CHAPTER 7

Peirce and the Neo-Peirceans

Although many see precursors to the idea of abduction in Aristotle's conception of *apagoge* or William Whewell's "consilience of inductions," the work of C. S. Peirce has had far and away the greater influence on philosophical thinking about abduction.[1] Indeed, there is a "Neo-Peircean" tradition that embraces many of Peirce's ideas with little question. Further, this tradition typically sets itself apart from the IBE tradition beginning with Harman (1965) and continuing most notably through Lipton (2003). This chapter focuses on the Peircean tradition, whereas the Chapters 8 and 9 focus on the IBE tradition.

I should emphasize at the outset that my goal is not to offer a critique of Peircean or Neo-Peircean views of abduction. Instead, my goal is to locate my view in philosophical space. I refer to Peirce and the neo-Peirceans to articulate my views. A primary rationale for this is that the Peircean and Neo-Peircean projects are largely orthogonal to mine. It would be a mistake to criticize Peirce and the Neo-Peirceans for not doing something they are not trying to do. Whereas my goal is characterizing how many scientists have reasoned about experimental results in twentieth-century scientific journal articles, Peirce's was not. While Peirce may have correctly articulated how he and his fellow scientists reasoned in the late nineteenth and early twentieth century, he obviously did not have in mind mid-twentieth-century scientific journals and their regimented formats for presenting experimental results. Nor did he have in mind compositional abduction. The Neo-Peircean attention to science is generally much more limited and indirect, typically by way of what other philosophers of science have had to say about science. Thus, Peirce and the Neo-Peirceans have not tried to focus their efforts on science as described in Chapter 1. Nor have they provided extended case studies of episodes in the history of science that they take to illuminate, or be illuminated by, their preferred

[1] Anderson (1986), Gabbay and Woods (2005), Bellucci and Pietarinen (2022).

conceptions.[2] Nor have they been concerned to develop a theory of explanation.

7.1 Peirce

If I were to give a succinct, rough comparison of my view with Peirce's, I would say that, whereas I accept the idea of abduction as an inference to an explanatory hypothesis, I find many of his other related ideas about abduction unhelpful for my project. A principal source of our divergences is what appears to be a bedrock Peircean assumption: scientists use abduction to introduce, but not confirm, new hypotheses.

Abduction, Deduction, and Induction. For present purposes, I will only discuss what is sometimes described as Peirce's "late" or "mature" theory of abduction. His 1901 work, "On the Logic of Drawing History from Ancient Documents," is sometimes identified as the first full treatment of this late theory.[3] Here, Peirce distinguishes between abduction, deduction, and induction both as patterns of inference and as stages in scientific inquiry. Let me begin with the description of abduction.

> Accepting the conclusion that an explanation is needed when facts contrary to what we should expect emerge, it follows that the explanation must be such a proposition as would lead to the prediction of the observed facts, either as necessary consequences or at least as very probable under the circumstances. A hypothesis, then, has to be adopted, which is likely in itself, and renders the facts likely. This step of adopting a hypothesis as being suggested by the facts, is what I call *abduction*. (Peirce, 1992, pp. 94–95, italics in original)

From this passage, I take the idea that abduction is not just any adopting of some hypothesis. It is adopting a hypothesis *because it is explanatory*. Adopting the hypothesis that sodium carries the initial inward current of the action potential, because it is a simple hypothesis would not be an abductive inference. In this passage, Peirce includes the idea that the explanandum should be "contrary to what we should expect." I do not include this in my understanding of abduction but will return to this issue.

I think Peirce and I share common ground in thinking of abduction as inferring a hypothesis because it is explanatory. Peirce next observes that it (often?) happens that for any explanation of some observation, there are

[2] The one exception may be Hanson (1972).
[3] Fann (2012, p. 32). See also Bellucci and Pietarinen (2022).

incompatible rival explanations so that a given hypothesis must be further tested and rivals eliminated:

> Aristotle had been a great reader of other philosophers, and it had struck him that there are various inconsistent ways of explaining the same facts. Ultimately, the circumstance that a hypothesis, although it may lead us to expect some facts to be as they are, may in the future lead us to erroneous expectations about other facts, – this circumstance, . . . was brought home to scientific men so forcibly, first in astronomy, and then in other sciences, that it became axiomatical that a hypothesis adopted by abduction could only be adopted on probation, and must be tested. (Peirce, 1992, p. 95)

Here again, Peirce and I share common ground. Scientists subject new hypotheses to subsequent tests. They do not merely pick one explanatory hypothesis and call an end to investigation.

As an account of this subsequent testing, Peirce offers hypothetico-deductivism:[4] "When this is duly recognized, the first thing that will be done, as soon as a hypothesis has been adopted, will be to trace out its necessary and probable experiential consequences. This step is *deduction* (Peirce, 1992, p. 95). Then, somewhat later, Peirce adds, "Having, then, by means of deduction, drawn from a hypothesis predictions as to what the results of experiment will be, we proceed to test the hypothesis by making the experiments and comparing those predictions with the actual results of experiment" (Peirce, 1992, pp. 96–97). Finally, "When, however, we find that prediction after prediction, notwithstanding a preference for putting the most unlikely ones to the test, is verified by experiment . . . we begin to accord to the hypothesis a standing among scientific results. This sort of inference it is, from experiments testing predictions based on a hypothesis, that is alone properly entitled to be called *induction*" (Peirce, 1992, p. 97). This gives us an abduction-deduction-induction conception of scientific method.

Where I break most decidedly with Peirce is in his contention that abduction and this hypothetico-deductivism are incompatible steps in a scientific method:

> The induction adds nothing. At the very most it corrects the value of a ratio or slightly modifies a hypothesis in a way which had already been contemplated as possible.

Abduction, on the other hand, is merely preparatory. It is the first step of scientific reasoning, as induction is the concluding step. Nothing has so

[4] McKaughan (2008, p. 451) concurs. See also Mohammadian (2021).

much contributed to present chaotic or erroneous ideas of the logic of science as failure to distinguish the essentially different characters of different elements of scientific reasoning; and one of the worst of these confusions, as well as one of the commonest, consists in regarding abduction and induction taken together (often mixed also with deduction), as a simple argument. (Peirce, 1992, p. 106)On my view, historians and philosophers of science do indeed need to distinguish abduction from the process of drawing out the consequences of a hypothesis and from the process of determining whether those consequences obtain. Abduction, deduction, and induction do involve distinct reasoning processes. Historians and philosophers of science should distinguish abductive, deductive, and inductive inferences. On my account, however, what Peirce describes scientists as doing in the deductive and inductive steps is, sometimes, scientists laying the groundwork for an abductive inference. Deduction and induction together are sometimes used for an abductive inference. I set out this view in Chapter 4, but I will revisit it here giving it a more Peircean gloss.

Bellucci and Pietarinen (2022) proposes a helpful example of how Peirce's three-stage process of abduction-deduction-induction works. However, I will use this example to illustrate how to reconcile my abductive approach with Peirce's comments on the deductive and inductive steps. Bellucci and Pietarinen's example begins with Peirce's early account of landing in a Turkish seaport:

> I once landed at a seaport in a Turkish province; and, as I was walking up to the house which I was to visit, I met a man upon horseback, surrounded by four horsemen holding a canopy over his head. As the governor of the province was the only personage I could think of who would be so greatly honored, I inferred that this was he. This was an hypothesis. (Peirce, 1878, p. 472)

Bellucci and Pietarinen propose that this example can be put in the "classic" 1903 schema:

> The surprising fact, C, is observed.
> But if A were true, C would be a matter of course.
> Hence, there is reason to suspect that A is true (Peirce, 1992, p. 231),

as follows

> The surprising fact that a man riding a horse surrounded by four horsemen holding a canopy over his head is observed.
> But if this man were the governor of the province, the fact that he rides horses surrounded by four horsemen holding a canopy over his head would be a matter of course.

Hence, there is reason to suspect that this man is the governor of the province. (Bellucci & Pietarinen, 2022, pp. 5–6)

Bellucci and Pietarinen interpret the conditional as stating that there is a deductive relation between the man being the governor of the province and his riding a horse surrounded by four horsemen holding a canopy over his head. Further, the deductive relation is intended to offer an explanation: the man's being the governor of the province would explain the fact that he rides horses surrounded by four horsemen holding a canopy over his head. This much is the abductive step, the "first step in inquiry."

The second step is to trace further deductive consequences or predictions of the hypothesis that the man is the governor. Bellucci and Pietarinen offer the following:

This man is the governor of the province.
Governors of the province can speak French and wear long shirts under the coat.
Hence, this man can speak French and wears a long shirt under the coat. (Bellucci & Pietarinen, 2022, p. 8)

The third and final inductive step is to check whether the man can speak French and wears a long shirt under the coat. This might be done by addressing the man in French and somehow having him raise his coat. "[I]f the predictions are fulfilled, if he can replies [sic] in French and a long shirt becomes visible under the coat, the experiment has had [a] positive result, and the hypothesis that the man is the governor of the province is confirmed (at least, provisionally)" (Bellucci & Pietarinen, 2022, p. 9). This is simple hypothetico-deductive confirmation.

In Chapter 4, I proposed that scientific hypothetical reasoning that philosophers commonly interpret as hypothetico-deductive confirmation can, at least at times, also be interpreted as abductive reasoning. It is easy to see how to do this with Bellucci and Pietarinen's example. Rearrange matters to fit Peirce's "classic" abductive schema:

The surprising fact that this man can speak French and wears long shirts under the coat is observed.
If this man were the governor of the province, the fact that he can speak French and wears long shirts under the coat would be a matter of course.
Hence, there is reason to suspect that this man is the governor of the province.

As Bellucci and Pietarinen note, for Peirce, the deductive/explanatory relation between the man's being the governor, on the one hand, and his speaking French and wearing a long shirt under his coat, on the other, is the same deductive/explanatory relation as between the man's being the governor, on the one hand, and his riding a horse surrounded by four horsemen holding a canopy over his head, on the other.[5] This enables us to use the Peircean perspective to see hypothetico-deductive reasoning as a means of serving abductive inference.

One may, of course, observe that this is Bellucci and Pietarinen's example, so they can stipulate what happens in it. They can stipulate that the "scientists" in their example really do use HD confirmation and never reason abductively. That is fine by me, since I am not ultimately concerned with imaginary cases meant to serve expository purposes. I am instead interested in how scientists have reasoned in specific historical episodes. I offer a different theory of some of the actual scientific practices. So, let me review some real historical, scientific examples using the Peircean framing of the issues.

Consider representing Baumgartner's 1960 reasoning regarding the Hermann grid illusion and retinal ganglion cells according to Peirce's 1903 schema:

> The surprising fact that scintillating smudges are found in the Hermann grid is observed.
> But if RGCs implement the illusory perception, the fact that scintillating smudges are found in the Hermann grid would be a matter of course.
> Hence, there is reason to suspect that that RGCs implement the illusory perception.

Now consider Spillman's 1971 reasoning. Spillman envisioned that Baumgartner's hypothesis predicts that reducing the bar sizes would weaken the intensity of the scintillating smudges, so he did engage in what Peirce would call deductive-inductive reasoning. Yet, this reasoning serves to set up a further abductive inference that can be fit within Peirce's 1903 schema:

> The surprising fact that scintillating smudges in the Hermann grid are weaker when the bars are shortened is observed.

[5] Bellucci and Pietarinen (2022, p. 8).

https://doi.org/10.1017/9781009435710.008 Published online by Cambridge University Press

But if RGCs implement the illusory perception, the fact that scintillating smudges in the Hermann grid are weaker when the bars are shortened would be a matter of course.
Hence, there is reason to suspect that RGCs implement the illusory perception.

This story respects much of what Peirce says about abduction-deduction-induction and scientific method. Spillmann did not simply accept Baumgartner's RGC hypothesis. Instead, he reasoned out one of its consequences, then performed an experiment to check the prediction and the experiment verified the prediction. Yet, I differ from Peirce in admitting that both Baumgartner's and Spillmann's inferences illustrate the idea of inferring some hypothesis because of what that hypothesis explains. This is why I say that scientists sometimes use abduction not merely to introduce some hypothesis, such as the RGC hypothesis. Instead, they may also confirm some hypothesis using abduction. Hypothetical reasoning does not conflict with abductive reasoning. Instead, hypothetical reasoning can set the stage for abductive reasoning.

Surprising facts. Peirce might object to the foregoing interpretation of hypothetico-deductivism as abductive on the grounds that, in the hypothetico-deductive phase, there is no surprising fact to be explained. Peirce might say that, in Bellucci and Pietarinen's example, it is not surprising that the man can speak French and wears long shirts under the coat, since this was a consequence deduced from the hypothesis that he was a governor. In the Spillman example, it is not surprising that scintillating smudges in the Hermann grid are weaker when the bars are shortened is observed, since this is a consequence of the RGC hypothesis. The time is now ripe to return to the issue of "surprising facts."

I noted above that, in "On the Logic," Peirce includes the idea that the explanandum should be "contrary to what we should expect." The more famous formulation of the idea is from the 1903 schema wherein the explanandum is "surprising." Adolfas Mackonis argues that "Peirce stated that abduction begins from a surprise. . . Thus, neither abduction nor IBE can be conducted if there is no surprising fact to be explained" (Mackonis, 2013, p. 977). Yet, Mackonis must have a suppressed premise linking what Peirce claimed about abduction and what is true of abduction. The obvious candidate is "If Peirce stated that abduction begins from a surprise, then neither abduction nor IBE can be conducted if there is no surprising fact to be explained." But why believe this premise? Is the assumption here that abduction just is what Peirce says it is?

Jaime Nubiola has gone as far as proposing that abduction is the logic of surprise.[6] Of the 1903 schema, Nubiola writes, "This is the logical structure of all abductions. The key for understanding it properly is to realize that the trigger of abduction is the surprising character of the fact referred to in the first premise" (Nubiola, 2005, p. 126). For my part, I am willing to allow that Peirce's 1903 schema captures the logical structure of all *Peircean* abductions and that the key for understanding *Peirce's* conception of abduction properly is to realize that the trigger of abduction is the surprising character of the fact C. In this book, however, I am proposing a conception of abduction that does not presuppose this feature.[7] Nubiola might not count my "thin" conception of abduction as a Peircean conception. He might – like Mackonis – not count my "thin" conception of abduction as a conception of abduction at all. Nevertheless, I persist in this conception because my non-Peircean conception of abduction better describes the cases I examine.

Consider the point more concretely. Scientists often design experiments with preconceptions regarding the future results of those experiments. They may do this after what Peirce described as deductive reasoning. Scientists are guided in these preconceptions by their theories. In many of these cases, they are not surprised by the results. Thus, after learning of the results of Blodgett's experiment with the six-unit T-maze, Tolman was probably not surprised to find similar results with the fourteen-unit T-maze. Given the latent learning in the six-unit T-maze, Tolman was not surprised to find latent learning in the fourteen-unit T-maze. Similarly, given the currents discovered in their first two experiments on the sodium hypothesis in Hodgkin and Huxley (1952a), Hodgkin and Huxley had some definite ideas about what would happen in their third experiment wherein the axon was voltage-clamped in a medium containing 10% and 30% of the normal sodium of content of seawater.

Tolman believed that the rat navigates with a cognitive map, since that would compositionally explain the rat's performance in the maze. This inference is (thin) compositional abduction. Hodgkin and Huxley believed that sodium carries the initial inward current of the action potential, since that would compositionally explain the currents in the reduced sodium media. This inference is (thin) compositional abduction. The theory of compositional abduction captures this feature of Tolman's and Hodgkin and Huxley's scientific writing.

[6] Nubiola (2005). [7] Gabbay and Woods (2005, p. 85) drop the surprise condition as well.

Abduction is for Generating Hypotheses; IBE is for Confirming Hypotheses. There is a tradition in Peircean and Neo-Peircean scholarship to propose, to a first approximation, that Peirce's concept of abduction assumes that abduction is for introducing/generating hypotheses, whereas Harman's or Lipton's IBE is for hypothesis confirmation.[8] This claim is often juxtaposed with the idea that abduction – with no mention of Peirce – is for introducing/generating hypotheses, whereas IBE – with no mention of Harman or Lipton – is for hypothesis confirmation.[9] So, for example, Daniel Campos writes,

> I argue against the tendency in the philosophy of science literature to link abduction to the inference to the best explanation (IBE), and in particular, to claim that Peircean abduction is a conceptual predecessor to IBE. This is not to discount either abduction or IBE. Rather the purpose of this paper is to clarify the relation between Peircean abduction and IBE in accounting for ampliative inference in science. This paper aims at a proper classification – not justification – of types of scientific reasoning. In particular, I claim that Peircean abduction is an in-depth account of the process of generating explanatory hypotheses, while IBE, at least in Peter Lipton's thorough treatment, is a more encompassing account of the processes both of generating and of evaluating scientific hypotheses. There is then a two-fold problem with the claim that abduction is IBE. (Campos, 2011, p. 419)

Campos is clearly shifting back and forth between writing about abduction and about Peircean abduction. Of course, insofar as this is a mere stylistic shift, there is no philosophical issue at stake. And plenty of scholars are careful not to make this stylistic shift.[10] There is, however, a philosophical issue if one assumes that Peirce's concept of abduction somehow constrains any theory of abduction. There is a philosophical issue if one contends that abduction just is what Peirce says it is. This is an assumption that requires further argumentation than it has been given in the literature.[11] I am happy to allow that my conception of abduction differs from Peirce's – maybe it is a "non-Peircean" concept of abduction, but it is quite another matter to hold that what I call abduction is not abduction at all.

[8] McKaughan (2008) proposes a third alternative: abduction introduces hypotheses worthy of pursuit. See also Nyrup (2015).

[9] There are other ways of reading Peirce as well. McKaughan (2008) proposes a third option, that abduction introduces hypotheses as worthy of pursuit. Magnani (2001, p. 25) suggests, without textual references, that Peirce sometimes treated abduction as for both hypothesis generation and hypothesis confirmation.

[10] McKaughan (2008), Cabrera (2022).

[11] See, for example, McAuliffe (2015) and Mohammadian (2021).

More concretely, one might think that since abduction is for hypothesis generation/ introduction, whereas IBE is for hypothesis confirmation, the very idea of abductive inference for hypothesis confirmation is a conceptual confusion. My reply is that I am not using the Peircean concept of abduction. I am developing another one – taking one element from the Peircean view – that I believe supports a more detailed and accurate description of scientific reasoning in the primary experimental literature than does the Peircean conception. I am not confused at all.

One might, however, object that my terminology is confusing to others. Yet clearly the terminology is only potentially confusing to those who are resolutely locked into the picture of abduction for hypothesis generation and IBE is for hypothesis confirmation. Philosophers of science outside the Neo-Peircean tradition should not be confused. Further, suppose one holds the view that abduction is for hypothesis generation and IBE is for hypothesis confirmation. Nevertheless, if one can at least conceive that some philosophers hold a different view and argue for that different view, then there should be no confusion.

It should be emphasized that almost all the discussion of this topic in Peirce scholarship focuses on comparing Peircean ideas with one or another conception of IBE. No serious consideration is given to the extent to which either conception correctly describes scientific practice. There is no serious attempt to develop a science-first interpretation of abduction or IBE. This is one reason why the project of understanding Peirce's view need not compete with my project of describing how scientists confirm compositional hypotheses.

7.2 Neo-Peirceans

I introduce "Neo-Peircean" as a term for those philosophers who begin with Peirce's idea of abduction as a means of hypothesis generation/ introduction. A Neo-Peircean might then show how Peirce's concept applies in one or another area of intellectual activity. A Neo-Peircean might introduce additional distinctions. Or a Neo-Peircean may try to build a formal theory of Peircean abduction. Insofar as these approaches address scientific practice – which they often do not – they embody a "philosophy first" approach, taking pre-existing philosophical ideas and applying them to scientific cases. This approach is diametrically opposed to the science-first approach I have taken in Parts 1 and 2 of this book. Little, if any, of the "philosophy first" work shows how Neo-Peircean ideas apply in detailed case studies. Moreover, none of this Neo-Peircean work

develops a theory of explanation. I would classify most papers in the monumental *Handbook of Abductive Cognition* as "Neo-Peircean."[12] Rather than attempting to discuss the large Neo-Peircean literature, I will instead briefly touch on two prominent Neo-Peircean contributions, namely, Lorenzo Magnani's *Abduction, Reason, and Science* (Magnani, 2001), and Gabbay and Woods's *The Reach of Abduction* (Gabbay & Woods, 2005).

7.2.1 Magnani

Magnani (2001) is a paradigm of Neo-Peircean work. Following Peirce, Magnani assumes that abduction is for hypothesis generation. Chapter 1 of his book sets the stage for this by reviewing Platonic, Kantian, and Polanyian philosophical theories of hypothesis generation. Magnani's Chapter 2 reviews, among other things, a version of the familiar idea of abduction for hypothesis generation, IBE for hypothesis confirmation, with Magnani preferring abduction for hypothesis generation. It also embraces a form of hypothetico-deductivism.

Chapter 2 introduces what is widely considered one of Magnani's most important distinctions, between selective and creative abduction. Both abduction and IBE can be classified as either selective or creative. In brief, a selective inference selects some hypothesis from an existing stock of possible explanations. So, in Magnani's preferred context of medical diagnosis, a physician might use either abduction or IBE to select some hypothesis H_i from a pre-existing set of possible hypotheses, H_1, H_2, ..., H_n, because H_i explains the symptoms some patient displays. By contrast, in a creative abduction, a physician/scientist might use either abduction or IBE to advance a new hypothesis regarding a new disease that explains some patient's symptoms.

Let me explain why Magnani's selective/creative distinction is of limited use for my project. Consider, first, the Hodgkin–Huxley case study. By 1947, Hodgkin and Huxley had arrived at the hypothesis that sodium ions carried the initial inward current of the action potential. Suppose for the sake of argument that Hodgkin and Katz's replication of the Overton experiment was creative. Once that was performed there were additional experiments, the experiments with reduced-sodium and enriched-sodium media, that were not creative. They were not creating a new hypothesis. It had already been created. So, creative abduction for hypothesis

[12] Magnani (2023).

https://doi.org/10.1017/9781009435710.008 Published online by Cambridge University Press

generation does not illuminate why Hodgkin and Huxley did the reduced-sodium and enriched-sodium experiments. Further, neither selective abduction nor selective IBE applies, as the sodium hypothesis had already been selected.

Next, consider again Baumgartner's 1960 reasoning and Spillmann's 1971 reasoning. Suppose, for the sake of argument, Baumgartner offered a creative abductive inference generating the hypothesis that RGCs implement the grid illusion. Spillmann subsequently tested the hypothesis by reasoning hypothetically that the grid with shorter bars would produce a weaker illusion. This prediction was confirmed. I have proposed this hypothetical reasoning to be understood as compositional abduction. Yet, Spillmann's reasoning was neither creative nor selective. The RGC hypothesis had already been introduced by Baumgartner and Spillmann was not trying to select the RGC hypothesis from rivals.

Finally, consider Tolman's reasoning about the six-unit and fourteen-unit T-mazes. Suppose, for the sake of argument, that the cognitive map hypothesis was introduced in response to the six-unit maze results. When Tolman and his colleague did the replication with the fourteen-unit maze, they did not introduce a creative new hypothesis to explain the results. They were further testing the cognitive map hypothesis. Moreover, they were not selecting the cognitive map hypothesis from among rivals. They had already fixed upon the cognitive map hypothesis.

Next, consider a second of Magnani's major distinctions, the distinction between theoretical and manipulative abduction. He writes, "theoretical abduction is the process of *inferring* certain facts and/or laws and hypotheses that render some sentences plausible, that *explain* or *discover* some (eventually new) phenomenon or observation; it is the process of reasoning in which explanatory hypotheses are formed and evaluated" (Magnani, 2001, pp. 30–31, italics in original). This looks to be a standard understanding of abduction as the process of inferring some hypothesis because it is explanatory, albeit allowing that this inference may be formative or evaluative. Although Magnani introduces the term "manipulative abduction" early in his book and comments on it at several points, he only begins articulating what he means by it in Chapter 3. Magnani's description of manipulative abduction is less clear to me, so I quote him at length:

> Manipulative abduction . . . happens when we are thinking through doing and not only, in a pragmatic sense, about doing. So the idea of manipulative abduction goes beyond the well-known role of experiments as capable of forming new scientific laws by means of the results (the nature's answers to the investigator's question) they present, or of merely playing a predictive

> role (in confirmation and in falsification). Manipulative abduction refers to an extra-theoretical behavior that aims at creating communicable accounts of new experiences to integrate them into previously existing systems of experimental and linguistic (theoretical) practices. (Magnani, 2001, p. 66)

I take from this that a manipulative abduction is an inference that happens more or less spontaneously, as when scientists physically manipulate objects in the laboratory.

Here again, this distinction is largely orthogonal to my project. Given a forced choice between Magnani's categories of theoretical and manipulative abduction, I would have to say that all my examples are instances of theoretical abduction. The scientists who I have discussed may well have engaged in manipulative abduction in the laboratory, but my project is not intended as an account of the psychology of laboratory life. It is more like a sociological account of how scientists interpret certain experimental results to support hypotheses in the primary experimental literature. What makes it sociological, rather than psychological, is that it is shaped by social conventions about how scientists present experimental results and interpret them for their fellow scientists within the confines of relatively regimented scientific journal articles. This reasoning is somewhat removed from a completely faithful recounting of scientific thinking in the laboratory. Part of the burden of Chapter 1 was to emphasize this feature of my account.

The foregoing is not, of course, a critique of Magnani's project. He has not directed his efforts as I have. He takes on a lot of the Peircean framework and adds some further refinements, but he is not engaged in a science-first project in the philosophy of science. He is not, in the first instance, aiming to use scientific examples to build a theory of compositional abductive inference. Instead, Magnani tells us that "the aim of this book is to emphasize the significance of abduction in order to illustrate the problem-solving process and to propose a unified epistemological model of scientific discovery ..., diagnostic reasoning ..., and other kinds of creative reasoning (Magnani, 2001, p. 15). Magnani's stated aim differs from mine at multiple levels. It should be no surprise, therefore, that the conceptual tools Magnani develops for his project are of little use to mine.

7.2.2 *Gabbay and Woods*

Gabbay and Woods (2005) is a monumental contribution to the logic of abduction. Moreover, it begins with a promising nod to what looks like a science-first philosophy of science and logic of abduction: "In recent

decades, philosophy of science has evolved from its original stance to its present non-foundational or *neoclassical* [stance] . . . Accordingly, the flow of inquiry is bottom-up, from science, to the philosophy of science, to the logic of science" (Gabbay & Woods, 2005, p. 4). Yet, quite quickly, the science-first approach is abandoned. When describing the targets of their formalization, Gabbay and Woods write, "In a rough and ready way, these can only be what is known informally about how agents actually reason. In a well-entrenched analytic tradition, inputs are made accessible by the theorist's intuitions about the subject at hand, which stand as his *data* for a theory of it. Intuitions are the conceptual data for theory, the inputs to the theory's formal models" (Gabbay & Woods, 2005, p. 4). Yet, neither a logician nor a historian and philosopher of science needs to rely on what is informally known about how agents actually reason. The input to the theory need not be a theorist's "intuitions." Instead, one might make a close study of actual scientific reasoning, maybe using cognitive science or history and philosophy of science. Perhaps looking to cognitive science or history and philosophy of science only leaves one with an "informal" account, but it is also not merely looking to a logician's intuitions. So, right away, Gabbay and Woods undertake a project that is orthogonal to mine. It is not a science-first project.

A second feature of Gabbay and Woods's project is that they aspire to something more general than a logic of scientific reasoning. "As conceived of in the neoclassical tradition, the logic of science provides idealized models of the most general and central features of the conceptual content of the process of scientific reasoning. If we remove the qualification 'scientific', the logic which the new classical logic of science exemplifies resembles our own more general conception of logic" (Gabbay & Woods, 2005, p. 4). Gabbay and Woods aspire to something like a "general theory of reasoning." My project is near the opposite end of a spectrum. As I have been at pains to note at many points, singular compositional abduction is only one form of scientific reasoning. It is, indeed, only one form of abductive reasoning. Moreover, this book offers a granular reading of some of the scientific literature, focusing on details of the scientific interpretation of specific experimental results. Indeed, one might naturally view this granular work, rather than a logician's intuitions, as a reasonable starting point for developing a logic of abduction. In principle, the specific scientific cases might be a basis for some sort of generalization to the philosophy of science, then to the logic of science, and then to logic in general.

This drive for generality creates some tension for historians and philosophers of science trying to understand the reasoning by Baumgartner,

Hodgkin, Huxley, Spillmann, and Tolman. On the one hand, Gabbay and Woods propose that abduction as an inference to an explanatory hypothesis is a mere special case of abduction.[13] This special case is what they call explanatory abduction. For Gabbay and Woods, abductively inferred hypotheses are pragmatically justified for various reasons, only one of them being that the hypothesis is explanatory. This is a manifestation of a drive for generality. It suggests that the reasoning by Baumgartner, et al., will fall out as a very special case of abduction. On the other hand, Gabbay and Woods also propose a strong narrowing of the concept of abduction: "As we see things here, the fundamental fact about abduction is that abduction is *ignorance-preserving reasoning*" (Gabbay & Woods, 2005, p. 40). As a conceptual point, abduction cannot lead to knowledge. So, we may face a forced choice: either Hodgkin and Huxley did not know that the sodium carries the initial inward current or they were not using abduction in the Gabbay and Woods sense. Maybe the simplest resolution is to say that Hodgkin and Huxley were not using Gabbay–Woods abduction.

Some of the Gabbay–Woods discussion of explanatory abduction has an interesting feature for the present work. (Not the discussion of explanation, which is largely a review of the DN model and some familiar alternatives.) Gabbay and Woods introduce a schema for explanatory abduction along the following lines:

1. E
2. It is not the case that K $\nrightarrow$ E.
3. It is not the case that H $\nrightarrow$ E.
4. K(H) is consistent
5. K(H) is minimal
6. K(H) $\nrightarrow$ E.
7. Therefore H (cf., Gabbay & Woods, 2005, pp. 48–49),

Where E stands for some evidence, K is a set of background beliefs, H is a putative explanatory hypothesis, and $\nrightarrow$ is an explanatory consequence relation. What is interesting is that Gabbay and Woods treat entailment as one instance of the explanatory consequence relation, so that their take on explanatory abduction looks very much like a version of HD confirmation.[14] Of course, on my account, the mere logical features of this schema and of HD confirmation do not capture the worldly thinking that I propose engages practicing scientists.

[13] Gabbay and Woods (2005, p. 40). [14] See, for example, Glymour (1980b).

Yet again, as with Magnani's treatment of abduction, Gabbay and Woods do not develop a set of tools germane to my project. They do not seriously look to scientific practice. Nor do they develop a theory of explanation. While one might hope that the formal tools would offer some theoretical clarity, the foundational assumption that abduction is ignorance-preserving means that the account fails to capture one of the key features of the scientific use of explanation to confirm scientific hypotheses.

7.3 Summary

The gist of this chapter has been to argue that there is but a slender connection between my work and that of Peirce and the Neo-Peirceans. This connection is captured in the idea that abduction is an inference to some hypothesis because of what that hypothesis explains. Whereas I believe that compositional abduction is sometimes used for scientific confirmation, Peirce and the Neo-Peirceans believe abduction is for hypothesis introduction. This difference has further consequences for the features one takes abductive inference to have. Most notable are the questions of how historians and philosophers of science are to understand scientific hypothetical reasoning, how surprising the abductive explananda are, how novel the explananda are, and what confirmation, if any, is provided by an abductive inference.

As a final word, one might suggest that I not use Peirce's word "abduction." Using "abduction" is confusing. Yet, a new word choice has its challenges. Maybe call the view "explanationism." Even with the new term, the question would still arise regarding how explanationism relates to the Peircean concept of abduction. One might also object that this new terminology slights Peirce by not acknowledging his role in championing the idea of an inference to some hypothesis because of what that hypothesis explains. Further, one might ask how my explanationism relates to the Harmanian/Liptonian concept of IBE. Maybe there is still another term I could use, but the principal issue remains. How does my concept relate to Peirce's abduction and Harman's IBE? There is no avoiding the project of philosophical orientation. I stand by "abduction" as a sensible terminological choice.