perceptual theories. What is perceived, according to such theories, is a distal stimulus which causes a sensory event. Observation-sentences are taken to report features of this distal stimulus. The vocabulary of an observation-sentence is provided by a theory which explains the sensory event. Such a theory identifies the role of the distal stimulus in the production of the sensory event and characterizes the distal stimulus in terms of this role.[36] Roughly the same account is provided when instruments and machines take the place of human observers. The analogue of a perceptual event is an event produced in the apparatus by a distal cause, described in terms of a theory which appeals to that cause to explain what happened in the apparatus.[37] What we would call a claim about phenomena—a claim about the flux of solar neutrinos, for example—would then be an observational claim for someone who accepted an explanatory theory of a certain kind. In this case, the required theory would explain the pattern of splodges on the screen, or the seeing of those splodges, and would characterize the distal cause (or stimulus) as neutrinos or the neutrino flux.

[36]For a representative statement of the "New Look" theory, see Richard Gregory, *Concepts and Mechanisms of Perception* (London, England: Duckworth, 1974). For a helpful, sympathetic, and highly informative account of connections between the notion of theory-laden observation-sentences and "New Look" theories, see Peter K. Machamer, "Essay Review: Understanding Scientific Change," *Studies in the History and Philosophy of Science* 5 (1975), pp. 373–381; and "Observation," in R. Buck and R. Cohen, eds., *Boston Studies in the Philosophy of Science*, vol. VIII, (Dordrecht, Holland: D. Reidel Publishing Company, 1971). Our presentation follows Machamer's with two exceptions: we think (Section XI) that Kuhn need not be committed to the view that observations can be explained by the theory which loads reports of those observations. And, as is obvious, we are far less sympathetic to the view than Machamer.

[37]The notions of proximal and distal stimulus and cause are not terribly well marked off. The underlying idea is to distinguish happenings in the sensory system (or instrument) which cause the perceptual event (or its analogue in the case of an instrument), and causes outside the system (instrument) which produce those happenings. Intuitively, light interacting with retinal cells is more proximal, and the object which emits or reflects that light is more distal. The temperature of a mercury column, is still more distal, if (by contributing to the height of the column and, thereby, to the patterns of light it reflects), it should be considered a stimulus at all. Although the distinction seems intuitive and convenient, anyone who attempts to identify the distal and proximal stimuli for a given perceptual event is liable to find himself wandering through a grey area in which little guidance is provided by neuroscience and perceptual psychology, as these subjects stand now.

Our view is that few, if any, claims about phenomena qualify as observational on this account. In the case of human observers, the distal stimuli for sensory events from which phenomena are detected are seldom, if ever, the detected phenomena themselves. Furthermore, although theories which describe the detected phenomena may show how those phenomena make a causal contribution to the processes which eventually lead to the sensory event, they seldom actually explain the occurrence of the sensory event. For example, the flux of solar neutrinos plays a relatively well understood causal role in the production of the isotope of argon which in turn produces splodges by means of interaction with a Geiger counter. But the sensory events by which the phenomenon of the flux is detected are seeings of splodges or seeings of graphs of splodges. Although the notions of proximal and distal stimuli are notoriously difficult to pin down, these notions would be of no use whatsoever to the explanation of perceptual events if they allowed us to call the neutrino flux the distal stimulus involved in the seeing of splodges. The neutrino flux has to do with the production of the isotope of argon which is eventually swept out of the cleaning fluid and presented to the Geiger counter. The neutrino flux does not itself determine what light reaches the experimenter's eye as he looks at the screen. The neutrino flux is not what the experimenter is looking at. The distal stimuli for the sensory events involved in the seeing of splodges occur on the screen. If we wish instead to think of the screen as analogous to a visual system and the occurrence of splodges as analogous to perceptual events, the proximal stimulus will be found in the mechanism of the screen and the distal stimulus will be found in the Geiger counter or the radiation from the isotope of argon. There is no perceptual theory according to which the neutrino flux could serve as a distal stimulus for the seeing of splodges.[38] But the neutrino flux is the phenomenon the apparatus is used to detect.

[38]Recall that neutrinos, unlike electromagnetic radiation, interact only very weakly with matter. Most neutrinos (including those emitted by the sun) pass not only right through the observer's eye, but through the earth itself. Because of this, it is hard to imagine what the biology or evolutionary history of a creature whose perceptual system could detect neutrinos would be like. It is also easy to see why our biology and evolutionary history do not provide us with sensory systems capable of doing this.

Of course the distant causes of the splodges include interactions involving solar neutrinos. That is why the experimental set-up can be used to detect the flux from the sun. But to recognize that X played a part in causing Y or to recognize that Y was produced by causes which include X is not to see X. That is why you cannot see our grandfathers, or the midwives who delivered them, by looking at us. No matter how much you know about where we came from, they are not distal stimuli and they are not what you look at when you look at us. This is our first objection to the identification of claims about phenomena with theory-laden observational claims: even if observation-sentences describe the distal causes of perceptual events or the distal causes of events in which data register on instruments, they do not thereby describe phenomena. Moreover, as we have already insisted at some length, it is wrong to think that we possess a theory about neutrinos, or any combination of theories, which explains, at least in any very detailed or systematic way, either the occurrence of a splodge or the seeing of it by an observer.

XI.

In *The Structure of Scientific Revolutions*, Kuhn provides an additional account of observation, which differs from that described above. According to this account, what an observer perceives is partly a function of what he looks at (or listens to, and so forth). But it is also partly determined by his "expectations" or "mental set." These in turn are determined by the observer's background beliefs and attitudes, including the theories or paradigms which guide his investigations. Such theories or paradigms supply a system of categories describing the objects of perception. This version of Kuhn's account leaves it open whether the theory or paradigm which plays this role must also be such that it can explain facts about what is observed. It thus avoids some of the difficulties described above.

According to this account, the phenomena which are described in observational reports are literally perceived. However, while the "New Look" theories at least tell us that what is seen is a distal stimulus, this account does not offer us any way of deciding what the object of perception is in any given case. Presumably, though, if the claim that something is an object of perception is to mean anything definite at all, the relevant notion of perception must be constrained by what we know empirically about the operation of human perceptual systems. What is seen must, for example, be detected by means of electromagnetic radiation of certain wavelengths which figures in retinal interactions. But, given even very abstract and general constraints of this kind, it is wrong to think there are many cases in which an observer looks at or perceives phenomena. Neutrinos, neutral currents, and the functional defects of the subjects investigated by Millner and Teuber do not emit or reflect visible light which can interact with any observer's retina.

It does seem likely that those who worked in the solar neutrino detection project saw what their expectations and background beliefs led them to think of as splodges that were indicative of interactions involving neutrinos. But neither Kuhn's account nor the psychological theories from which it derives make it plausible that the investigators could see the flux of neutrinos—the phenomenon they used their equipment to detect. If phenomena are not perceived, Kuhn's theory gives no reason to say they are reported by theory-laden observation claims. Of course, for reasons set out by Lewis Carroll, we can talk about the perception of anything we want, using perceptual terms loosely enough to avoid the bothersome constraint that what is seen should figure somehow in retinal interactions. But the claim that phenomena are observed can be no more informative than the usage of the terms like "perception" and "see" allows it to be.

XII.

Another way to try to extend the notion of observation to phenomena like elementary particles is to appeal to information theory. In an effort to take literally scientists' talk of "seeing the inside of the sun" by means of the detection of solar neutrinos, Dudley Shapere proposes that a scientist observes whatever "directly transmits information" to "an appropriate receptor" without "interference."[39] Ian Hacking accepts this proposal with the addi-

[39]Dudley Shapere, "The Concept of Observation in Science and Philosophy," p. 492.

tional requirement that the processing by which the information is picked up and extracted must be sufficiently independent of the theory the observation is used to test.[40] If perception is the pick-up of information, it might seem that the procedures by which data are interpreted in the solar neutrino experiments are sufficiently similar to what goes on in garden-variety everyday perception to warrant the claim that fluxes of neutrinos, and interactions in the solar core which produce them, can count as observable. It might seem, then, that descriptions of such phenomena could count as observation-sentences. Reports of phenomena and reports of observations could be equated—avoiding our objection that phenomena are seldom, if ever, perceived.

The quotation marks in our sketch of Shapere surround words and phrases Shapere actually uses and which are essential to his characterization of observation. Unfortunately there is at present no information-theoretical account of perception which attaches to these words and phrases any definite meaning suitable for Shapere's purposes. Moreover, the prospects for the development of an information-theoretical account of perception are dim. As things stand now, Shapere has not so much a theory of observation or observation-sentences as a promissory note we do not know how to cash.[41] Furthermore, it would be difficult to cash in the

[40]Ian Hacking, *Representing and Intervening*, p. 185. But as noted above, Hacking's purpose is different from Shapere's, and in general, he rejects the view of science we have been criticizing.

[41]The only well-worked-out theory of information is the Shannon-Weaver theory which provides a mathematical characterization of information whose only clear application is to signal transmission in which the semantic content of the signal is irrelevant. There is nothing in the Shannon-Weaver theory to tell us what counts as information in the sense of content conveyed by a signal. But this is the notion of information required for a theory of perception. See, for example, Fred Dretske, *Knowledge and the Flow of Information* (Cambridge, England: Cambridge University Press, 1982), Chapter 2; and Claude Shannon and Warren Weaver, *The Mathematical Theory of Communication* (Urbana, Ill.: The University of Illinois Press, 1949). Furthermore, the application of anything remotely like the theory of Shannon-Weaver to perception would require a principled specification of proximal or distal stimuli, and an account of the prior probabilities that one or the other has any given perceptible feature. It would also require an account of the degree to which the registration of a stimulus would affect the probability of its having the relevant feature. None of this is known for real cases of seeing. The attempts of Dretske and others to provide a new concept of information suitable for the treat-

language in quotes without classifying data as observable. If observation-sentences mention sources of "directly transmitted information," then bubble chambers and bubble chamber photographs ought to count as information sources just as much as elementary particles. It is hard to see how the thermometer could be excluded while the melting point of lead is included, how the neutrinos could qualify as observable unless the vat of cleaning fluid, the argon isotope, the reactions in the Geiger counter, and the splodges qualify too. Thus, even if Shapere's program could be worked out in detail, it is hard to see how the developed version could distinguish between phenomena and data. Shapere does think that theories explain, predict, and are tested against facts about what is observed. Thus, if observation reports, as Shapere conceives them, turn out to include reports about data as well as reports about phenomena, his theory would lack the distinctions needed to avoid the mistaken requirement that science must explain facts about data.

XIII.

We can think of the traditional picture—according to which science explains facts about what we observe—as motivated by two considerations. The first is the unexceptionable idea that (1) we should have good grounds for believing that those explananda which we require a theory to explain are (roughly) true. The second is the idea, associated with the British Empiricists and with their intellectual descendents, the Logical Empiricists, that (2) perception and sense-experience (as direct and unmediated as possible) have an epistemologically privileged status regarding the justification of beliefs about the natural world and that the most

ment of perception reduce to older and unpromising accounts which are not information-theoretical. Shapere's own discussion of detailed cases suggests that some of his talk of information-transmission and pick-up boils down to the detection of unperceived phenomena from data. As such his account agrees with ours. But what Shapere says about what it is to pick up information also seems to commit his account to the claims that (a) we observe interactions occurring within the sun and that (b) we observe those interactions only if systematic explanations are given for the data from which they are detected. We of course hold that the relevant data cannot be systematically explained.

secure and convincing grounds for belief that something is the case is that one perceives it to be the case. We think that a commitment to some version of (1) and (2) underlies not only the idea that science explains what we observe, but also a number of other doctrines in both traditional and post-traditional philosophy of science, centering around scientific realism and the status of "theoretical entities." Consider, for example, Grover Maxwell's well-known criticisms of the observational/theoretical distinction and van Fraassen's recent defense of "constructive empiricism."[42] Maxwell is a realist about theoretical entities; he holds that terms like "electron" and "neutrino" denote. He argues for this position by attempting to show that there is no crucial or essential difference between such theoretical entities, and the things we happen to be able to observe. Maxwell claims that in principle, although not in fact, all such entities are observable. By this, Maxwell means that it is just a matter of biological accident that our sense organs do not register theoretical entities and a matter of historical accident that we do not have instruments which allow us to see them (for example, in the way we can see large molecules through an electron microscope). Presumably, Maxwell finds this form of argument appealing at least in part because he also finds some version of (2) plausible. In sharp opposition to this, van Fraassen argues that to accept a theory, one need not believe that the designata of its theoretical terms exist. Instead, acceptance requires only belief that a theory is "empirically adequate"—that is, that it conforms to what can be observed. Here too, van Fraassen seems to rely on some version of (2), although of course his views about what is observable differ sharply from Maxwell's.

Our view is that while claim (1) is indeed correct, claim (2) is fundamentally misguided, at least when it is understood as the claim that we lack secure grounds for belief in the existence of entities we cannot perceive. For the most part, phenomena cannot be perceived and, in many cases, the justification of claims about the existence of phenomena does not turn, to any great extent, on facts about the operation of the human perceptual system. None-

[42]Grover Maxwell, "The Ontological Status of Theoretical Entities," in H. Feigl and G. Maxwell, eds. *Minnesota Studies in the Philosophy of Science*, Vol. 3 (Minneapolis, Minn.: University of Minnesota Press, 1962), pp. 3–27; Bas van Fraassen, *The Scientific Image*.

theless, we are justified in believing claims about phenomena as long as data are available which constitute reliable evidence for such claims, where the notion of reliability is meant to include (but is not limited to) all of the disparate considerations described in Section V. Thus, the proper strategy for philosophers interested in understanding whether and why we are justified in believing in the existence of neutral currents is not to try to show that they are perceivable in principle or that the processes by which they are detected are relevantly analogous to those underlying vision, but rather simply to focus on the relevant bubble chamber data and the complex considerations (having to do with correcting for the neutron background and so forth) which were relevant to establishing that this data was reliable.

While we agree with van Fraassen that a successful theory should be "empirically adequate," we do not accept his construal of this notion. Empirical adequacy, as we understand it, means that a theory must "save" or "be adequate to" the phenomena, which for the most part are not observed, rather than the data which are observed. By contrast, van Fraassen requires that theories save or be adequate to what can be observed. This is tantamount to requiring that a theory must save the data—that an acceptable theory of molecular structure, in Nagel's example, must fit the observed scatter of thermometer readings, rather than the true melting point of lead which is inferred from these readings. We have argued at length that this is an unreasonable requirement to impose on any theory. It seems unlikely that van Fraassen could accept our notion of empirical adequacy without abandoning many of his most central claims. If we possess evidence and procedures which can justify belief in claims about phenomena, even though many phenomena are unobservable, it is hard to see on what grounds van Fraassen could deny that we are justified in believing as true many other typical theoretical beliefs regarding entities like atoms, electrons, and neutrinos.

Contrary to Maxwell, the differences between phenomena and what is observable (that is, data) are both striking and important. Events which are accessible to the human sensory system are rarely the result of a single phenomenon operating alone, but instead typically reflect the interaction of many different phenomena. Nature, and the environmental problems we have to solve in order to flourish in nature, are so complicated that even if a sense organ which did nothing but provide noiseless data accurately registering a single phenomenon were biologically possible, it would be unlikely to have any survival value whatsoever. Similarly, it is hard to imagine an instrument which could register any phenomenon of interest in isolation from the multitude of background factors which operate in such a way as to make data-analysis and interpretation necessary. To the extent that Maxwell's rejection of a principled distinction between theoretical entities and observables involves a conflation of data with phenomena, it belongs to the tradition against which we have been arguing.

The empiricist thesis (2), when understood as the claim that we lack secure grounds for belief in the existence of entities which we cannot perceive, is both overly optimistic about the capacities of our sense organs and instruments, and unduly pessimistic about our resources for establishing the existence of phenomena. It is overly optimistic, and biologically unrealistic, to think that our senses and instruments are so finely attuned to nature that they must be capable of registering in a relatively transparent and noiseless way all phenomena of scientific interest, without any further need for complex techniques of experimental design and data-analysis. It is unduly pessimistic to think we cannot reliably establish the existence of entities which we cannot perceive. In order to understand what science can achieve, it is necessary to reverse the traditional, empiricist placement of trust and of doubt. Our stance is to be modest and conservative in our estimation of what our senses and instruments can register, and to put more trust in the abilities of scientists to detect phenomena from the relatively little our senses and instruments do provide. We are bearish on perception and bullish on data-analysis, experimental design, and other techniques employed for the detection of phenomena.

Pitzer College
California Institute of Technology