# Reintroducing Prediction to Explanation

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Although prediction has been largely absent from discussions of explanation for the past 40 years, theories of explanation can gain much from a reintroduction. I review the history that divorced prediction from explanation, examine the proliferation of models of explanation that followed, and argue that accounts of explanation have been impoverished by the neglect of prediction. Instead of a revival of the symmetry thesis, I suggest that explanation should be understood as a cognitive tool that assists us in generating new predictions. This view of explanation and prediction clarifies what makes an explanation scientific and why inference to the best explanation makes sense in science.

**1. Introduction.** It has become canonical among philosophers of science that explanation is a central goal of science. Many philosophers of science also recognize prediction as a central goal of science, and often they are paired together as two potentially competing goals of the scientific enterprise. This has been apparent in various works over the past half century. A small sampling includes

> Albert Hofstadter in 1951: "Prediction and explanation are the two main functions of scientific knowledge." (339)
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> Wes Salmon in 1978: "Science, the majority [of philosophy of science texts] say, has at least two principal aims—prediction (construed broadly enough to include inference from the observed to the unobserved, regardless of temporal relations) and explanation." (684)
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> Helen Longino in 2002: "The purpose of scientific inquiry is not only to describe and catalog, or even explain, that which is present to everyday experience, but to facilitate prediction, intervention, con-

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trol, or other forms of action on and among the objects in nature." (124)

Despite the fact that most philosophers acknowledge the general importance of prediction for science, the vast majority of the intellectual focus between the two goals rests on explanation. Prediction is rarely a topic in its own right, appearing mainly in discussions of confirmation, realism, and other topics. It has been this way for over 40 years.

In this essay I will argue that such an exclusive focus on explanation, and an abandonment of prediction in discussions of explanation, has hampered our ability to properly understand explanation. There was a time when explanation and prediction were not just two goals that could both be mentioned as important for science but when they were thought to be closely related to each other. In this essay, I will remind us of that period, giving an account of both how it came about and how it was replaced with the predominant focus on explanation. I will not argue for a return to the earlier standard view on the relationship between explanation and prediction (called the symmetry thesis, discussed in greater detail below). There were good reasons for rejecting such a model. Nor will I advocate a position wherein explanation is a source for suspect metaphysics and science should be construed as solely aimed at prediction. Instead, I will argue that explanation and prediction are best understood in light of each other and thus that they should not be viewed as competing goals but rather as two goals wherein the achievement of one should facilitate the achievement of the other.

Such an understanding is badly needed, as the difficulty of explicating the importance of explanation rests in part on our forgetting the importance of prediction. The overemphasis on explanation, and excessive distancing between explanation and prediction, has led us astray. If we reconsider the possibilities of a relationship between the two, particularly beyond the straitjacket of a purely logical relationship, we can get a deeper answer concerning why explanations are important, thus answering more thoroughly Salmon's query, "Why ask 'why?'?" We can also gain insight into which explanations should be taken more seriously in science and which less so. I will suggest that it is on the basis of explanation's ability to generate new predictions that we should evaluate the explanatory strength of an account. In other words, the scientific nature of an explanation is to be found in its ability to generate testable predictions. The generative process may be purely logical, but it may not be. The generation depends on the type of explanation under consideration, whether it be covering law, causal, unification, or mechanism models, or some other form.

Bringing prediction back into the philosophical discourse on explanation provides an understanding of explanation that captures the insights produced by the proliferation of theories of explanation since the demise of Hempel's covering law model as the sole contender, without falling prey to the idea that one of these theories must win out over the others.[1] If instead of looking for the one correct view of explanation, considered in isolation from prediction, we look at explanation in relation to prediction, it becomes clearer why there are currently multiple models of explanation. I will articulate how each of the different main contenders for an account of explanation can be interpreted in light of a relationship to prediction—how they each can serve to generate predictions. Explanations that do not have this generative power should be considered scientifically suspect. Predictions assist our explanatory endeavors by providing a check on our imaginations, helping to narrow the explanatory options to those that will provide a more reliable basis for decision making. Once prediction is back in the picture, a general understanding of all the theories of explanation can be had, without losing the richness of theories of explanation.

First, some preliminary clarifications. I will be focused here on scientific explanations, not explanation in general. Thus, unlike Lipton 2004, I will not be focused on a descriptive account of explanatory inference in general but, rather, with the use of explanations in scientific reasoning alone. In the end, my ideas about the nature and function of explanation will help make more precise a delineation between explanation in general and scientific explanation, although I suspect the aspects that make an explanation scientific are also virtues in explanation more generally.

I also will take prediction to be indexed to the predictor's epistemological state rather than temporal location.[2] Thus, a claim that we should find a piece of evidence in a particular context is a prediction, even if that context occurred in the past and the evidence is merely preserved and awaiting discovery.[3] Indeed, if someone claims that some event should have taken place, but they do not know that it did, and someone else already does, that should still count as a prediction. The relevant contrasting concept, accommodation, refers to evidence that is known and

1. Newton-Smith (2000, 132) sees the current proliferation of theories of explanation as "an embarrassment for the philosophy of science." My arguments here can provide a unifying umbrella for the disparate views. They do so at the cost of seeing explanation alone as "*the* main task" of science, a view about which Newton-Smith expresses some doubts (2000).

2. In agreement with Lipton 2004, 173.

3. Some have called this a retrodiction or postdiction. See Scheffler 1957, 299; Toulmin 1961, 23–27, where he spends much time tripping over the temporal issues; or Grünbaum 1963.

available when the explanatory account is put together. There has been an interesting debate for the past 2 decades on whether successful prediction provides more epistemic warrant to a theory than accommodation. I will address this debate briefly below. If one construes accommodating evidence as explaining it, explanation and prediction appear epistemically parallel. I will argue that prediction provides epistemic warrant above and beyond accommodation, although not because of a strictly logical relation between predicted evidence and theory as compared with accommodated evidence and theory.[4] However, as we will see, not all epistemically relevant considerations are purely logical.

My focus will thus be on reintroducing prediction to theories of explanation. In order to see the full promise of this reintroduction, it would help to return to a time when they were well nigh inseparable in philosophy of science. I will describe how the two became separated before describing what we can gain by putting them together again.

**2. A History of Explanation and Prediction.** I begin this story in the 1940s. This is somewhat arbitrary, as any careful historian knows, but it is also the beginning of philosophy of science's canon on explanation, which starts with Hempel and Oppenheim in 1948. Most overviews on explanation in philosophy of science simply begin with this paper.[5] We need to understand what Hempel and Oppenheim were arguing against, however, if we are to understand why it had such an impact.

The standard view of pre-1948 accounts of explanation consists of two basic elements:[6] (1) accounts of explanation, where available, were vague and underdeveloped, and (2) many philosophers eschewed explanation as "beyond the scope of science, in such realms as metaphysics and theology" (Salmon 1999, 338). The broad antimetaphysical stance of the logical positivists made delving into explanation appear a dubious enterprise, but the antimetaphysical approach was having trouble giving a full account of science without appealing to apparently metaphysical explanatory or theoretical entities. How could one introduce these metaphysical elements, these theoretical entities that explained phenomena, without giving up on

4. Again, in agreement with Lipton 2004, 166.

5. Salmon 1989 and Kitcher 1989 are two lengthy examples. Newton-Smith 2000 and Woodward 2002 are two shorter examples. This beginning is canonical despite the fact that Hempel's 1942 paper is an earlier expression of the symmetry thesis, although focused on explanation in history.

6. Whether this standard view is adequate is beyond the scope of this essay. At any rate, it does seem to be the backdrop against which all subsequent work on explanation is understood.

the general spirit of logical positivism, especially its emphasis on logic, rationality, the unity of science, and human progress?

Hempel and Oppenheim had a way.[7] With the covering law model, they introduced a *logic* of explanation. The explanandum (the statement explained) should be logically deducible from a set of statements (the explanans) that included general laws. In addition, Hempel had already argued that such a model could make sense of explanation in history, thus unifying natural science and history (and presumably any social science in between; Hempel 1942). One could move past the view of explanations as potentially suspect metaphysics if one could provide a theory of explanation that was logical and provided a unifying structure across rational human endeavors.

In the Hempel-Oppenheim model, predictions were just explanations given at a different epistemic-temporal location. As they wrote in their 1948 paper: "the same formal analysis . . . applies to scientific prediction as well as to explanation. . . . An explanation is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the phenomenon under consideration" (Hempel and Oppenheim 1948, 138). Scientific explanations that we develop after the explanandum occurred could have been used to predict the explained event beforehand. For example, suppose you wanted to explain the trajectory of a cannonball. Such a trajectory could be deductively derived from Newtonian mechanics and the initial conditions of firing the cannonball. Such a derivation explained the trajectory. Or, if one wanted to know the trajectory ahead of time (often the more useful question), one could use the exact same derivation to predict the trajectory. Explanations and predictions took precisely the same logical form, an idea that came to be known as the "symmetry thesis."[8] This account had the advantage of being very unthreatening to the logical positivist tradition of emphasizing the predictive capacity of science. Explanations were merely late predictions. It was a beautiful model and provided much over which philosophers could fight.

And fight they did. The symmetry thesis was a central locus of critique, with many different criticisms raised against it in the late 1950s and early 1960s. Scheffler critiqued the idea of a strict logical parallel between the

7. See Koertge 1992, 89, or Hardcastle 2002 for similar, although perhaps more nuanced, accounts of the Hempel-Oppenheim paper's background and importance. Rescher 1997 provides a charming personal history of the paper's background.

8. On a historical note, this term was probably introduced following Hanson 1959, which argued against the "symmetry between prediction and explanation." Prior to this essay (e.g., Scheffler 1957 or Rescher 1958) the debates were about the structural similarities or parallels between explanation and prediction.

two, noting problems such as (1) explanations were required to be true if they were to be acceptable explanations, but predictions were not, and (2) we could produce successful predictions without adequate explanations (1957, 298). Rescher (1958) argued that it was not clear that we lived in the kind of universe where one should expect explanations and predictions to be logically symmetrical, that is, that the universe was not clearly governed by laws that supported such a model. Hanson (1959) argued that quantum mechanics provided perfectly fine explanations for single events for which the logic of quantum mechanics forbade prediction. Scriven (1959) argued that evolutionary biology was focused only on explanation and not prediction. These initial concerns were developed further in the early 1960s.[9] By the late 1960s, there were some lingering defenses of the symmetry thesis (e.g., Angel 1967), but most had abandoned it.

More interesting for my purposes is not just the abandonment of the symmetry thesis but the decentering of prediction itself as important to explanation or even as an important goal of science. This process begins with Scheffler. In 1957, he argued that "pragmatists and positivists" emphasized verifiable predictions of future states as providing "meaning or content" and that this outmoded doctrine needed to be rejected: "Abandoning it in favor of some other criterion of meaning, we remove a reason for considering prediction as primary which has been dominant in recent philosophy" (Scheffler 1957, 304). The remainder of Scheffler's essay focused on explanation. Similarly, Scriven closes his 1959 essay with the conclusion that the calling and purpose of evolutionary biology was "in the tasks of explanation, not in those of prediction" (Scriven 1959, 481). And although Rescher was embarking on a project concerning prediction with Olaf Helmer in 1958, by 1963 he seems to eschew the importance of prediction: "It is mistaken to think of the definitive task of science as being the specific prediction of the future states of natural systems. It would appear more appropriate to construe the root of science in terms rather of explanation than of prediction" (Rescher 1963, 343). To be fair, Rescher then went on to say that really it was the search for laws of nature that should be the focus for science, as neither explanation (with the Hempelian model) nor prediction could get far without such laws. But his willingness to place explanation before prediction in the importance of scientific aims is telling.

A similar emphasis on explanation can be found in Nagel's classic work, *The Structure of Science* (1961). Here, Nagel argued that "the distinctive aim of the scientific enterprise is to provide systematic and responsibly

9. See, e.g., Scriven 1962, 176–190, for a summary.

supported explanations" (1961, 15). While Nagel acknowledges that this is not the sole aim of science, he does suggest that it is the goal that makes enterprises scientific. Nagel addresses philosophical concerns from the 1920s on whether science can explain anything at all (1961, 26–27) but never mentions prediction as a potentially competing or additional goal. Discussions of prediction are present only when Nagel is attempting to undermine a qualitative distinction between the natural and the social sciences. Then, he argues that the social sciences have no special problems of prediction, aside from the reflexive prediction problems that arise from human knowledge changing human behavior (Nagel 1961, Chapter 13).

Such a decentering can also be seen in a change Hempel made to his 1948 paper for its publication in his 1965 *Aspects of Scientific Explanation*. A key paragraph was deleted, which read, "It is this potential predictive force which gives scientific explanation its importance: only to the extent that we are able to explain empirical facts can we attain the major objective of scientific research, namely not merely to record the phenomena of our experience, but to learn from them, by basing upon them theoretical generalizations which enable us to anticipate new occurrences and to control, at least to some extent, the changes in our environment" (Hempel and Oppenheim 1948, 138; *not* reprinted in Hempel 1965, 249). It is not just the logical parallel between explanation and prediction in science that interested Hempel and Oppenheim in 1948. It was also the conceptual and practical link between them, of having explanatory generalizations that could then serve as a basis for predictions, of explanations with "potential predictive force" and the practical control that comes with such force, that they saw as being important. It is this insight that I want to recover. Their theory of explanation captured one way (although not the only way, as we shall see) that this link could be understood. By 1965, such a link was receding into the philosophical background.[10]

The decentering of prediction can be seen as a backlash against the perceived centrality of prediction for positivists, but like most backlashes, it went too far. Just because explanation and prediction are not simply logical mirrors of each other does not mean there is no philosophical relationship between the two concepts. The backlash had separated prediction from explanation and marginalized prediction in our accounts of explanation. This would lead to a fruitful flowering of theories of expla-

10. By the early 1970s, some directly attacked the centrality of prediction. See, e.g., Coffa's vehement rejection of the instrumentalist view that explanations are nothing but tools for prediction. "For the instrumentalist," Coffa wrote, "science is a prediction machine with pleasant, but only psychological side-effects" (quoted in Salmon 1989, 132). For those reacting against such instrumentalism, the pendulum swung far in the other direction.

nation, focused solely on explanation. But it would also leave aside a crucial part of an understanding of scientific explanation, prediction.

**3. Proliferation of Explanation Models (sans Prediction).** In the decades that would follow, outlined in detail in Salmon's book-length essay, alternatives to the Hempelian model were developed and debated (Salmon 1989). But what was discussed less often was the issue of why explanation was important and where it fit in with the other goals of science. In addition, it was generally presumed that there could be only one correct theory of scientific explanation. Debates raged in the 1970s and 1980s as theories of explanation proliferated. By 1990, there were at least three main contenders: (1) the covering law model, (2) the causal model, and (3) the unification model. (See Kitcher 1989 for detailed accounts, criticisms, and defenses of each.) There has been at least one model added since, the mechanism model (Machamer, Darden, and Craver 2000).[11]

The trouble seems to be that each approach to explanation has its strengths and its weaknesses. For example, the Hempelian covering law model captures some of our key ideas about explanation. It provides an account of how describing the details of a context and showing how regularities (like laws) govern that context can give us a sense of understanding for events occurring in that context. The example of the cannonball's trajectory explained by Newtonian mechanics is but one of many classic examples of successful and plausible covering law explanations. But the covering law model fails miserably to provide accounts of situations where we are less confident that universal regularities ("laws") are governing events and seems to allow some regularities to be explanatory when we know they are not. For example, we can see that a barometer falling is a good indication that it is likely to rain and thus deduce the oncoming rain from the presence of the falling barometer in apparent covering law form, but the falling barometer is a lousy explanation of the weather.

Or consider the causal model of explanation. As Peter Lipton (a supporter of the causal model of explanation as "our best bet") notes, causal explanations are very common in our explanatory repertoire (2004, 32). We commonly see claims about one thing causing another as explanatory of the latter event. But Lipton also argues that "a causal model of explanation cannot be complete" (2004, 32). He gives two wonderful scientific explanations that are not causal as examples that shatter the model's universality (2004, 31–32). The first involves the explanation for an asymmetry in the behavior of sticks tossed in the air, that if frozen in

11. And this is not even considering Van Fraassen's and Achinstein's different attempts to explicate a pragmatics or contextual approach to explanation or the debates over statistical explanatory accounts, to be ignored here as largely tangential to the argument.

midair, we see more oriented toward the horizontal axis than the vertical axis. This is explained by noting that there are statistically more horizontal orientations than vertical, so the probability space explains the asymmetry (2004, 31). The second example involves the apparent failure of praise as compared to criticism for critiqued air force pilots. They always seemed to do worse after praise and better after criticism, but regression back to the mean explains the near perfect pattern better than appealing to the causal efficacy of criticism (2004, 32). Both are good noncausal scientific explanations that rely upon statistical tendencies. Although causal explanations figure prominently in science, we would be remiss to presume that causal explanations subsume all aspects of science.[12]

The unification approach also has its appeal and its weaknesses. The idea behind the unification model is that it is in the drawing together of seemingly disparate phenomena under one overarching rubric that we achieve explanation and scientific understanding (Friedman 1974). While the technical details of how we are to evaluate such unification has presented problems for this view,[13] the underlying idea has some force and fits with our intuitions, particularly when applied to our most general, overarching theories like Newtonian mechanics. Newtonian mechanics unifies such disparate laws as Galilean acceleration for projectiles and Keplerian laws about the motions of planets under one theoretical structure. Such unification seems to provide a deeper explanation of the Galilean and Keplerian laws. However, as noted by Lipton, the unification model works best for laws explaining other sufficiently disparate laws and seems to provide no account for explanations of singular instances, where we often appeal to specific causes rather than a unifying law (Lipton 2004, 28). Further, in some cases, merely unifying various instances (e.g., of particular metals expanding when heated) under a general law (metals expand when heated) is hardly explanatory at all. Unifications do not always suffice.

By the time the mechanism account of explanation was broached, this plurality had become more acceptable. In presenting this account, no claims were made that this was to be *the* theory of explanation. Rather, this new account was solely to provide insight into the workings of particular areas of science, with the focus on molecular and neurobiology (Machamer et al. 2000, 2). The authors eschew making claims about the

12. Kitcher (1989, 422–428) raises other concerns with the causal model.

13. See Kitcher 1976 and Salmon 1989, 94–101, for details about the technical difficulties of defining what should count as an instance of unification. The difficulties center on whether the laws are sufficiently independent such that unification of them is explanatory and whether the unifying law is sufficiently unified itself. Friedman (1974) lays out and grapples with these issues, and Kitcher (1989) attempts a detailed defense of the underlying view.

universality of mechanisms. Instead, they claim that four basic kinds of activities can be seen to be important in the history of science, and that the intelligibility these mechanistic accounts provided "is historically constituted and disciplinary relative" (2000, 22). The claims to universality of an explanatory approach have been abandoned as a goal for philosophers of science.[14]

Perhaps, given the plurality of views on explanation (indeed, even the disagreements about how to define the project of explanation for philosophy of science—see Koertge 1992), we should retreat back to accounts that are local, that rest within individual sciences. In these days of disunity, with a central project of the logical positivists, the unity of science, finally discarded, it would seem the prudent thing to do. However, there is one loose end that needs to be examined before we should give up on the Hempelian project. We should refocus on prediction again and its relationship to explanation. With the demise of the symmetry thesis, no one ventured any suggestions about how prediction and explanation might be related to each other. A reexamination of this potential relationship reveals how explanation and prediction are mutually beneficial, even necessary, for each other.

**4. Why Ask Why? The Function and Value of Explanation in Relation to Prediction.** One way to approach the explanation-prediction relationship is to ask about the value of each for science. The value of predictions has always been clear: predictions help us check whether our accounts of the world have any veracity and, once proven reliable, help us make better decisions about how to proceed in our lives. The value of explanations is a bit more opaque. One response to the question of why value explanations was given in Wes Salmon's 1978 APA Presidential Address, "Why Ask 'Why?'?" Salmon begins his essay by arguing that if we had all the information in a determinate world (like Laplace's demon), and we could predict and retrodict everything, we would still want more—we would want explanations of our world. Why pursue them, even if we had the predictive capacities of Laplace's demon? Salmon answers that it is because explanation "provides knowledge of the mechanisms of production and propagation of structure in the world" and because this "yields scientific understanding" (1978, 701).

However, the trope of Laplace's demon raises more questions than it answers. As Salmon later wrote, the "the demon would have no occasion to ask 'Why?'" with such perfect predictive power (1989, 127). We can wonder whether we would still want explanation under such circum-

14. Much to the regret of some. See note 1 above.

stances. Why should I care about explanatory understanding if I can predict *everything* perfectly? Why we would need explanation (other than for mere psychological satisfaction) is less than clear.

Things for explanation get even more difficult when one considers recent arguments that a sense of understanding, often tied so closely to explanation, is epistemically suspect (Trout 2002). Although explanation and understanding have been conceptually linked by philosophers of science (Trout 2002, 215), Trout argues that the sense of understanding so highly valued can arise solely from cognitive biases such as overconfidence and hindsight, wherein we fixate on a particular account of how something came about (once it has), even if we never would have constructed such a story ahead of time (2002, 222–229). Once we think we understand how something has occurred, we then are overconfident in our explanation. The sense of understanding is as much a peril as a benefit to science under this account and certainly weak ground for the value of explanation.

The value of explanations can be rescued, however, when we recall that we are not Laplacian demons. We do not have perfect predictive capacities. And even if we had a factually complete account of this very complex world, and perfect predictive capacities to utilize those facts, the complexity would be overwhelming to us. We are finite beings, with finite mental capacities. We need explanations to grapple with all of this complexity. Explanations help us to organize the complex world we encounter, making it cognitively manageable (which may be why they also give us a sense of understanding). However, as Trout notes, the sense of understanding is no good indicator of the accuracy of the explanation. It is the ability of an explanation to generate new predictions, which then serve as a check on the explanation, that improves the accuracy of our scientific explanations.

So the relationship between explanation and prediction is a tight, functional one: explanations provide the cognitive path to predictions, which then serve to test and refine the explanations. Explanations help us conceptually grapple with a complicated world, enabling us to think through the implications of our theories in particular contexts and to think all the way through to new predictions to test those theories. Explanatory understanding, on at least one recent account, is at least partly the ability to *use* a set of scientific conceptions in a practical context—to apply it properly and to see its full implications (De Regt and Dieks 2005). Rather than the potentially vacuous "sense of understanding" of such concern to Trout, it is the pragmatic aspect of explanatory understanding that grounds our need for explanation and its value in scientific inquiry. Explanatory understanding helps us "to 'see intuitively' the consequences of a scientific theory" and thus to generate new predictions to test that theory (De Regt and Dieks 2005, 157). Predictions then serve as a check on our explanations, so that our imaginative faculties do not take us too far from reliable knowledge.

Predictions are valuable because they force us (when followed through) to test our theories, because they have the potential to expand our knowledge into new realms and because they hold out the possibility (if successful) of gaining some measure of control over natural processes.

To see how explanations help to generate new predictions, consider the utilization of explanations to generate predictions for each of the four kinds of explanations currently discussed by philosophers of science:

1. Complying with the symmetry thesis, covering law explanations allow one to deductively predict the implications of theoretical laws in different contexts. So, to take the canonical example of Newtonian mechanics, we can deduce the future path of a planet from the laws of Newtonian mechanics and the initial conditions of the current location and momentum of the planet. Such a deduction can both explain the path of the planet and predict the path of the planet, depending on the direction of interest. And if the planet fails to follow its predicted path (as in the case of Uranus or Mercury), that suggests a change is needed in the deductive structure (either a change in initial conditions—there is another planet such as Neptune present—or a change in the covering law being used—as in the case of Mercury). It is the explanatory structure that allows for the deduction of predictions, which can then test the explanation.
2. Causal explanations allow one to follow the line of causal inferences to what should happen next in a causal process. If low-pressure systems, rather than barometers, cause rain, we can use this causal explanation to predict where we can expect rain in the future. We will quickly learn that low pressure is not enough and that other conditions concerning temperature and moisture levels must also be present. The causal story gets more complete, and our predictions become more successful, bolstering our confidence in the causal explanation. Causal explanations allow us to make predictions in new, similar contexts, by helping us to think through what would happen when such causal forces are in play. Such predictions can then be used to refine the causal accounts.
3. Similar to causal explanations, mechanistic explanations give us a feel for a set of functional patterns in a particular context, so we can work out what will happen in similar contexts. With the mechanism account, the importance of the predictive capacities of the explanatory framework surface explicitly. Mechanisms, according to Machamer, Darden, and Craver, help to generate predictions and possible experimental interventions that, if failures, require one to figure out which mechanism was failing (Machamer et al. 2000, 17). Thus, the explanatory import of the mechanistic schema is in part

generating testable predictions. In addition, mechanisms help provide the intelligibility that enables one to track down where an explanatory schema has failed, producing a flawed, inaccurate prediction.

4. Finally, unifying explanations bring together disparate theoretical realms so that we can more fully hammer out the potential implications and new predictions. For example, consider the wide range of predictions that (contra Scriven) follow from the unifying power of evolutionary theory. In unifying findings from disparate areas such as geology (the finding and dating of fossils), distribution patterns of species, and genetic information, evolutionary theory has proven to be among the most powerful in our time. And its explanatory structure allows us to make a plethora of predictions, including which kinds of fossils we should expect to find in rocks from certain eras, what kinds of fossilized organisms we can expect to find with each other (no human fossils with T. rexes, for example), and the extent of genetic similarity among various species. Various regularities find explanation when unified under evolutionary theory, and the unifying explanation allows us to think through the implications of the theory in new contexts. With these implications come predictions, which can then test the theory.[15] For another example, consider the Newtonian unification of various disparate laws of mechanics, which tells us that we should expect gravitational forces to act across a wide array of contexts. Once Galilean and Keplerian laws are unified under Newtonian laws, we can then predict what will happen with what were once boundary objects, objects that seemed to be potentially bound by either Galilean or Keplerian laws, such as meteorites.

Thus, all of these types of explanation can be useful and appropriate in different contexts. We should embrace the diversity of explanatory modes and look at what function explanations serve. Why ask why? Because we need the answers to those why questions to *do* stuff in science, in particular to draw out the next testable predictions from our theories. This might happen deductively, as with the covering law model, but, just as legitimately, it will happen by thinking through a mechanism or causal story and how it should act in a slightly different context or by developing a unifying theory for two existing theories and seeing what new implications follow, thus drawing out new predictions. Explanation, and the useful understanding that accompanies it, is a cognitive tool invaluable

15. That evolutionary theory can be predictive in some contexts is argued by Williams (1973) and Naylor and Handford (1985).

to science. In practice, explanations help to organize experience conceptually so that we can see the potential implications of the ideas with which we are working. Explanations are a tool for pursuing the implications of a theory and thus for generating new predictions that can then test the theory. This is the umbrella under which all the various models and theories of scientific explanation fit.

Given the wide variety of good explanations we have operating in science (arguments, schemes, expansions, detailed mechanisms), it is not surprising that every "total" theory, sans prediction, has failed. There is always a counterexample where a good explanation is excluded (an explanation without laws, an explanation without causes, a local explanation, etc.) or where a bad explanation is included (barometers, birth control pills, etc.). What makes an explanation scientific is not that it fits within one of these particular models or that it avoids some of the conceptual pitfalls that have littered the explanation landscape over the past decades. What makes an explanation scientific is that it is useful for producing that other important goal of science: testable predictions. Science needs to have its theories thrown up against the world over and over again, in new and creative ways. By providing the conceptual structure to explore the implications of a theory, explanations generate these ways. Understood in this light, it is not surprising that scientists value explanations.

The account of explanation given here is not equivalent to the purported pre-Hempel view that explanations are suspect metaphysics nor is it that they provide mere psychological comfort. Explanations are a crucial cognitive tool in doing science. Nor have I merely rejuvenated the symmetry thesis. One need not be able to deductively draw a prediction from an explanatory argument as one does with the Hempelian covering law model. In some cases, the explanations provide a conceptual bridge from theory to prediction that might be an analogical rather than deductive inference. The symmetry thesis is too strict; for it, the explanation must predict the very same event that is explained. That is the potency of the symmetry thesis and why it ultimately failed.

The relationship between prediction and explanation that I am suggesting is not nearly so strict. The explanation need not have been able to predict the very same event that was explained. Instead, it need only enable us to make further predictions, predictions drawn from using the explanation to think about the context, which we can then test against the world. So the symmetry thesis is too strong, but it captures an important intuition about prediction and explanation. Explanations should be generative of predictions, which can then be used to test the explanations and the theories they utilize. This is not a view that would have been easy to promote in the heyday of logical empiricism, for it abandons the emphasis on straightforward logical relations. With or without deductive inferences, explanation is a cognitive tool to think of additional consequences, predictions that can serve as tests.[16]

**5. Four Implications.** What potential benefits are there for adopting this view of explanation and prediction in science? I will discuss four here briefly. The first is that it provides a clear answer to what counts as a scientific explanation, as opposed to an explanation in general usage. The second is insight into why we prefer to use inference to the "loveliest" explanation (Lipton 2004). The third insight concerns how to think about the accommodation/prediction discussion. And the fourth concerns the realism debates.

*5.1. Delineating Scientific Explanation.* I have suggested that my view of the central virtue of explanation, the production of testable and hopefully successful predictions, can delineate between scientific and nonscientific explanations. A scientific explanation will be expected to produce new, generally successful predictions. An explanation that is not in fact used to generate predictions, or whose predictions quickly and obviously fail, would be scientifically suspect. An example of an explanation that fails to meet these criteria is any "just-so" story. How the leopard got its spots, and the elephant its trunk, Kipling style, are marvelous explanatory stories, but we should not take them seriously as scientific explanations, even if they are causal stories. This is because we can produce no predictions (or no successful predictions) based on these stories. What can be extrapolated from them? Should we expect other animals to gain spots or trunks? Either we fail to find any clear predictions that we can glean from these accounts, or, if we manage to cobble one together, it quickly fails to pan out. Such is the fate of the unscientific explanation.[17]

Now we should be cautious here. An explanation that can produce no predictions is clearly unscientific. If an explanation rests on a one-time unrepeatable and inferentially unusable event (a fairy caused it and will never be back), it is clearly unscientific. However, some one-time events have clear additional implications that can be used to draw out predictions—for example, explanations of the beginning of the universe, such as the big bang, and implications for what we might find when we look for certain characteristics in the cosmos, for example, the background radiation and its texture. And some competing explanatory accounts may produce identical predictions, but scientists should work to differentiate

16. Hopefully severe tests. See Mayo 1991.

17. I suspect this is the main failing of intelligent design accounts, which seem to be better at accommodation than prediction and which generally lack a specific enough design to be predictively useful.

them, utilizing the explanatory apparatuses to pursue predictions that diverge. What of explanations that produce predictions, albeit failed ones? There, it is only after we can see that the predictions are clear failures that we must abandon the explanatory account. To not do so at the point of widespread predictive failure is unscientific, regardless of explanatory power.

*5.2. Inference the Best Explanation?* The view of explanation linked to prediction can also illuminate a recent philosophical puzzle. In the admirably clear *Inference to the Best Explanation* (2004), Peter Lipton argues that we should prefer the loveliest explanation to the likeliest explanation. While counterintuitive at first glance, Lipton gives some good reasons for such a preference. Crucially, the likeliest explanation is often the most vague, least illuminating of its competitors. For example, consider two competing explanations for a recent global temperature rise. It might be a very likely explanation to say that something in the planetary conditions is changing, but the vagueness of this explanation undermines its helpfulness. (Indeed, it might be considered tautologous with what we are trying to explain, and tautologies are indeed likely to be true!) A more lovely explanation that would draw upon complex causal processes, such as greenhouse gases increasing the retention of infrared radiation, thus warming the planet over time, is more specific and thus less likely. However, it is also more fruitful and amenable to more potential tests, particularly once we developed thermodynamic models of the planet. Thus, Lipton suggests that while "likeliness speaks of truth," "loveliness [speaks] of potential understanding" (2004, 59). Given this choice, we should prefer the loveliest, because the likeliest may well be trivial and thus unilluminating.

Later in the book, Lipton attempts to argue that the loveliest explanations are good guides to the truth, but those arguments are admittedly incomplete (Lipton 2004, 142–163). The most he can argue is that inference to the best explanation is no worse than any other form of inductive inference. We are still left wondering why we should trust explanations that we find lovely to be likely. Lipton claims to be giving primarily a descriptive account, rather than a normative account, of this inference, arguing that we use it and that this is what it looks like. Thus, in many respects, this is not his fight. Lipton does not give us good reasons why the lovelier explanation is more likely to be true and retreats back to the merely descriptive claim that we really do prefer the lovelier explanation. But if we want a normative account of *scientific* explanation, we will need more.

With the account of the relation between explanation and prediction I have given, we can see why we might have such a preference and how we should think about it. The lovelier explanation, being more unified, or deeper, or more thorough, provides all the more means for robust prediction production when investigating the truth of the explanation. If we want explanations that will allow us to probe them, to test their reliability as guides for inference, then the lovelier the explanation, the more opportunities for testing will arise. So we should not infer the truth from the loveliness of an explanation alone. We should rather infer that we can more rigorously test such lovelies and thus produce more evidence (for or against) the explanation. The loveliest explanation is not necessarily the one that captures the most of the available evidence (although there is hopefully no strong evidence against it), but it is the easiest to work with. It has the inferential virtues of simplicity, elegance, and scope that appeal to us, not just because we are aesthetically driven creatures but because such virtues help us to use the explanation to think and, in particular, to think our way through to new predictions, new tests, new rigors for our beautiful explanation. It is often painful to lose such beauties to the outcomes of these tests, but we would be abandoning the strength of science if we allowed loveliness to trump evidence. Thus, we can better understand our preference for inference to the loveliest explanation. We should not infer the truth of an explanation based on its loveliness, or even its likelihood. Instead, we should infer its fruitfulness and robust testability. For this reason, the loveliest explanation may be the best bet in science.

*5.3. Accommodation and Prediction.* One might suppose that the relative disinterest in prediction among philosophers stems from the notion that explained evidence and predicted evidence are epistemically equivalent. From this perspective, whether we have evidence that is accommodated (usually read as explained) or evidence that is predicted, it is the relationship of the evidence to the theory that is important, not whether it was predicted or explained. (Collins [1994] argues for this position; White [2003] argues against it.) There has been an ongoing debate about whether prediction provides an epistemic warrant above and beyond accommodation over the past several decades. Although I do not agree with Lipton that the loveliest explanation is also likely to be true, I do find his arguments on the accommodation/prediction issue to be persuasive. He argues that prediction has an epistemic edge over accommodation not because of some difference in logical relationship between theory and evidence in the cases of prediction versus accommodation but because it assures us that the scientist has not "fudged" his or her theory in some way to accommodate unwelcome evidence (Lipton 2004, 164–183).

If we accept Lipton's arguments, prediction is not just explanation or accommodation temporally earlier in the game. Those who argue for this contemporary version of the symmetry thesis generally find epistemic warrant in the cognitive values of explanatory power, simplicity, and scope, suggesting that these values are reasons to believe a theory to be true. But this view is problematic (Laudan 2004). If we keep from collapsing explanation and prediction, noting the epistemic advantage prediction has over accommodation and the importance of explanation in indicating the rigorous testability of a theory, we can better capture the virtues of both explanation and prediction. Prediction is important because we can be surer that the scientist generating the theory has not fudged or somehow subtly made his theory inconsistent or less clearly applicable to certain contexts by virtue of some torturous, ad hoc accommodation. Prediction also allows for the generation of new (hopefully supporting) evidence. Explanation is important because it helps us think our way through to new predictions. Accommodation is valuable only insofar as it helps incorporate unexpected evidence into a theory, which *should* then lead to new predictions. Explanatory power, scope, and simplicity are not warrant providing in themselves. They are indicators of good bets for further research.

*5.4. Explanation and Realism.* In the realist/antirealist debates there has been a long-standing dispute over the virtues of inference to the best explanation. Realists have argued that we should take the best explanation as reflecting at least an approximation of the structure of the world. Antirealists have looked at the history of science and, recalling explanatory theories such as caloric, phlogiston, epicycles, and ether, rejoined that given the history of such explanatory entities, we should do no such thing.

If we understand explanation in the way I have described it, we can see a bit of the impasse between the two positions. For if an explanatory theory like ether yields no predictions, or yields predictions that, once tested, fail, we should have deep skepticism over whether this explanation gets at the structure of the world. Explanatory accounts, on the other hand, that provide predictions that do pan out, that are tested and found to be successful predictions, bolster our confidence in the truth of the theory. The history of science is replete with examples of both kinds of explanatory theories.

Happily, it is not a philosophical theory of what counts as a best explanation that should give us confidence in the explanation but rather its predictive success in practice—its progressive nature (in Lakatosian terms). Whether one wants to adopt a strong realist approach to explanations so successful, arguing that they are true or, more plausibly, approximately true, or whether one prefers a pervasive antirealist stance, viewing all explanatory accounts with some degree of skepticism, I leave to the philosophical preferences of the reader. Whichever approach, it is not just explanation, divorced from prediction, that should get our epistemic attention. It is explanations that produce prediction, which then are successful, that should get our attention. In the debates between the realists and antirealists, being able to cleave the history of science into empty explanations and fruitful explanations should help promote a clearer debate.

**6. Conclusion.** The philosophers of the late 1950s and early 1960s were right. Explanations are not just mirror images of predictions. But that does not mean that there is no relationship between them. Instead, explanations are the means that help us think our way through to the next testable prediction. This is an invaluable task in any empirical science. It is no wonder that explanations are important to scientists. They are also a worthy subject of study to philosophers. However, developing theories of explanation without thinking about their relationship to prediction has weakened accounts of explanation, even as it allowed for a diversity of views to flourish.

The view of explanation developed here has the following virtues: (1) it takes into account the strengths of all the theories of explanation, (2) it does not succumb to the pitfalls of looking for the one correct account for all explanations but, instead, provides an umbrella under which the various theories can be utilized, and (3) it provides a clear answer to the old question of when is an explanation scientific. It also may help illuminate why we utilize inference to best explanation while showing the limitations of such an inference. Finally, it may help to clarify some of the impasse in the realism debates. Explanation needs prediction if we are to fully understand its strengths.

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