

Harman and Inference to the Best Explanation

In Chapter 7, I drew attention to a putative forced conceptual choice. Some philosophers have proposed that abduction is for hypothesis generation, whereas IBE (inference to the best explanation) is for hypothesis confirmation. Given this forced choice, one might think that, since my view is that scientists sometimes use abduction for confirmation, I should say that scientists use IBE. That would be clearer.

This forced choice, however, runs entirely counter to my science-first approach to understanding scientific reasoning. My goal is not to select one of these two preexisting conceptions, Peircean abduction versus Harmanian IBE, to impose on scientific cases. That would be a philosophy-first approach. My goal is to use scientific cases as a basis upon which to build an account of scientific interpretation. In Chapter 7, I argued that certain Peircean and Neo-Peircean ideas, developed outside the context of close attention to the mid-twentieth-century primary experimental literature, do not accurately describe the reasoning of Baumgartner, Hodgkin, Huxley, and Tolman, among others. Similarly, in this chapter, I argue that Harman's ideas, developed with no particular interest in the primary scientific experimental literature, do not helpfully describe this reasoning either. Harman's view may be sufficiently general or open-ended as to include singular compositional abduction, but that very generality or open-endedness means that it lacks the detail one might want in a philosophical account of how scientists interpret controlled experiments.

I must emphasize that my goal is not to "refute" Harman's IBE model. To do that would presumably require an account of what IBE is, which is tangential to my concerns. So, for example, one might think that IBE requires that there be a set of hypotheses $H_1, H_2, \dots, H_n$ from which one selects one, H_i , that has the best weighted balance of virtues, such as simplicity, plausibility, and scope. Then again, one might maintain that the case in which there is only one hypothesis is just a special case

of IBE.¹ Far be it from me to tell advocates of IBE what IBE is.² One might also think that, since Harman proposed that IBE is warranted abduction, then, at the very least, by definition, IBE must be a warranted inference. Then again, one might think that, in typical cases where IBE might be involved, one cannot eliminate all the rival hypotheses, so that in typical cases, one does not have warrant, but one still has something describable as IBE.³ Again, far be it from me to tell advocates of IBE what IBE is. A subproject of specifying what IBE is, is tangential to my concerns. My overarching concern is developing an account of how scientists interpret the results of controlled experiments. In this chapter, I only intend a bit of philosophical cartography, indicating how my view relates to some of the ideas in Harman (1965).

8.1 IBE as Warranted Abduction

Harman's "Inference to the Best Explanation" is among the most influential papers in the entire abduction literature, spawning many disparate lines of philosophical inquiry. The principal thesis of the paper is that warranted enumerative induction is a special case of IBE. A secondary thesis is that taking IBE as the basic form of nondeductive inference accounts for an interesting feature of using the word "know." In contrast to these two central theses, there are passing "stage-setting" comments that have taken on a life of their own in the philosophical literature. These include discussions of (a) the relationship between Peirce's concept of abduction and Harman's concept of IBE; (b) a role for explanatory virtues, such as simplicity; and (c) the idea that IBE is used in science. My concern in this section is to focus philosophical attention on Harman's two principal theses and the stage-setting comments, thereby enabling me to indicate how the picture of IBE Harman developed for his project need not be deployed in my project of developing an account of how scientists interpret experimental results in support of compositional hypotheses.

Harman (1965) is a tightly argued paper that surely repays reading. It begins with a statement of Harman's primary objective: "I wish to argue

¹ See McCain (2014, p. 68).

² For efforts along these lines, see for example Lipton (2003), McCain (2015), and Lange (2022).

³ Bird (2022, p. 177) calls this "ordinary IBE", as opposed to what he calls "Holmesian inference" or "inference to the only explanation."

that enumerative induction should not be considered a warranted form of nondeductive inference in its own right. I claim that, in cases where it appears that a warranted inference is an instance of enumerative induction, the inference should be described as a special case of another sort of inference, which I shall call ‘the inference to the best explanation’” (Harman, 1965, p. 88, cf. p. 91). The gist of Harman’s argument is simple: An enumerative induction from “All observed A’s are B’s” to “All A’s are B’s” is warranted when the hypothesis that all A’s are B’s is the best explanation of why all the observed A’s are B’s.

Harman’s secondary objective is to show that there is a feature of knowledge that is captured within the framework of IBE that is not captured in terms of enumerative induction. The feature is that our knowledge is often thought to rely on what Harman calls “lemmas” or what might be called “background knowledge.” Take an example of knowledge by testimony. Suppose Jones believes that P because she read that P in a scientific journal article. To thereby know that P requires background knowledge that P does not include a falsifying typographical error. In this case, mere background *belief* that P does not include a falsifying typographical error would not suffice for knowing that P. According to Harman, IBE captures this feature. He proposes that the best explanation of the journal article’s containing P is that P was written without a typographical error. With enumerative induction, however, there is no role for the lemma. Instead, on an enumerative induction, the inference can only be of the form:

the journal printed P* and P* was true,
the journal printed P** and P** was true, etc.,

so that one can infer that when the journal printed P that P is true. There is no role for background knowledge regarding a typographical error in the enumerative induction story.

Here, I do not mean to defend Harman’s account. Instead, my primary point is to contrast Harman’s project with my project of correctly describing a bit of scientific practice. A key element here is that Harman was interested in warranted inference. Further, I wish to emphasize how some of Harman’s “stage-setting” comments have shaped the later philosophical literature. These are his passing comments about the relationship between abduction and warranted abduction (Section 8.2), the role of explanatory virtues (Section 8.3), the role of IBE in science (Section 8.4), and the connection between abduction and IBE (Section 8.5).

8.2 The Ramifications of Warrant

As noted above, Harman's goal was to defend the epistemological thesis that warranted enumerative induction is a special case of what he called "inference to the best explanation": "in cases where it appears that a warranted inference is an instance of enumerative induction, the inference should be described as a special case of another sort of inference, which I shall call "the inference to the best explanation" (Harman, 1965, p. 88). A key element here – one that is not always made explicit in subsequent discussions – is warrant. Indeed, Harman refers to warrant twice more in his brief introductory section.

In introducing the concept of IBE, Harman explicitly connects warrant with the elimination of rival explanatory hypotheses:

In making this inference one infers, from the fact that a certain hypothesis would explain the evidence, to the truth of that hypothesis. In general, there will be several hypotheses which might explain the evidence, so one must be able to reject all such alternative hypotheses before one is warranted in making the inference. Thus one infers, from the premise that a given hypothesis would provide a "better" explanation for the evidence than would any other hypothesis, to the conclusion that the given hypothesis is true. (Harman, 1965, p. 89)⁴

Harman's point is that a rival explanatory hypothesis would undermine one's justification or warrant. A rival hypothesis would be a defeater. Thus, in order for one's preferred hypothesis to be warranted, the defeaters must be removed. It is this elimination of all the rivals that motivates Harman to describe warranted abductive inference as "inference to the best explanation." Again, a foundational motive in formulating abduction as IBE is the concern with warrant.

For many subsequent projects, philosophers have not needed to distinguish between abduction and warranted abduction. So, Alexander Bird once proposed to use "inference to the best explanation" as a synonym for "abductive inference," rather than as a synonym for "warranted abductive inference."⁵ In proposing this usage, I do not think Bird intended to offer a substantive philosophical thesis. Nor do I think he intended to register some comment on Peirce's view of abduction. He simply set out

⁴ Harman is, of course, not the first to note an important role for rival hypotheses. Peirce (1992) noted it, but so did Frankfurt (1958).

⁵ Bird (2005, p. 5).

terminology that suited his local philosophical purpose, namely, to articulate a concept of “Holmesian inference.”

Douven (2017a) provides an authoritative account of abduction, but one that does not mark a distinction between warranted and unwarranted abduction. Like Harman, Douven is concerned with warranted abduction. But, when broaching a schema for abduction, he labels the relevant section of his discussion “Explicating Abduction,” rather than “Explicating Warranted Abduction.” Moreover, he supposes that the core idea of abduction is that “explanatory considerations have confirmation-theoretic import, or that explanatory success is a (not necessarily unfailing) mark of truth” (Douven, 2017a, p. 11). The implication of warrant is implicit at best.

One philosophical interest in warranted abduction/IBE is as a type of argument for scientific realism.⁶ Many philosophers of science wish to argue that the best explanation for the success of science is that scientific terms refer and scientific hypotheses are true. In this context, the philosopher of science is not merely interested in abductive inference, the philosopher of science is interested in warranted abductive inference. In the context of the scientific realism debate, there is little need to be concerned with the difference between abduction and warranted abduction.

As this is such a deeply embedded assumption in the IBE tradition, I will spend a lot of time trying to spell out how failure to distinguish between abduction and warranted abduction can lead to some dubious history of science. So, to begin, Lipton writes, “Of course, there is always more than one possible explanation for any phenomenon – the tracks might have instead been caused by a trained monkey on snowshoes, or by the elaborate etchings of an environmental artist – so we cannot infer something simply because it is a possible explanation. It must somehow be the best of competing explanations” (Lipton, 2003, p. 56). I assume that Lipton’s claim regarding what one cannot infer is not about the limits of our psychological capacities, that certain inferences are beyond the capacities of our finite minds. Instead, I take him to mean that one cannot *justifiably* infer something simply because it is a possible explanation. What one can justifiable infer, however, does not tell us how scientists actually infer.

Schurz tells a similar story at greater length:

[I]f your evidence consists in the trace of the imprints of sandals on an elsewhere empty beach, then your immediate conjecture is that somebody was recently walking here. How did you arrive at this conjecture? Classical

⁶ For a small sample, see Boyd (1983), Day and Kincaid (1994), Psillos (2005), Mizrahi (2012), Doppelt (2014), and Bird (2020).

physics allows for myriads of ways of imprinting footprints into the sand of the beach, which reach from cows wearing sandals on their feet to footprints which are drawn into the sand, blown by the wind, or caused by radioactive decay of foot-shaped portions of the sand, etc. The majority of these physically possible abductive conjectures will never be considered by us because they are extremely improbable. The major strategic algorithm which we apply in factual abduction cases of this sort is a probabilistic elimination technique which usually works in an unconscious manner: our mind quickly scans through our large memory store containing millions of memorized possible scenarios and only those which have minimal plausibility pop up in our consciousness. (Schurz, 2008, p. 207)⁷

Schurz asks how his hypothetical beachcomber came up with the conjecture that somebody was recently walking on the beach. There is, no doubt, some psychological story to be told. Moreover, classical physics does indeed allow for a myriad of alternatives. However, just because there is some psychological story to be told and physics allows for alternatives to someone walking on the beach does not provide any reason to think that the beachcomber gave any of the alternatives the slightest consideration, conscious or otherwise. What goes on in the beachcomber's mind is an empirical question. Schurz assumes that the psychological story includes subconsciously entertaining some of the millions of alternative hypotheses allowed by classical physics. But why think that? What are these millions of possible scenarios? And why think there are millions of them, rather than dozens, hundreds, or billions? Empirical evidence about the case is needed, but Schurz provides none. What is driving Schurz is a concern with warranted inference. This, in turn, drives him to a speculative psychological proposal.

Here is another perspective on the speculative psychology. Suppose one asks the beachcomber, "How do you know that the marks in the sand were not caused by radioactive decay of foot-shaped portions of the sand?" The beachcomber might say that radioactive decay of such a shape is highly improbable. Suppose that suffices to rule out the radioactive decay hypothesis. However, the beachcomber might also say, "What is radioactive decay?" Here, the beachcomber has no concept of radioactive decay, hence could not have ruled out the radioactive decay hypothesis, either consciously or unconsciously. Philosophers of science interested in specific instances of actual scientific reasoning should not assume *a priori* that

⁷ Schurz's discussion is nice, since it compactly presents the principal ideas. Similar discussions, however, can be found in Okasha (2002, p. 23f) and McCain (2019, p. 55f).

actual cases will proceed along the lines of the first scenario rather than the second.

One might observe that it is Schurz's imaginary example, so he can imagine it as he pleases. And indeed, he can. Or one might observe that Schurz is merely trying to explain what IBE reasoning is like. And maybe the latter is the more charitable reading of his comment. My concern, however, is to draw attention to how historians and philosophers of science should *not* relate imaginary examples, such as Lipton's or Schurz's, to actual scientific examples. While Lipton's or Schurz's purposes may require that an agent somehow eliminate rival hypotheses, mine do not.

The need for an abduction/warranted abduction distinction is relevant to *bona fide* scientific cases. Sometimes, scientists respond to the proposal of a rival hypothesis by pointing to features of their experimental design that rule out the rival. The rat could not be following an odor trail, since the arms of the maze were cleaned between each trial. Sometimes the rival might be ruled out by a prior experimental result. However, sometimes scientists also respond to a rival hypothesis by saying, "That is a good point. I should run an experiment that controls for that." Philosophers of science who are sufficiently familiar with actual science will know of representative cases. Thus, I propose *not* to assume *a priori* in any given case that scientists are making warranted abductive inferences. So, I am not assuming *a priori* that they have somehow eliminated some rival hypotheses. I assume that, in any given case, which rival hypotheses, if any, scientists may have eliminated is an empirical question.

What do I say to those philosophers who say that abduction is synonymous with the examination of rival hypotheses?⁸ These philosophers are counterparts to Peirce scholars who insist that abduction is simply synonymous with inference to a novel explanatory hypothesis. I would suggest that this usage is stipulative and that I am not using abduction in that sense. Moreover, I would note that there is a long tradition of using "abduction" to describe a defeasible, inductive inference to some hypothesis H on the ground that H explains E, without the presupposition that the inference somehow involves some perhaps tacit consideration of rivals to H. Recall from Chapter 7 that Peirce's "Turkish province" story involved no rival hypotheses.

In distinguishing between abduction and warranted abduction, I might cite a similar precedent. Norton discusses two examples of what he takes to be enumerative induction:

⁸ I do not assume that Bird would take this line, but in point of logic he might.

Some men are mortal.
Therefore, all men are mortal.

Some men are Greeks.
Therefore, all men are Greeks.⁹

He considers both of these instances of enumerative induction, but argues that the first is justified, whereas the second is not. Norton does not build a requirement of warrant or justification into the very idea of enumerative induction. Just so, I propose to use the term “abduction” so as not to build in a requirement of warrant, hence not build in a requirement that rival hypotheses be considered and eliminated.

At this point, one might object, “By setting aside questions regarding the warrant of abductive inferences, are you not casting doubt on the warrant of science, or at least the warrant of the parts of science that you think scientists have confirmed abductively?” No. For it is one thing to withhold judgment on the warrant of individual inferences at particular points in time, but quite another to withhold judgment on the warrant of the entire body of scientific reasoning. To suspend one’s judgment on the warrant of Hodgkin and Huxley’s first 1952 inference to the sodium hypothesis when it was first advanced says nothing about the warrant the hypothesis eventually comes to have. To return to a formulation I gave earlier, it might be that such warrant as the sodium hypothesis enjoys is not the product exclusively of the interpretation of the first Hodgkin–Huxley experiment, but instead the product of a temporally extended process of examining and eliminating multiple rivals to the sodium hypothesis. Maybe a scientist eliminates one rival based on one experiment, then a few pages later eliminates another rival based on another experiment. Furthermore, one might well endorse the overall warrant of scientific reasoning in some global sense without having to endorse every single inference made by every scientist. Indeed, I would speculate that any philosopher of science who reads enough science will eventually come across some dubious scientific inference. What I am proposing to bracket are questions about the warrant of individual scientific inferences. This is part and parcel of my granular approach to compositional abduction.

To this point of the section, I have been suggesting that by focusing on abduction, rather than warranted abduction, philosophers of science might understand individual scientific inferences about specific experimental

⁹ Norton (2021, pp. 5–6).

results without the presupposition that, in those cases, scientists are making warranted inferences and that they are somehow entertaining rival hypotheses and eliminating them. As noted in Chapter 1, I am not doing epistemology or metaphysics. I am not trying to argue that scientific abductive arguments for compositional hypotheses are warranted or that they show us what really exists in the world. Instead, I am describing what scientists are doing. Perhaps once there is an inventory of scientific practices, then philosophers of science can turn to the task of determining which are justificatory and which are not.

8.3 Explanatory Virtues

To this point in my discussion of Harman's views, I have indicated why I do not assume, going into any individual scientific interpretation of an experimental result, that the scientist will examine any rival hypotheses. Now, let me turn to the matter of how scientists cope with rival hypotheses once they have been introduced. Having introduced the need to eliminate rival hypotheses, Harman offers two sentences of guidance about how this might be done:

There is, of course, a problem about how one is to judge that one hypothesis is sufficiently better than another hypothesis. Presumably such a judgment will be based on considerations such as which hypothesis is simpler, which is more plausible, which explains more, which is less ad hoc, and so forth. (Harman, 1965, p. 89)

Clearly, there is a wide range of options for developing this proposal. Harman does not say what simplicity is, what plausibility is, etc. He does not have an account of what it is for one hypothesis to explain more than another; he does not have an account of what is sometimes called "scope" or "breadth." Nor is his list of considerations meant to be exhaustive. There might be different weightings of the relative importance of items on the list. The weightings might be dynamic, changing from context to context. There might be individual scientific variations in the weightings, wherein one scientist will assign one set of weightings, whereas another will assign another set of weightings. Harman clearly left a lot of room to develop this story.

Given the open-ended character of IBE, it is highly likely that some version of it can be made to cover the cases I describe. What I have called "abduction" might be a special case of IBE in which there is only one hypothesis under consideration. In Chapter 5, I argued, in brief, that Hodgkin and Huxley took the membrane hypothesis to be unable to

explain the overshoot of the action potential, whereas postulating sodium and potassium fluxes could explain the overshoot. One might get the IBE model to fit the case by proposing that Hodgkin and Huxley put low weights on simplicity, plausibility, and being non-ad hoc, but high weight on the greater scope of the sodium and potassium hypotheses. In Chapter 6, I noted, in brief, that Schiller and Carvey took the RGC theory of the Hermann grid illusion to explain the standard version of the illusion, but to be unable to explain the weakening of the illusion with a 45° rotation. By contrast, they took their SI theory to be able to explain both the standard and rotated versions of the illusion. As with the Hodgkin and Huxley case, one might get the IBE model to fit by proposing that Schiller and Carvey put low weights on simplicity, plausibility, and being non-ad hoc, but high weight on the greater scope. So, it would likely be a mistake to try to show that IBE is wrong.

Of course, it is one thing to not be wrong. It is another to be illuminating. While IBE may not be refuted by actual cases, it does not help to illuminate the cases. Simply starting with Harman's IBE sketch, one would not have any idea how to apply it to the scientific study of the action potential or the Hermann grid illusion. To understand how IBE might apply to episodes in the history of science, the natural thing to do is examine the history of science.

8.4 IBE is Used in Science

To this point in the chapter, I have examined IBE as a possible model of how scientists evaluate the results of a single scientific experiment. Yet, that application is not a foregone conclusion. Let me return again to Harman's paper. In an effort to motivate the significance of the idea of IBE, Harman proposes that IBE is used in detective work, knowledge by testimony, and in scientific investigation. He comments that "When a scientist infers the existence of atoms and sub-atomic particles, he is inferring the truth of an explanation for various data which he wishes to account for" (Harman, 1965, p. 89). This passing comment has invited some philosophers, such as Lipton and Schurz, to treat IBE as a psychological hypothesis or as a description of how scientists make warranted inferences in particular cases, such as the evaluation of the results of individual scientific experiments. Yet, this is not the only application.

One might, instead, treat IBE as an account of a scientific community's theory choice. The rough and ready idea here is that it is one thing for an individual to claim that evolution by natural selection provides the best

explanation of the blind cave forms in Kentucky resembling the nearby surface forms, but another thing to claim the biological community concluded that evolution by natural selection provides the best explanation of terrestrial biogeography. Very roughly, there is a distinction between explaining some individual fact and explaining some non-singleton set of facts.

In fact, in the literature, there is often a slide between introductions of IBE as a model for the explanation of some singular event and using it as a model of how scientists choose theories. Kevin McCain gives the following illustration of IBE: “You come home ready to eat the noodles that you saw in the refrigerator this morning, but they’re gone. You only have one roommate, and he has been home all day. You infer that he ate the noodles” (McCain, 2019). Over the course of a few pages, McCain shifts to IBE as an account of the confirmation of the theory of natural selection and the oxygen theory of combustion.¹⁰ These two applications of IBE should not be assumed to be identical.

My science-first approach and its resulting account of singular compositional abduction is not intended to be a rival to an IBE account of theory choice. Nevertheless, it might be useful input to such an account. One might think that singular compositional abductive inferences support hypotheses of some theory, hence be among the factors that influence scientific theory choice. Or, to put the matter another way, theory choice might be a function of singular compositional abductive inferences. Maybe singular compositional abduction would help articulate Harman’s idea that theory choice might be a function of what it is for a theory to explain more. Maybe it would help articulate the idea of “explanatory scope” or “explanatory breadth.”¹¹ For a concrete illustration, consider Thagard’s 1978 account of theory choice. Thagard proposes three main criteria: consilience, simplicity, and analogy. For present purposes, I focus on consilience, where my approach may have the most to contribute.

Thagard uses the following notation for his account. Let a theory T be a set of hypotheses $\{H_1, H_2, \dots, H_m\}$, A be a set of auxiliary hypotheses $\{A_1, A_2, \dots, A_n\}$, C be a set of accepted conditions $\{C_1, C_2, \dots, C_j\}$, and F be a set of facts $\{F_1, F_2, \dots, F_k\}$ with $k \geq 2$.¹² T is consilient if, and only if, $T \cup A \cup C$ explains a non-singleton subset of F . Next let FT_i be the set

¹⁰ See Lipton (2003, p. 56) for another example.

¹¹ Thagard (1978). Beebe (2009), among many others, also refers to explanatory scope.

¹² Strictly speaking, Thagard proposes that F is a set of classes of facts, although he does not specify what a class of facts is. This detail is inessential for my discussion.

of facts explained by T_1 . This notation enables Thagard to formulate two concepts of consilience: (1) T_1 is more consilient than T_2 if, and only if, the cardinality of FT_1 is greater than the cardinality of FT_2 and (2) T_1 is more consilient than T_2 if, and only if, FT_1 is a proper subset of FT_2 .

Before turning to the contributions of my account of compositional abduction, consider a general challenge for Thagard's account, namely, that his two conceptions of consilience do not capture some important features of scientific practice. The first conception of consilience does not capture the idea that theory choice typically involves a choice between two "competing" theories, that is, two theories that are concerned with at least some of the same facts. Let F be the union of the set of facts explained by, say, the Hodgkin–Huxley theory of the action potential and the set of facts explained by, say, the RGC theory of the Hermann grid illusion. In the event that these two sets differ in cardinality, Thagard's first conception will judge the Hodgkin–Huxley theory more consilient than the RGC theory, or *vice versa*. However, scientists do not often make theory choices like this. The choice is not between the Hodgkin–Huxley theory and the RGC theory. Instead, the target of scientific choices is more apt to be between Bernstein's membrane theory and the Hodgkin–Huxley theory or between the RGC theory and the S1 theory of the Hermann grid illusion. So, the first version of consilience suggests too many theory choices.

The second version, however, may not capture some consilience-based theory choices. Consider a case where FT_1 is a not proper subset of FT_2 and FT_2 is not a proper subset of FT_1 , but FT_1 and FT_2 have common members. Scientists might say of such a case that T_1 is more consilient than T_2 or that T_2 is more consilient than T_1 , but Thagard's second version of consilience does not deliver an account of that scientific judgment. Thagard's second version only applies in cases where one theory explains everything another theory explains and more. This second version, thus, suggests too few theory choices.

Now consider three positive contributions my theory of compositional abduction may make to Thagard's theory. First, there is a striking gap in Thagard's theory: there is no account of what it is for $T \cup A \cup C$ to explain a fact. The outline of singular compositional explanation in Chapters 2 and 3 may not cover everything one wants in a theory of explanation, but there is an account of at least some explanations. Second, with a theory of singular compositional abduction in hand, it is easier to see that an abductive inference stemming from the explanation of a single fact or result is not the same as Thagard's inference based on consilience. Clearly

the inference based on consilience must involve a consideration of the cardinality of two sets and the greater than relation. Singular compositional abduction need not consider these. Thagard's inference from consilience may be a "neighbor" of singular compositional abductive inferences in the sense that it may be used in conjunction with singular compositional abductive inferences, but it is not to be identified with singular compositional abduction. Thagard's inference from consilience is an "amalgamative inference" that involves considerations of multiple factors, such as what is explanatory and the cardinality of sets of explanations. Third, my account of singular compositional abduction includes an account of disconfirmation. Surely, Thagard's account of scientific theory choice should have something to say about the role of failures to explain. Thus, there are features of my compositional abductive account that would be useful additions to Thagard's view of theory choice.

8.5 Peircean Abduction and Inference to the Best Explanation

As a final point to round out the discussion of Harman (1965), I will touch on the connection between Peircean abduction and Harman's IBE. Harman begins the first section of his paper with a comment to orient his reader: "The inference to the best explanation' corresponds approximately to what others have called 'abduction,' 'the method of hypothesis,' 'hypothetic inference,' 'the method of elimination,' 'eliminative induction,' and 'theoretical inference'" (Harman, 1965, pp. 88–89). As I read this, it is not a serious bit of Peirce scholarship. It is merely a bit of stage setting.

This has not, however, kept Peirce scholars from uncharitable readings of Harman. In "How did abduction get confused with inference to the best explanation?," McAuliffe lays the blame on Harman.

Harman equates IBE with several terms that Peirce used for abduction: "The inference to the best explanation' corresponds approximately to what others have called 'abduction,' 'the method of hypothesis,' 'hypothetic inference,' 'the method of elimination,' 'eliminative induction,' and 'theoretical induction.'" (Ibid., pp. 88–89) Harman does not refer to Peirce or any other author. There is no way to determine whether Harman is referring to Peirce's earlier writings, or to Peirce's later writings, or not referring to Peirce at all. (McAuliffe, 2015, p. 306)

Part of what makes this uncharitable is that Harman clearly does not equate IBE with abduction. He says IBE "corresponds approximately" to

abduction. Another part is that it ignores the context of Harman's comment, namely, a study of the relationship between warranted enumerative induction and IBE, rather than a study of the history of philosophy. It is, of course, unclear what Harman means by "corresponds approximately," but the qualification does provide cover for Harman's orienting comment – a comment not meant to be a serious treatment of the history of philosophy. Yet a third reason the treatment is uncharitable is that it does not explore an obvious conjecture that might underlie Harman's thought about the approximate correspondence between IBE and abduction. Peirce claimed that abduction is "the operation of adopting an explanatory hypothesis" (Peirce, 1992, p. 231). Why is it not reasonable to think that the operation of adopting an explanatory hypothesis "corresponds approximately to" IBE? A charitable interpretation of Harman's text should attend to this.

McAuliffe also claims that "Contemporary philosophers of science have falsely cited Peirce's idea of abduction as a conceptual precursor to the modern notion of inference to the best explanation" (McAuliffe, 2015, p. 300).¹³ McAuliffe does not spell out why this is false. What reason is there to think that Peirce's idea of abduction is not a conceptual precursor to IBE? After all, "being a precursor to" – like "corresponding approximately" – is weaker than being identical to. Moreover, what reason is there to think that contemporary philosophers of science intend to make any claims about what Peirce believed about abduction? Contemporary philosophers of science might have an agenda that differs from the agenda of Peirce scholars.

Finally, McAuliffe claims that "Peirce defined 'abduction' and then philosophers of science took the word, assigned it a new meaning, and used Peirce's work to give IBE the illusion of a respectable pedigree" (McAuliffe, 2015, p. 310). Again, this is a surprisingly uncharitable interpretation. As McAuliffe notes, Harman (1965) does not refer to Peirce. Lipton (2003) does not discuss Peirce's view; it only refers to his collected papers. Maybe many of the references to Peirce in the IBE literature are simply nods to his philosophical contribution, rather than attempts to draw on Peircean prestige.

8.6 Summary

My goal in this chapter has not been to refute Harman's conception of confirmation. A better take is that the goal has been to show how what

¹³ Cf., Campos (2011, p. 419).

Harman said about IBE is compatible with my project. It is quite clear that his philosophical project is orthogonal to mine, so there need be no conflict. Harman was primarily concerned with the thesis that warranted enumerative induction is a special case of IBE, whereas I am concerned with the scientific interpretation of some experimental results. To this end, I have developed a theory of singular compositional abduction and reviewed its use in two cases studies. One should not assume that the IBE model Harman developed for his project is suited for my project. The key element here is that, in the interpretation of scientific experimental results, one should not assume that scientists take the confirmation of a compositional hypothesis to lie in the interpretation of a single scientific experiment.