Explanation v. Prediction: Which Carries More Weight?

Author(s): Peter Achinstein
Source: *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, Vol. 1994, Volume Two: Symposia and Invited Papers (1994), pp. 156-164
Published by: The University of Chicago Press on behalf of the Philosophy of Science Association
Stable URL: https://www.jstor.org/stable/192926
Accessed: 14-10-2019 14:20 UTC

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Peter Achinstein

Johns Hopkins University

1. The Historical Thesis of Evidence

According to a standard view, predictions of new phenomena provide stronger evidence for a theory than explanations of old ones. More guardedly, a theory that predicts phenomena that did not prompt the initial formulation of that theory is better supported by those phenomena than is a theory by known phenomena that generated the theory in the first place. So say various philosophers of science, including William Whewell (1847) in the 19th century and Karl Popper (1959) in the 20th, to mention just two.

Stephen Brush takes issue with this on historical grounds. In a series of fascinating papers he argues that generally speaking scientists do not regard the fact that a theory predicts new phenomena, even ones of a kind totally different from those that prompted the theory in the first place, as providing better evidential support for that theory than is provided by already known facts explained by the theory. By contrast, Brush claims, there are cases, including general relativity and the periodic law of elements, in which scientists tend to consider known phenomena explained by a theory as constituting much stronger support than novel predictions.[1]

Both the predictionist and the explanationist are committed to an interesting historical thesis about evidence, viz.

> *Historical thesis*: Whether some claim e, if true, is evidence for an hypothesis h, or how strong that evidence is, depends on certain historical facts about e, h, or their relationship.

For example, whether, or the extent to which, e counts as evidence for h depends on whether e was known before or after h was formulated. Various historical positions are possible, as Alan Musgrave (1974) noted years ago in a very interesting article. On a simple predictionist view (which Musgrave classifies as "purely temporal") e supports h only if e was not known when h was first proposed. On another view (which Musgrave attributes to Zahar (1973) and calls "heuristic"), e is evidence for h only if when h was first formulated it was not devised in order to explain e. On yet a third historical view (which Musgrave himself accepts), e is evidence for some theory T only if e cannot be explained by a "predecessor" theory, i.e., by a competing theory which was devised by scientists prior to the formulation of T. These views, and other variations, are all committed to the historical thesis.

PSA 1994, Volume 2, pp. 156-164

Is the historical thesis true or false? I propose to argue that it is sometimes true, and sometimes false, depending on the type of evidence in question. Then I will consider what implications, if any, this has for the debate between Brush and the predictionists.

Before beginning, however, let me mention a curious but interesting fact about various well–known philosophical theories or definitions of evidence. As Laura Snyder (1994) points out in a perceptive paper entitled "Is Evidence Historical?", most such theories, including Carnap's (1962) a priori theory of confirmation, Hempel's (1945) satisfaction theory, Glymour's (1980) bootstrap account, and the usual hypothetico–deductive account, are incompatible with the historical thesis. They hold that whether, or the extent to which, e is evidence for, or confirms, h is an objective fact about e, h, and their relationship. It is in no way affected by the time at which h was first proposed, or e was first known, or by the intentions with which h was formulated. Defenders of these views must reject both the predictionist and the explanationist claims about evidence. They must say that whether, or the extent to which, e supports h has nothing to do with whether e was first formulated as a novel prediction from h or whether e was known before h and h was constructed to explain it.

Accordingly, we have two extreme or absolutist positions. There is the position, reflected in the historical thesis, that evidence is always historical (in the sense indicated). And there is a contrasting position, reflected in certain standard views, that evidence is never historical. Does the truth lie at either extreme? Or is it somewhere in the middle?

2. Selection Procedures

Suppose that an investigator decides to test the efficacy of a certain drug D in relieving symptoms S. The hypothesis under consideration is

h: Drug D relieves symptoms S in approximately 95% of the cases.

The investigator may test drug D by giving it to persons suffering from S and by giving a placebo to other persons suffering from S (the "control group"). In deciding how to proceed, the investigator employs what I will call a "selection procedure," or rule, determining how to test, or obtain evidence for, an hypothesis, in this case determining which persons he will select for his studies and how he will study them.

For example, here is one of many possible selection procedures (SP) for testing h:

SP 1: Choose a sample of 2000 persons of different ages, sexes, races, and geographical locations, all of whom have symptoms S in varying degrees; divide them arbitrarily into 2 groups; give one group drug D and the other a placebo; determine how many in each group have their symptoms relieved.

Now, suppose that a particular investigator uses this (or some other) selection procedure and obtains the following result:

e: In a group of 1000 persons with symptoms S taking drug D, 950 persons had relief of S; in a control group of 1000 S–sufferers not taking D but a placebo none had symptoms S relieved.

The first thing to note about this example is that whether the report e supports hypothesis h, or the extent to which it does, depends crucially on what selection procedure was in fact used in obtaining e. Suppose that instead of SP1 the following selection procedure had been employed:

SP 2: Choose a sample of 2000 females aged 5 all of whom have symptoms S in a very mild form; proceed as in SP1.

If result e had been obtained by following SP2, then e, although true, would not be particularly good evidence for h, certainly not as strong as that obtained by following SP1. The reason, of course, is that SP1, by contrast with SP2, gives a sample that is varied with respect to two factors that may well be relevant: age of patient and severity of symptoms. (Hypothesis h does not restrict itself to 5 year old girls with mild symptoms, but asserts a cure–rate for the general population of sufferers with varying degrees of the symptoms in question.)

This means that if the result as described in e is obtained, then whether, or to what extent, that result confirms the hypothesis h depends crucially on what selection procedure was in fact used in obtaining e. That is, it depends on an historical fact about e: on how in fact e was obtained. If e resulted from following SP1, then e is pretty strong evidence for h; if e was obtained by following SP2, then e is pretty weak evidence for h, if it confirms it at all. Just by looking at e and h, and even by ascertaining that e is true, we are unable to determine to what extent, if any, e supports h. We need to invoke "history."

To nail down this point completely, consider a third selection procedure:

SP 3: Choose a sample of 2000 persons all of whom have S in varying degrees; divide them arbitrarily into 2 groups; give one group drugs D and D' (where D' relieves symptoms S in 95% of the cases and blocks possible curative effects of D when taken together); give the other group a placebo.

Consider once more result e (which, again, let us suppose, obtains). In this case e supports h not at all. And, again, whether this is so cannot be ascertained simply by examining the propositions e, h, or their "logical" relationship. We need to know an historical fact about e, viz. that the information it (truly) reports was obtained by following SP3.

So far then we seem to have support for the historical thesis about evidence. Can we generalize from examples like this to all cases. Can we say that for any true report e, and any hypothesis h, whether, or to what extent, e is evidence for h depends upon historical facts about how e was obtained? No, we cannot.

Consider another very simple case. Let e be the following report, which is true:

e = In last week's lottery, 1000 tickets were sold, of which John owned 999 at the time of the selection of the winner; this was a fair lottery in which one ticket was selected at random.

h = John won the lottery.

In an attempt to obtain information such as e to support h different rules or "selection procedures" might have been followed, e.g.,

SP 4: Determine who bought tickets, and how many, by asking lottery officials.

SP 5: Determine this by standing next to the person selling tickets.

SP 6: Determine this by consulting the local newspaper, which publishes this information as a service to its readers.

Let us suppose that following any of these selection procedures results in a true report e. (And, as in the symptoms case, we may suppose that following any of these procedures is a reasonable way to establish whether e is true.) But in this case, unlike the drug example, which selection procedure was in fact followed is completely irrelevant in determining whether, or to what extent, e is evidence for h. In this case, unlike the drug example, we do not need to know how information e was obtained to know that e (assuming it is true) is very strong evidence for h. Nor do we need to know any other historical facts about e, h, or their relationship. (In particular, contrary to both the predictionist and explanationist views, we do not need to know when e was first known relative to when h was first formulated; i.e., we do not need to know whether e was explained or predicted. But more of this later when these two historical views are examined more fully.) Accordingly, we have a case that violates the historical thesis of evidence.

Since examples similar to each of the two above can be readily constructed, we may conclude that there are many cases that satisfy the historical thesis of evidence, and many others that fail to satisfy it. Is there a general rule for deciding which do and which do not?

Perhaps our two examples will help generate such a rule. In the drug case the evidence report e is historical in an obvious sense: it reports the results of a particular study made at some particular time and place. But this is clearly not sufficient to distinguish the cases, since the evidence report e in the lottery case is also historical: it reports facts about a particular lottery, who bought tickets, and when. So, I submit, what distinguishes the cases is not the historical character of the evidence, but something else.

I shall say that a putative evidence statement e is *empirically complete* with respect to an hypothesis h if whether, or to what extent, e is evidence for, or confirms, h depends just on what e reports, what h says, and the relationship between them. It does not depend on any additional empirical facts—e.g., facts about when e or h were formulated, or with what intentions, or on any (other) facts about the world. In the drug example, e is not empirically complete with respect to h: whether, or to what extent, e supports h depends on how the sample reported in e was selected—empirical information not contained in e or h.[2] By contrast, in the lottery example, e is empirically complete with respect to h: whether, and to what extent, e supports h in this case does not depend on empirical facts in addition to e. To determine whether, and how much, e supports h in this case we do not need any further empirical investigation. To be sure, additional empirical inquiry may unearth new information e' which is such that both e and e' together do not support h to the same extent that e by itself does. But that is different. In the drug but not the lottery example information in addition to e is necessary to determine the extent to which e itself supports h. In the drug case we cannot legitimately say whether or to what extent the report e supports the efficacy of drug D unless we know how the patients described in e were selected. In the lottery case information about how purported evidence was obtained is irrelevant for the question of whether or how strongly that evidence, assuming its truth, supports h.

So we have one important difference between the two examples. Is this enough to draw a distinction between cases that satisfy the historical thesis of evidence and those that do not? Perhaps not. There may be cases in which e is empirically incomplete with respect to h, but in which empirical facts needed to complete it are not historical. Consider

e = Male crows are black.

h = Female crows are black.

One might claim that whether, or the extent to which, e supports h in this case depends on empirical facts in addition to e. If, e.g., other species of birds generally have different colors for different sexes, then e does not support h very much. If other species generally have the same color for both sexes, then e supports h considerably more. But these additional facts are not "historical," at least not in the clear ways of previous examples. (I construe "other species of birds generally have different colors for different sexes" to be making a general statement, and not to be referring to any particular historical period.) If this is granted, then we need to add a proviso to the completeness idea above.

There are cases (including our drug example) in which a putative evidence claim e is empirically incomplete with respect to an hypothesis h, where determining whether, or to what extent, e supports h requires determining the truth of some historical fact. I shall speak of these as historical evidence cases. They satisfy the historical thesis of evidence. By contrast, there are cases in which a putative evidence claim e is empirically complete with respect to hypothesis h (e.g., our lottery case); and there may be cases in which a putative evidence claim e, although empirically incomplete with respect to h, can be settled without appeal to historical facts (possibly the crow example). Cases of the latter two sorts violate the historical thesis of evidence. What implications, if any, does this hold for whether predictions or explanations provide better confirmation?

3. Predictions v. Explanations

Let us return to the original question proposed by Brush. Do predictions of novel facts provide stronger evidence than explanations of old one, as Whewell and Popper claim? Or is the reverse true? My answer is this: Sometimes a prediction provides better evidence for an hypothesis, sometimes an explanation does, and sometimes they are equally good. Which obtains has nothing to do with the fact that it is a prediction of novel facts or that it is an explanation of known ones.

To show this, let us begin with a case that violates the historical thesis of evidence. Here it should be easy to show that whether the putative evidence is known before or after the hypothesis is formulated is irrelevant for confirmation. Let the hypothesis be

h = This coin is fair, i.e., if tossed in random ways under normal conditions it will land on heads approximately half the time in the long run.

e = This coin is physically symmetrical, and in a series of 1000 random tosses under normal conditions it landed on heads approximately 500 times.

We might reasonably take e to be empirically complete with respect to h. Accordingly, whether e supports h, and the extent to which it does, does not depend on empirical facts other than e. In particular, it does not depend on when, how, or even whether e comes to be known, or on whether e was known first and h then formulated, or on whether h was conceived first and e then stated as a prediction from it. Putative evidence e supports hypothesis h and does so (equally well) whether or not e is known before or after h was initially formulated, indeed whether or not e is ever known to be true.

So let us focus instead on cases that satisfy the historical thesis of evidence. We might suppose that at least in such cases explanations (or predictions) are always better for confirmation. Return once again to our drug hypothesis:

h = Drug D relieves symptoms S in approximately 95% of the cases.

Consider now two evidence claims, the first a prediction about an unknown future event, the second a report about something already known:

e_1 = In the next clinical trial of 1000 patients who suffer from symptoms S and who take D approximately 950 will get some relief.

e_2 = In a trial that has already taken place involving 1000 patients with S who took D (we know that) approximately 950 got some relief.

On the prediction view, e_1 is stronger evidence for h than is e_2. On the explanation view it is the reverse. And to sharpen the cases let us suppose that e_2, by contrast to e_1, was not only known to be true prior to the formulation of h, but that h was formulated with the intention of explaining e_2. Which view is correct? Neither one.

Let us take the prediction case e_1 first. Whether, and to what extent, e_1 (if true) supports h depends on empirical facts in addition to e_1. In this case it depends on the selection procedure to be used in the next clinical trial. Suppose this selection procedure calls for choosing just 5 year old girls with very mild symptoms who in addition to D are also taking drug D' which ameliorates symptoms S in 95% of the cases and potentially blocks D from doing so. Then e_1 would be very weak evidence for h, if it supports it at all. This is so despite the fact that e_1 is a correct prediction from h, one not used in generating h in the first place. By contrast, suppose that the selection procedure used in the past trial mentioned in e_2 is much better with respect to h. For example, it calls for choosing humans of both sexes, of different ages, with symptoms of varying degrees, who are not also taking drug D'. Then e_2 would be quite strong evidence for h, much stronger than what is supplied by e_1. In such a case, a known fact explained by h would provide more support for h than a newly predicted fact would.

Obviously the situations here can be reversed. We might suppose that the selection procedure used to generate the prediction of e_1 is the one cited in the previous paragraph as being used to generate e_2 (and vice versa). In this situation a newly predicted fact would provide more support for h than an already explained one.

In these cases what makes putative evidence have the strength it does has nothing to do with whether it is being explained or predicted. It has to do with the selection procedure used to generate that evidence.[3] In one situation—whether it involves something that is explained or predicted—we have a putative evidence statement generated by a selection procedure that is a good one relative to h; in the other case we have a flawed selection procedure. This is what matters for confirmation—not whether the putative evidence is being explained or predicted.

4. Brush Redux

Brush is clearly denying a general predictionist thesis. By contrast he cites cases in which scientists themselves regarded known evidence explained by a theory as stronger support for that theory than new evidence that was successfully predicted. And he seems to imply that this was reasonable. He offers an explanation for this claim, viz. that with explanations of the known phenomena, by contrast with successful predictions of the new ones, scientists had time to consider alternative theories that would generate these phenomena. Now, even if Brush does not do so, I want to extend this idea and consider a more general explanationist view that is committed to the following three theses that Brush invokes for some cases:

(1) A selection procedure for testing a hypothesis h is flawed, or at least inferior to another, other things equal, if it fails to call for explicit consideration of competitors to h.

(2) The longer time scientists have to consider whether there are plausible competitors to h the more likely they are to find some if they exist.

(3) With putative evidence already known before the formulation of h scientists have (had) more time to consider whether there are plausible competitors to h than is the case with novel predictions.

I would challenge at least the first and third theses. In my first example, selection procedure 1 for the drug hypothesis does not call for explicitly considering competitors to that hypothesis. Yet it does not seem flawed on that account, or inferior to one that does. However, even supposing it were inferior, whether or not a selection procedure calls for a consideration of competitors is completely irrelevant to whether the putative evidence claim is a prediction or a known fact being explained. In the case of a prediction, no less than that of an explanation, the selection procedure may call for a consideration of competitors.

For example, in our drug case, where h is "Drug D relieves symptoms S in approximately 95% of the cases," and e is the prediction "In the next clinical trial of 1000 patients suffering from symptoms S who take D, approximately 950 will get some relief," the selection procedure to be used for the next clinical trial might include the rule

> In conducting this next trial, determine whether the patients are also taking some other drug which relieves S in approximately 95% of the cases and which blocks any effectiveness D might have.

Such a selection procedure calls for the explicit consideration of a competitor to explain e, viz. that it will be some other drug, not D, that will relieve symptoms S in the next trial. This is so even though e is a prediction. Moreover, to respond to the third thesis about time for considering competitors, an investigator planning a future trial can have as much time as she likes to develop a selection procedure calling for a consideration of a competing hypothesis. More generally, in designing a novel experiment to test some hypothesis h as much time may be spent in precluding competing hypotheses that will explain the test results as is spent in considering competing hypotheses for old data.

5. Thomson v. Hertz

Finally, let me invoke an example more recognizably scientific. It involves a dispute between Heinrich Hertz and J.J. Thomson over the nature of cathode rays.[4] In experiments conducted in 1883 Hertz observed that the cathode rays in his experiments were not deflected by an electrical field. He took this to be strong evidence that cathode rays are not charged particles (as the English physicist William Crookes had concluded), but some type of ether waves. In 1897 J.J. Thomson repeated Hertz's experiments but with a much higher evacuation of gas in the cathode tube than Hertz had been able to obtain. Thomson believed that when cathode rays pass though a gas they make it a conductor, which screens off the electric force from the charged particles comprising the cathode rays.[5] This screening off effect will be reduced if the gas in the tube is more thoroughly evacuated. In Thomson's 1897 experiments electrical deflection of the cathode rays was detected, which Thomson took to be strong evidence that cathode rays are charged particles.

Here, however, I want to consider the evidential report of Hertz in 1883, not of Thomson in 1897. Let

> e = In Hertz's cathode ray experiments of 1883 no electrical deflection of cathode rays was detected.
>
> h = Cathode rays are not electrically charged.

Hertz took e to be strong evidence for h. In 1897 Thomson claimed, in effect, that Hertz's results as reported in e did not provide strong evidence for h, since Hertz's experimental set–up was flawed: He was employing insufficiently evacuated tubes. To use my previous terminology, Thomson was claiming that Hertz's selection procedure for testing h was inadequate.[6]

Here we can pick up on a point emphasized by Brush. Hertz, we might say, failed to use a selection procedure calling for considering a competitor to h to explain his results (viz. that cathode rays are charged particles, but that the tubes Hertz was using were not sufficiently evacuated to allow an electrical force to act on these particles). But—and this is the point I want to emphasize—in determining whether, or to what extent, Hertz's putative evidence e supports his hypothesis h, it seems to be irrelevant whether Hertz's e was a novel prediction from an already formulated hypothesis h or an already known fact to be explained by h. Hertz writes that in performing the relevant experiments he was trying to answer two questions:

> Firstly: Do the cathode rays give rise to electrostatic forces in their neighbourhood? Secondly: In their course are they affected by external electrostatic forces? (Hertz 1896, p. 249)

In his paper he did not predict what his experiments would show. Nor were the results of his experiments treated by him as facts known before he had formulated his hypothesis h. Once he obtained his experimental result he then claimed that they supported his theory:

> As far as the accuracy of the experiment allows, we can conclude with certainty that no electrostatic effect due to the cathode rays can be perceived. (p. 251)

To be sure, we might say that Hertz's *theory* itself predicted some such results, even if Hertz himself did not (i.e., even if Hertz did not himself draw this conclusion before getting his experimental results). But even if we speak this way, Hertz did not claim or imply that his experimental results provide better (or weaker) support for his theory because the theory predicted them before they were obtained. Nor did Thomson in his criticism of Hertz allude to one or the other possibility. Whichever it was—whether a prediction or an explanation or neither—Hertz (Thomson was claiming) should have used a better selection procedure. This is what is criticizable in Hertz, not whether he was predicting a novel fact or explaining a known one.

I end with a quote from John Maynard Keynes (1921, p. 305), whose book on probability contains lots of insights. Here is one:

> The peculiar virtue of prediction or predesignation is altogether imaginary. The number of instances examined and the analogy between them are the essential points, and the question as to whether a particular hypothesis happens to be propounded before or after their examination is quite irrelevant.

Notes

[1]To what extent Brush wants to generalize this explanationist position is a question I leave for him to answer. There are passages in his writings that strongly suggest a more general position. For example: "There is even some reason to suspect that a successful explanation of a fact that other theories have already failed to explain satisfactorily (for example, the Mercury perihelion) is more convincing than the prediction of a new fact, at least until the competing theories have had their chance (and failed) to explain it" (1989, p. 1127). In what follows I consider a generalized explanationist thesis.

[2]For Carnap (1962) and others, every e is empirically complete with respect to every h. For these writers, whether, and the extent to which, e confirms h is an a priori matter.

[3]Cf. Mayo (1991).

[4]See Achinstein (1991), Essays 10 and 11; also Buchwald (1994), ch. 10.

[5]See Thomson (1897), p. 107.

[6]Lord Rayleigh (1942, pp. 78–9), in a biography of Thomson, made the same claim: "He [Hertz] failed to observe this [electrical] effect, but the design of his experiment was open to certain objections which were removed in a later investigation by Perrin in 1895, directed to the same question. Perrin got definite evidence that the rays carried a negative charge. J.J. Thomson, in a modification of Perrin's experiment showed that if

the Faraday cylinder was put out of the line of fire of the cathode, it acquired a charge when, and only when, the cathode rays were so deflected by a magnet as to enter the cylinder." [Note Rayleigh's claim that Perrin (and Thomson) got "definite evidence" that cathode rays carry a negative charge, whereas, by implication, Hertz's experiments did not give "definite evidence" concerning the question of charge.]

Acknowledgement: I am indebted to Laura J. Snyder for very helpful discussions, and to Robert Rynasiewicz for trying to convince me of the error of my ways.

References

Achinstein, P. (1991), *Particles and Waves*. New York: Oxford University Press.

Brush, S. (1989), "Prediction and Theory Evaluation: The Case of Light Bending," *Science* 246: 1124–29.

Buchwald, J. (1994), *The Creation of Scientific Effects*. Chicago: University of Chicago Press.

Carnap, R. (1962), *Logical Foundations of Probability*. Chicago: University of Chicago Press, 2nd ed.

Glymour, C. (1980), *Theory and Evidence*. Princeton: Princeton University Press.

Hempel, C. (1945), "Studies in the Logic of Confirmation," *Mind* 54: 1–26, 97–121.

Hertz, H. (1896), *Miscellaneous Papers*. London: Macmillan.

Keynes, J.M. (1921), *A Treatise on Probability*. London: Macmillan.

Mayo, D. (1991), "Novel Evidence and Severe Tests," *Philosophy of Science* 58: 523–52.

Musgrave, A. (1974), "Logical versus Historical Theories of Confirmation," *British Journal for the Philosophy of Science* 25: 1–23.

Popper, K. (1959), *The Logic of Scientific Discovery*. London: Hutchinson.

Rayleigh, R. (1942), *The Life of Sir J.J. Thomson*. Cambridge: Cambridge University Press.

Snyder, L. (1994), "Is Evidence Historical," *Scientific Methods: Conceptual and Historical Problems,* P. Achinstein and L. Snyder, eds. Malabar, Florida: Krieger.

Thomson, J.J. (1897), "Cathode Rays," *The Electrician* 39: 104–108.

Whewell, W. (1847), *The Philosophy of the Inductive Sciences*. New York: Johnson Reprint, 1967.

Zahar, E. (1973), "Why Did Einstein's Programme Supercede Lorentz's," *British Journal for the Philosophy of Science* 24: 95–123, 223–62.