

Chapter 2

On Classifying Abduction

1 Introduction

We can witness the recent surge of interest in classifying different patterns or types of abduction. Many philosophers, including Thagard, Magnani, Gabbay and Woods, Schurz, and Hoffmann, have suggested their own classifications emphasizing different aspects of abduction (Thagard 1988; Magnani 2001, 2009; Gabbay and Woods 2005; Schurz 2008; Hoffmann 2011). Such a development is remarkable, in view of the fact that until quite recently the focus of the research on Peircean abduction was basically to identify its logical form (Kapitan 1997). In one sense, it is strange that after all these years abduction is still unfamiliar to the general public: not on a par with deduction and induction. In another sense, however, it is also remarkable that abduction has been extensively studied by artificial intelligence researchers, cognitive scientists, and semioticians as well as by philosophers of science. No doubt, the diverse attempts in classifying abduction must represent the fact that the current level of the study of abduction is pretty high.

It is Schurz (2008) that could be a nice point of departure for useful comparisons of all these classifications, for he provides us with a rather extensive list of different “patterns of abduction”. In terms of what entity is to be abducted, he distinguished between more than ten different patterns of abduction, grouped under 4 categories: i.e., factual abduction, law-abduction, theoretical model abduction, and second-order existential abduction. One significant point in Schurz’s classification is that he emphasizes the importance of second-order existential abduction, paying special attention on common cause abduction. Largely concurring with Schurz, Hoffmann further expands the list into a table by adding what he calls “meta-diagrammatic abduction” as a fifth category (Hoffmann 2011, 581). Unlike Schurz, however, Hoffmann highlights what he calls “theoretical-model abduction” as a precondition for any abduction (Hoffmann 2011, 580). Schurz’s and Hoffmann’s classifications of abduction basically concern its “explanatory” character. They do not consider the problem of non-explanatory abduction, which is instead illustrated by Gabbay and

Woods when distinguishing between AKM models of abduction and their own GW model (Gabbay and Woods 2005, p. 49, n. 8).¹ Further, even Magnani not only concedes the existence of non-explanatory and instrumental abduction but also presents some extremely interesting examples. According to him, some cognitive processes that are occurring in mathematics are cases of non-explanatory abduction [cf. his study of “mathematical intuition” in Gödel’s sense (Magnani 2009, p. 72)].

In Sect. 2, I shall discuss two of the most controversial philosophical issues involved in these attempts, i.e., (1) Is classifying abduction compatible with the search for the logical form of abduction, and (2) could there be any non-explanatory abduction? In order to resolve these two closely related and potentially controversial issues, I propose to examine in Sect. 3 how Peirce would have responded to them. In particular, I suggest to do this in connection with Peirce’s other life-long project, the classification of sciences. In this examination, it will be shown that Peirce struggled with the problem of conflating induction and abduction. I shall discuss how this problem influenced both Peirce’s views on the interrelationship between abduction, deduction, and induction on the one hand, and his many classifications of sciences on the other. Finally, in Sect. 4, I shall discuss the classical problem of abduction as an inference to the best explanation. Insofar as this problem has bearing on the two controversial issues in classifying abduction, my negative answer to this problem will demonstrate that classifying abduction is yet to get off the ground.

2 Some Recent Attempts to Classify Abduction

2.1 *Is Classifying Abduction Compatible with the Search for the Logical Form of Abduction?*

Until quite recently, the focus of philosophical discussions of abduction was its logical form. Kapitan, for example, differentiates five forms or schemata of abduction based on broadly developmental approach to Peirce’s own writings (Kapitan 1997, pp. 480–488). However, Schurz criticizes this majority approach “aimed at one most general schema of abduction (for example IBE) which matches every particular case” as hopeless, for he writes:

I do not think that good heuristic rules for generating explanatory hypotheses can be found along this route, because these rules are dependent of the *specific type* of abduction scenario, for example, on whether the abduction is mainly selective or creative (etc.)” (Schurz 2008, 205; Emphasis is Schurz’s).

¹‘A’ for Aliseda, ‘K’ for Kowalski, Kuipers, and Kakas et al., and ‘M’ for Magnani, and Meheus et al. in AKM model, and ‘G’ for Gabbay, and ‘W’ for Woods in GW model.

As Schurz makes explicit, the distinction between selective and creative abduction originates from Magnani (2001).² Magnani likewise seems to endorse Schurz's "new route to abduction", for he counts Schurz (2008) as extending his own ideas by a classification of patterns of creative abduction (Magnani 2009, p. 12, note 18).

Contrary to appearances, Magnani's distinction between selective and creative abduction may not be entirely clear. Not to mention Jon Williamson, who finds "little substance to the selective/creative distinction" (Williamson 2003, 356). There may be some potential disagreements even between Magnani and Schurz in understanding this distinction. For example, Schurz claims that "[w]hile selective abduction has dominated the literature, creative abductions are rarely discussed, although they are essential in science" (Schurz 2008, 201). As he continues to emphasize, the primary contribution of Schurz (2008) must be found in his introduction and detailed discussion of several kinds of creative abduction (*ibid.*). Inadvertently, however, Schurz could have been ignoring or underestimating Magnani's somewhat extensive contribution to creative abduction. For Magnani has written extensively about creative model-based abduction and manipulative abduction (Magnani 2001, p. 38f., 2009, p. 31f.). Indeed, it is hard to find in Schurz (2008) any discussion of Magnani's distinction between theoretical and manipulative abduction or that between sentential and model-based, even though these are at least equally as important to Magnani as his selective/creative distinction (Magnani 2001, pp. 19–23, 39, 53, 2009, pp. 10–12, 31, 39).

It is certainly worthwhile to take a view of Magnani's multiple distinctions of abduction: (1) selective/creative; (2) theoretical/manipulative; and (3) sentential/model-based. Above all, our focal interest lies in understanding the relationships between these three distinctions. The most revealing seems to be the following text:

What I call *theoretical abduction* certainly illustrates much of what is important in creative abductive reasoning, in humans and in computational programs, especially the objective of selecting and creating a set of hypotheses (diagnoses, causes, hypotheses) that are able to dispense good (preferred) explanations of data (observations), but fails to account for many cases of explanations occurring in science and in everyday reasoning when the exploitation of environment is crucial. ...I maintain that there are two kinds of theoretical abduction, "sentential", related to logic and to verbal/symbolic inferences, and "model-based", related to the exploitation of internalized models of diagrams, pictures, etc., cf. below in this chapter (cf. Fig. 1.2) (Magnani 2009, p. 11).

This text is important because it presents a tentative definition of theoretical abduction and the subdivision of theoretical abduction into sentential and model-based abductions. The passage above says that theoretical abduction can be a kind of creative reasoning (not only selective, as in diagnosis).

²For Magnani, it is important to distinguish between selective abduction that merely selects from an encyclopedia of pre-stored hypotheses, and creative abduction abduction that generates new hypotheses (Magnani 2001, 2009).

Insofar as Magnani views theoretical abduction also as a kind of creative abduction, and again insofar as he tries to distinguish between theoretical and manipulative abductions, he must also view manipulative abduction as a kind of creative abduction. Magnani introduces the concept of manipulative abduction as follows:

The concept of *manipulative abduction* captures a large part of scientific thinking where the role of action is central, and where the features of this action are implicit and hard to be elicited: action can provide otherwise unavailable information that enables the agent to solve problems by starting and by performing a suitable abductive process of generation or selection of hypotheses (Magnani 2009, p. 12; See also Magnani 2001, pp. 15, 16, 43).

For my present purpose, the following text is more informative:

Manipulative abduction (Magnani 2001)—contrasted with theoretical abduction— happens when we are thinking through doing and not only, in a pragmatic sense, about doing. ... 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. 53, 2009, p. 39).

For, as is clear from this quote, manipulative abduction is contrasted with theoretical abduction by Magnani.

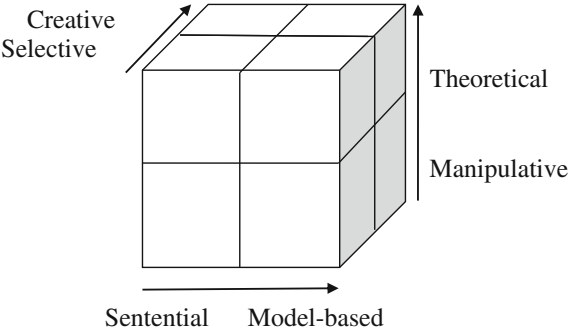
Further, in Magnani’s writings it is also stresses that manipulative abduction is occurring taking advantage of those model-based (for example iconic) aspects that are embedded in external models:

We have seen that manipulative abduction is a kind of abduction, usually model-based and so intrinsically “iconic”, that exploits external models endowed with delegated (and often implicit) cognitive and semiotic roles and attributes (p. 58).

This line of thought indicates clearly a possibility that Magnani’s multiple distinctions of abduction may work in such a way that each distinction represents a different dimension in our understanding of abduction. What I have in mind might be presented crudely as in the Cubic Model of Magnani’s classification of abduction (see Fig. 1).

It is important to report this passage by Magnani (2009, p. 40), which explains the “cognitive constraints” of the distinction between theoretical and manipulative

Fig. 1 Magnani’s classification of abduction: cubic model



abduction: “I would like to reiterate that it is important to note that my epistemological distinction between theoretical and manipulative abduction is based on the possibility of separating the two aspects in actual cognitive processes, relying on the differentiation between off-line (theoretical, when only inner aspects are at stake) and on-line (manipulative, where the interplay between internal and external aspects is fundamental). As Wheeler has recently observed, some thinkers like Esther Thelen and Andy Clark have raised doubts about the on-line/off-line distinction “[...] on the grounds that no intelligent agent is (they claim) ever wholly on-line or wholly offline” (Wheeler 2004, p. 707, footnote 14) I contend that, even if manipulative/online cases exist in great numbers, there are also cognitive processes that seem to fall into the class of off-line thinking, as we can simply introspectively recognize. Anyway, the distinction above is always rewarding from the epistemological perspective as a way of classifying and analyzing different cognitive levels, and it is endowed with an indisputable conceptual and explanatory usefulness”.

There is no doubt that Magnani’s distinction between selective, creative, theoretical, and manipulative abduction is a major event in the history of the problem of abduction. Nor can it be disputed whether Schurz’s elaborate identification of the patterns of abduction deserves due credit.

According to Schurz, “the evaluation criteria for abductions are different for different kinds of abductions” (Schurz 2008, 203). As a consequence, there is no general answer to questions related to “(1) *minimal criteria* for the *acceptability* of scientific abductions, and (2) *comparative criteria* for the *quality* of the abduced explanations” (Schurz 2008, 203; Emphasis is Schurz’s). In fact, he values as more important the particular results, in which he models each kind of abduction as a specific schema, than his general theses and results (Schurz 2008, 205).

It is not clear whether Schurz believes that there is no such thing as the logical form of abduction. Would he claim that, even if there is a singular logical form, his new route identifying specific schemata for different kinds of abduction is more promising than the previous route aiming at the general schema of abduction for capturing it? Does he believe that it is someone else’s burden to prove the existence of the logical form of abduction? Be that as it may, if there is no logical form but merely different kinds of abduction, as Schurz claims, why should there be kinds of abduction after all? Even before applying his evaluation criteria for abduction depending on the different kinds of abduction, shouldn’t we be able to judge that the case under issue is an instance of abduction first?

Some examples of non-explanatory abduction cited by Gabbay and Woods, and Magnani can be ascertained from the following quotes from Magnani (2009): “Non-explanatory modes of abduction are clearly exploited in the ‘reverse mathematics’ pioneered by Harvey Friedman and his colleagues, e.g., Friedman and Simpson (2000), where propositions can be taken as axioms because they support the axiomatic proofs of target theorems. The target of reverse mathematics is to answer this fundamental question: What are the appropriate axioms for mathematics? The problem is to discover which are the appropriate axioms for proving particular theorems in central mathematical areas such as algebra, analysis, and

topology (cf. Simpson 1999). The idea of reverse mathematics originates with Russell's notion of the regressive method in mathematics (Russell 1973), and is also present in some remarks of Gödel (1990, 1944). Magnani (2009, p. 72); "The action at-a-distance equation serves Newton's theory in a wholly instrumental sense. It allows the gravitational theory to predict observations that it would not otherwise be able to predict" (Gabbay and Woods 2005, pp. 118–119). In this case Newtonian explanations are seen as epistemically agnostic conjectures, that is they lack the classical epistemic virtues envisaged by the neopositivistic tradition. These abductions are secured by instrumental considerations and accepted because doing so enables one's target to be hit. They cannot be discharged because of their possible implausibility, for example on the basis of empirical disconfirmation. We have to note that in some sense all abductions embed instrumental factors. In the general case, one accepts because doing so enables ones target to be attained, notwithstanding that lacks the relevant epistemic virtue. However, in cases such as Newton, is selected notwithstanding that it is considered to be epistemically hopeless (Gabbay and Woods 2005, p. 119) call this extreme kind of abduction radically instrumental" (Magnani 2009, p. 77).

I initially thought that there are some potentially important disagreements between Magnani and Schurz in their understanding of selective/creative distinction. And, I counted it as indicating the possibilities of some other discrepancies between Magnani and Schurz, which might have far-reaching implications. For, unlike Schurz, who gives up once and for all the search for a logical form of abduction, Magnani might still want to pursue it. Of course, Magnani does not think there is the logical model of abduction but many logical models depending on tools and targets used. According to him, we can use not only classical logic (and in this case abduction is a fallacy) but also non-monotonic logics, adaptive logics, semantic tableaux, and many others: "We can also model abductive reasoning with other conceptual tools such as statistics, probabilistic networks, neural nets (and in many cases we can use these models (and also the logical ones) to build computational AI programs that render various kinds of abductive reasoning (selective, creative, visual etc.)" (Magnani 2013, Personal communication). Nevertheless, I think that such a stance does not necessarily force Magnani to give up once and for all the search for the logical form of abduction. No matter what model one uses, I think, each and every one of those models intends to capture the logical form of abduction. If not, what is the point of calling them models of abduction? But it is by now clear that Magnani is, not unlike Schurz, somewhat pessimistic about the possibility of securing the logical form of abduction.

Then, what was Magnani's motivation for his distinction between the selective and the creative abduction, which apparently presents Schurz a springboard for his meticulous classification of the patterns of abduction? Would Magnani also have to give up the search for the logical form of abduction? Was he self-consciously distancing himself from the majority's approach by the distinction between the selective and the creative abduction? I think not. He may not see any conflict between the search for the logical form of abduction and the attempts to differentiate the patterns of abduction. As we saw above, Magnani introduces many

intriguing and useful distinctions other than that between the selective and the creative abduction in his extensive discussions of abduction. Then, for what reason does he employ all these distinctions in his discussion of abduction? There seems no other possible answer than that Magnani introduces all those distinctions in order to capture the essence of abduction, thereby, finally, further chances for refining *the* logical form of abduction. As was pointed out above, Magnani would not agree with my understanding. It might sound strange, but contrary to his own intention, those distinctions themselves might be after *the* logical form of abduction (see Magnani 2015).

2.2 *Could There Be Any Non-explanatory Abduction?*³

How many different ways did Peirce try to define abduction? I do not know. Probably, there are more well-known and widely cited ones among them. Here is one:

Abduction is the process of forming an explanatory hypothesis. It is only logical operation which introduces any new idea; for induction does nothing but determine a value, and deduction merely evolves the necessary consequences of a pure hypothesis (Peirce 1903, CP 5.171).

Relying on this definition, some scholars have recently tried to confine abduction to only explanatory abductions. Hoffmann is a clear example, for he writes:

I think it is important to limit abduction to those forms of reasoning that generate “explanatory hypotheses,” as Peirce says in the first sentence of the quote above (Hoffmann 2011, 571).

On the other hand, there are other scholars, who do allow and discuss explicitly some non-explanatory abductions, such as instrumental abduction (Gabbay and Woods 2005; Magnani 2009). Magnani writes:

Gabbay and Woods (2005) contend – and I agree with them – that abduction *is not intrinsically explanationist*, like for example its description in terms of inference to the best explanation would suggest. Not only that, abduction can also be merely *instrumental*. This

³One anonymous reviewer points out that my presentation of different views about the possibility of non-explanatory abduction as if there is a serious controversy is misguided. The reviewer writes: “One the one hand, clearly there are views against non-explanatory abduction (such as Hoffmann’s, as the author rightly identifies), but on the other hand, there are also some views which simply do not include non-explanatory abduction without being against, such as those “logical”, identified with (some) parts of the AKM approach. Still others, do include non-explanatory abduction (e.g. Magnani) in their treatment, but are not solely committed to it”. I am indebted to the reviewer for this perceptive comment. But, if there are such a subtle nuanced difference among the philosophers about the possibility of non-explanatory abduction, isn’t it rather natural to detect some potentially controversial issue?

conviction constitutes the main reason for proposing the *GW*-schema, which offers a representation of abductive cases not captured by that of the *AKM*-schema (Magnani 2009, p. 71).

Insofar as *AKM* and *GW* models of abduction are the most promising competitors currently, it is not a small matter that Magnani, who is one of the champions of *AKM*-model about abduction, claims that “abduction is *not intrinsically explanationist*”. So, one might be curious as to how could so easily Magnani agree with Gabbay and Woods. There is no mystery involved, however. For Magnani makes clear the situation as follows:

In my previous book on abduction (Magnani 2001) I made some examples of abductive reasoning that basically are non-explanatory and/or instrumentalist without clearly acknowledging it. The contribution of Gabbay and Woods to the analysis of abduction has the logical and epistemological merit of having clarified these basic aspects of abduction, until now disregarded in the literature. Their distinction between explanatory, non-explanatory and instrumental abduction is orthogonal to mine in terms of the theoretical and manipulative (including the subclasses of sentential and model-based) and further allows us to explore fundamental features of abductive cognition (Magnani 2009, p. 71).

So, there seem different points of view, if not controversies, over whether there are non-explanatory abductions.

But what exactly is at stake here? Hoffmann’s way of motivating it is quite useful. He reminds us in the definition quoted above of the fact that Peirce was assuming that there are but three elementary kinds of reasoning, i.e., abduction, deduction, and induction [CP 8.209 (c. 1905)]. But this could be troublesome for Hoffmann, for “the concept of abduction becomes very broad” (Hoffmann 2011, 571). What exactly does Hoffmann have in mind here? What is so wrong if the concept of abduction becomes very broad? Hoffmann refers to Peirce’s passages related to perception as abduction, and animal abductive instinct.⁴ In particular, taking a cue from Peirce’s discussion of a newly hatched chicken’s instinct to pick up kernels of corn as being “just like abductive inference” (Peirce 1901, LOS II 900), Hoffmann perceptively notes that “[b]eing “like” abduction, however, is not the same as being abduction” (Hoffmann 2011, 571).

Even if we can see more clearly how much Hoffmann dislikes broadening the concept of abduction, we still fail to understand the reason why. Of course, Hoffman is not alone in the protest against non-explanatory abduction. In his review of Magnani (2009), Thagard exclaims that “Chapter 2 talks about ‘non-explanatory abduction’ which strikes me as contradictory” (Thagard 2010). Also, in his recent book, he repeats the same complaint: “Magnani (2009) writes of ‘non-explanatory abduction,’ which strikes me as self-contradictory” (Thagard 2012, p. 50). Since apparently “contradictory” or “self-contradictory” must be an accusation, we may expect Thagard to spell out what is so wrong with “non-explanatory abduction”.

⁴For a discussion of the problem of perception as abduction, see Park (2014), “How to Learn Abduction from Animals?: From Avicenna to Magnani”. See also the discussion of the problem of animal abduction in Park (2012).

In both places, however, Thagard concedes fully that there may be a need to let “abduction” cover some other kinds of inference that have similar structure. He even suggests a new term “gabduction” for such generalized abduction so, it turns out that it is merely Thagard’s preference “to see the term ‘abduction’ confined to it explanatory applications” (Thagard 2010). The term “non-explanatory abduction” could be self-contradictory, if we presuppose a definition of abduction like the one quoted above. As a result, there seems to be no progress in uncovering what is wrong with “non-explanatory abduction”.

Probably for that reason, Hoffman presents in more detail what is wrong with “non-explanatory abduction”:

Since terminological questions are questions of convenience, not of truth, I would argue that we get into serious practical troubles if we do not limit abduction to “forming an explanatory hypothesis”, as Peirce suggested. If we broaden the meaning of abduction so that it includes everything someone might come up with, then we could end up counting any daydream as an abductive inference (Hoffmann 2011, 572).

Still the problem is that it is not clear what Hoffmann has in mind by the alleged serious practical troubles, even if they are truly troubling at all. Such a suspicion becomes more serious, when he saves and reintroduces what he initially cited as problematic as satisfying his own definition of abductive insight:

Magnani’s example of formulating axioms that are instrumental for proving certain theorems in mathematics, on the other hand, would be a case of explanatory abduction according to my definition. All three conditions seemed to be fulfilled. The same would be true for the case of perception and the behavior of the chicken mentioned above (Hoffmann 2011, 573).

3 Classification of Science and Classification of Abduction in Peirce

In the last section, we reviewed briefly the two most salient issues in recent attempts to classify abduction: i.e., (1) Is classifying abduction compatible with the search for the logical form of abduction, and (2) could there be any non-explanatory abduction? What is surprising to me is the fact that those who deny the possibility of the logical form of abduction also tend to deny the existence of the non-explanatory abduction. They seem to claim that even if we confine our interest to explanatory abduction, there is no such thing as the logical form of abduction. Also, they seem to assume that their opponents are unmotivated in trying to expand Peirce’s original notion of abduction, which was meant to be explanatory. On the other hand, those who concede the variety of non-explanatory abduction do not have to give up the search for the logical form of abduction. I think there is no inconsistency in their positions, *eo ipso* nothing surprising. Be that as it may, I propose to examine how Peirce would have responded to this controversy. In particular, I suggest to do this in connection with the problem of the classification of sciences in Peirce.

3.1 *Chicken or Egg?*

Probably, it is Beverley Kent's *Charles S. Peirce: Logic and the Classification of the Sciences* (Kent 1987) that chronicles most meticulously the development of Peirce's changing views on the classification of science. Let us find in Kent some clues that might throw a light on possible relationships between the classification of science and the classification of abduction.

In his early inquiry, Peirce examined many different classifications of science of other scholars including Comte, Spencer, Fiske, Wundt, Brinton, and Agassiz. According to Kent, Peirce "adopted Comte's principle but nonetheless rejected his particular ordering" (Kent 1987, p. 90). This principle is nothing but the idea "to have the sciences arranged according to their abstractness—according to the level of generalization of their objects of investigation" (Kent 1987, p. 93). Kent also states this principle as:

The view that sciences form a hierarchy in which the higher sciences supply principles to the lower, while the lower ones furnish data to the higher (Kent 1987, p. 18).

Peirce's qualms with the particular ordering of sciences in Comte seem quite understandable. For Comte not only "placed astronomy ahead of physics" but also omitted philosophy and geometry in his plan (Kent 1987, p. 90, 357.26-8, 1866).⁵

It must be highly suggestive that in his early Lowell lecture (1866), Peirce criticized "a classification scheme based solely on induction because it resulted in a natural classification and so excluded certain sciences" (Kent 1987, p. 91). The idea of classifying science in terms of argument or reasoning itself is already quite significant. The realization that induction is not good enough for such a classification is also remarkable. Probably, however, what is most important for our present purpose is the fact that Peirce believed that "the defect" was remedied by adopting a classification based on a logical procedure that incorporated hypothesis and deduction as well as induction" (Kent 1987, p. 91).

In the resulting hierarchy of Deductive Sciences, Inductive or Classificatory Sciences, and Hypothetical or Causal Science, Peirce equated Inductive sciences as classificatory sciences, which include natural history or zoology, Botany and Mineralogy, Morphology, Descriptive Astronomy, Chemistry, Logic, Philosophy, Physiognomy, and (a portion of) Physical Geography (Kent 1987, p. 91; Peirce 1866, 357.26-8). It is interesting to note what kind of science Peirce counted as inductive or classificatory. A more interesting and important point, however, is that Peirce equated inductive sciences with classificatory sciences. It is a still more interesting and indispensable point to emphasize that the classification of science based entirely on induction would exclude deductive sciences and hypothetical (or causal) sciences.

⁵Another interesting fact is that there were several philosophers, like Spencer, Fiske, Wundt, who placed philosophy below the special sciences (Kent 1987, p. 91).

Kent claims that Peirce himself finds his 1866 classification as “inchoate and needed a new division: the underlying assumption is that the classification of the sciences should be based on a classification of arguments” (Kent 1987, p. 92). Further, she draws our attention to the fact that the same assumption is found in Peirce’s 1878 classification of sciences (ibid.). Kent claims, “he [Peirce] attempted to classify the sciences under the rubric of induction, deduction, and hypothesis on the assumption that a classification of the sciences is based on a classification of arguments” (Kent 1987, p. 51).

Now we can point out two significant facts in Kent’s observations on Peirce’s early classification of sciences and its relationship with his classification of arguments into deduction, induction, and abduction. Firstly, in early Peirce, the former classification depends on the latter one. Secondly, they are related in that fashion because of Peirce’s assumption that a classification of the sciences is based on a classification of arguments. The first point makes sure that we are on the right track. Insofar as these two matters of classification are so closely related, we can be hopeful of deepening our understanding of Peirce’s classification of abduction by studying his various classifications of sciences. The second point is important, because it reminds us of the fact that the early Peirce merely assumes without any argument that a classification of the sciences should be based on a classification of arguments. For, according to Atkins, Peirce’s 1903 classification of sciences seems to be based on another assumption that a classification of the sciences is based on categories (Atkins 2006). Insofar as both Kent and Atkins are right about Peirce’s assumption about the basis of the classification of sciences, we need to check whether Peirce gave up his early assumption and adopted another, or there is no incompatibility between the two assumptions (see Sect. 3.4 for further discussion of this problem).

3.2 *The Problem of Conflating Induction and Abduction*

Above all, we need to reflect on the fact that in early Peirce there seems to be no fine classification of induction or abduction.⁶ For, those critics of non-explanatory abductions might want to exploit this fact as supporting their ground. Even if they might also criticize my playful caricaturing of the critics of non-explanatory

⁶One anonymous reviewer doubts whether this is so: “Peirce’s early classification is based on syllogisms, and I do not see where the “conflation” is found”. According to the reviewer, my quotes by Peirce in Sect. 3.2 are by no means a proof of this alleged “conflation”. I find this criticism extremely interesting. Above all, thanks to the reviewer, I realize that I have been simply assuming that Peirce himself had difficulties in separating induction and abduction. Even if my assumption is ungrounded, that would not damage what I pursue in this subsection based on it. I agree that there is a need to examine my assumption critically. But I am afraid that it could be a digression if I indulge in such an examination here.

abduction, however, I rather doubt whether it is my burden to prove the point. In early Peirce, they might say, we have a scheme of science consisting of (1) Deductive Sciences, (2) Inductive or Classificatory Sciences, and (3) Historical or Causal Sciences, which seems to correspond to deduction, induction, and abduction, respectively. Apparently, there seems to be no need for non-explanatory abduction. Then, why bother?

Exactly here, however, we may find a conclusive proof that Peirce himself concedes the existence of non-explanatory abductions. For Peirce himself was well aware of “the problem of confusing/or conflating abduction and induction” (Anderson 1986, 151). According to Anderson, this problem is “precisely what he [Peirce] wants to overcome” (ibid.). Also, he claims that Peirce “was struggling to find what really made them [induction and abduction] distinct (ibid., 149). Perhaps the best text that shows exactly how Peirce struggled with this problem is the Sect. 5 of the article “Deduction, Induction, and Hypothesis” (1878) (CP 2.619–2.644; W 3, pp. 323–338).⁷ Here, Peirce enumerated at least four merits of distinguishing between induction and abduction:

Induction is, plainly, a much stronger kind of inference than hypothesis;

...

But the essence of an induction is that it infers from one set of facts another set of similar facts, whereas hypothesis infers from facts of one kind to facts of another (CP 2.642; W. 3, p. 336).

...

[H]ypothesis produces the *sensuous* element of thought, and induction the *habitual* element (CP 2.643; W 3, p. 337).

...

Another merit ... is, that it leads to a very natural classification of the sciences and of the minds which prosecute them (CP 2. 644; W 3, p. 338).

This problem of conflating abduction and induction seems to have far-reaching implications for Peirce’s classifications of sciences. To say the least, it seems possible to have abduction within the realm of inductive sciences, or to have induction within the realm of historical or causal sciences in Peirce’s 1866 classification of sciences. Another consequence of this problem could be that either in classifying induction or in classifying abduction, the most urgent problem would be to give a place for conflating cases.

If I am right in claiming that the problem of conflating abduction and induction does matter to the early Peirce in his classification of sciences based on a classification of argument, we should be able to detect in his subsequent attempts to classify sciences some notable changes attributable to this problem. Let us point out some such changes in some of Peirce’s subsequent classifications of sciences. First,

⁷Originally published in *Popular Science Monthly*, vol. 13, pp. 470–482 in 1878.

in the context of demonstrating the merits of distinguishing between induction and abduction, Peirce wrote:

Of the natural sciences, we have, first, the classificatory sciences, which are purely inductive—systematic botany and zoology, mineralogy, and chemistry. Then, we have the sciences of theory, as above explained—astronomy, pure physics, etc. Then, we have sciences of hypothesis—geology, biology, etc. (W 3, p. 338).

Compared to his 1866 classification, the most salient change is that the list of classificatory sciences is drastically reduced. Not only logic and philosophy but also at least some of the natural sciences previously classified as classificatory science disappeared from the list. Almost as salient change seems to be that Peirce divided the category of hypothetical or causal science into two parts: (1) sciences of theory, and (2) sciences of hypothesis. One important point deserves careful attention. In the quote above, Peirce emphasized the fact that what he meant by classificatory sciences are “purely inductive”. I think from this hint we may safely conclude that he eliminated all those natural sciences that are not purely inductive from the list of classificatory sciences. For example, descriptive astronomy is not among the classificatory sciences any more. In the 1878 scheme, we instead have astronomy as one of the sciences of theory. Physical geography, which had been one of the classificatory sciences, disappeared in the 1878 classification, only to reappear in the 1892 classification as one of the descriptive physics, and in 1895 classification as one of the descriptive sciences (Kent 1987, pp. 94, 98). Considering this, it is highly likely that descriptive astronomy and physical geography are partially inductive and partially abductive sciences.

In the 1866 classification, in fact, Peirce had two subdivisions in hypothetical or causal sciences: (1) order 1 (unity of science is unity of hypothesis), and (2) order 2 (unity is the unity of their object). As a consequence, we have reason to think that the division of sciences of theory and sciences of hypothesis in the 1878 classification is nothing other than that of order 1 and order 2 in the 1866 classification. That means, we cannot claim that the problem of conflating induction and abduction caused Peirce to divide the hypothetical or causal sciences into sciences of theory and sciences of hypothesis. It is interesting, however, to note that some of the sciences expelled from classificatory sciences found a new home either in sciences of theory or in sciences of hypothesis. Even if there were already different types of hypothetical or causal sciences in 1866, the problem of conflating induction and abduction at least gave Peirce a fine opportunity to rethink them. The crucial question is whether all this has implications for our problem of classifying abduction.⁸

⁸Peirce’s trilogy of natural sciences, i.e., science of theory, classificatory sciences, and sciences of hypothesis, reappeared in his 1889 classification with different names of nomological, classificatory and descriptive. Since then, it became one of the persistent features in most of Peirce’s varied classifications of sciences. Though Peirce constantly changed the names of its three members, it appeared in 1892, 1895, 1896, 1897, 1898, 1902, and 1903 classifications (Kent 1987, pp. 94, 97–98, 101–102, 104–105, 106–107, 115–116, 134–135).

A possible observation or objection could be that I am mixing up the problem of explanatory vs. non-explanatory abduction with that of conflating abduction and induction. One might be also dissatisfied by my failure to make Peirce's own position clearer as to the questions raised at the beginning of this chapter: for example, "What is Peirce's view on the non-explanatory role of abduction"? I would like to point out that the question invoked as an example can be invoked in response to what I attempt in this chapter. I believe, in order to be consistent, Peirce should have discussed non-explanatory abductions. But, for some reasons, all his explicit discussions of abduction are confined to explanatory abductions. As a consequence, the critics of the non-explanatory abduction do not want to allow the possibility of non-explanatory abductions, by being faithful to Peirce's explicit discussion of abduction. Insofar as no one can answer why Peirce does not explicitly discuss non-explanatory abductions in his writings, however, such an objection would be unfair to me.

3.3 From the Three Kinds of Arguments to the Three Stages in Inquiry

There seems to be enough evidence to believe that the problem of conflating induction and abduction indeed influenced Peirce's classifications of sciences to a certain extent. But exactly how did it influence Peirce's ideas of deduction, induction, and abduction, and thereby modify his classifications of sciences? Above all, we should be able to understand why by 1903 Peirce seems to give up or qualify his early assumption that the classification of sciences must be based on the classification of arguments. For, as was pointed out above, his perennial classification of sciences (in 1903) appears to be based on categories.

The key for solving this puzzle or mystery can be found in K.T. Fann's quite influential study of Peircean abduction (Fann 1970). Fann distinguishes between Peirce's early theory (1859–1890) and his later theory (1891–1914), and further divides both early and later theories into several temporal parts. The titles of Chaps. 1 through 5 of Part I in Fann (1970) well represent the developmental stages in Peirce's early theory: "Peirce's Earliest Conception of Inference (1859–1861)"; "Three Kinds of Inference and Three Figures of Syllogism (1862–1867)"; "Ampliative Inference and Cognition (1868–1870)"; "Induction and Hypothesis (1871–1878)"; "The Method of Methods (1879–1890)". Again the titles of Chaps. 1 and 2 of Part 2 in Fann (1970) suggestively represent the stages in Peirce's later theory: "The transitional Period (1891–1898)"; "Three Stages of Inquiry".

It is interesting to note that Fann does not indicate explicitly the dates for the period in which Peirce viewed the triad of deduction, induction, and abduction as the three stages of inquiry. Fann not only explicitly identifies "the first full statement of Peirce's later theory of abduction" in his 1901 manuscript "On the Logic of Drawing History from Ancient Documents" (Fann 1970, p. 31; CP 7.162–7.255),

but also confirms that “Peirce’s view on the relationship between the three modes of inference remains essentially the same from this date” (Fann 1970, p. 32). Further, according to Fann, “Peirce’s theory of abduction underwent a fundamental change during the decade between 1890 and 1900” (Fann 1970, p. 28). Anderson thinks that Fann is correct in arguing that “Peirce’s view of abduction changes rather drastically in its growth” (Anderson 1986, 147). The most important evidence is, as Anderson points out, Peirce’s own admission that “in almost everything I printed before the beginning of this century I more or less mixed up Hypothesis and Induction...” (CP 8.277; Anderson 1986, 147). Let us see how Peirce defined abduction, deduction, and induction in 1901:

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 suggested by the facts, is what I call *abduction* (CP 7.202).

...

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* (CP 7. 203).

...

This sort of inference it is, from experiments testing predictions based on a hypothesis, that is alone properly entitled to be called *induction* (CP 7.206).

Fann aptly captures the fundamental change in Peirce’s theory of abduction as follows:

The three kinds of inference now become three stages in a scientific inquiry. They are intimately connected as a method (Fann 1970, p. 32).

In the same vein, Anderson claims that Peirce found a vehicle through which the differences between induction and abduction could be made intelligible in viewing “logic as the method of methods” (Anderson 1986, 150).

3.4 *Parallel Changes in Peirce’s Classification of Sciences*

Early in this Sect. 3, we started with Peirce’s assumption that the classification of sciences should be based on the classification of arguments. Then we discussed how Peirce struggled with the problem of conflating induction and abduction. We also noted some evidence for the belief that this problem and Peirce’s changing views about the relation between induction and abduction in fact influenced to a certain extent his classification of sciences. By assuming that Fann and Anderson are right in detecting a fundamental change in Peirce’s views on the relation between

induction and abduction, now we need to check whether such a change made a parallel change in Peirce's classification of sciences.

Interestingly enough, there was indeed a truly drastic change in Peirce's classification of sciences between 1902 and 1903. As we already noted above, Atkins claims that "Peirce's (more or less) final classification of the sciences arises from the systematic application of his Categories of Firstness, Secondness and Thirdness to the classification of the sciences themselves" (Atkins 2006, 483). Unlike Kent, Atkins simply focuses on the comparison of Peirce's 1902 classification and his 1903 classification of sciences. As a consequence, he is so impressed by the following difference between Peirce's method of classifying the sciences of 1903 and that of 1902:

1. It makes no reference to Agassiz but derives its principles of classification from Auguste Comte.
2. Its divisions "happen" to be trichotomic, whereas in 1902 Peirce made no reference to the trichotomic character of divisions and where, in fact, most divisions are dichotomic.
3. Peirce's classification is still "natural", i.e. according to the present state of the sciences—which may account for the fact that not all of the divisions are of a trichotomic character (Atkins 2006, 491).

Based on Kent's report, we already saw above that Comte's heritage on Peirce's classification can be evidenced from an earlier period. Thus, it is not the case, as one might get the false impression from Atkins (2006), that Peirce suddenly adopted Comte's principles in his 1903 classification of sciences. Further, it may not be a truly significant fact that Peirce made no reference to Agassiz in 1903. Peirce's classification of sciences had evolved for more than a thirty year period. That means, even without explicit reference to Agassiz, Peirce's 1903 classification could contain elements influenced by Agassiz.

With all these reservations, however, we are indebted to Atkins a great deal. For, thanks to him, we can now ask whether Peirce changed his early assumption that the classification of sciences should be based on the classification of arguments. In passing, we may note the fact Kent never brings up this issue, even though it is she who identified Peirce's early assumption as to the relationship between the classification of sciences and the classification of arguments. After having made the transition from viewing deduction, induction, and abduction as the three different kinds of arguments to viewing them as the three stages of inquiry, what should Peirce say about his early assumption that the classification of sciences should be based on the classification of arguments? Is it still possible for Peirce to sustain his early assumption even for his 1903 classification of sciences? If so, how to explain the apparently new assumption that the classification of sciences should be based on his Categories?

It is simply beyond the scope of this chapter to discuss extensively the relationship between Peircean Categories, abduction and the classification of sciences. However, it may not be an arduous task to outline how to answer the questions

raised in the last paragraph. For, there are some commentators who already paved a way for such an answer. Indeed, Wim Staat even claims that the interrelationship of abduction, deduction, and induction “can be described with the help of Peirce’s categories” (Staat 1993, 225). Scholars unanimously agree with the Firstness of abduction. However, regarding deduction and induction, opinions are sharply divided. Some claim that induction and deduction should be seen as the Secondness and Thirdness respectively, while others claim that deduction and induction should be seen as the Secondness and Thirdness. Peirce himself confessed uneasiness regarding this issue: “Concerning the relation of these three modes of inference to the categories [...] my opinions, I confess, have wavered” (CP 5.146, 1903; Staat 1993, 227).

Staat’s solution is as follows:

If induction in its completion of inquiry is understood as the most complete representation of the theory of inquiry, from which deduction and abduction are abstracted phases, then the categorical predicate Thirdness best fits induction. For Thirdness encompasses Secondness which in turn encompasses Firstness, just like induction encompasses deduction encompassing abduction. Categorically, therefore, deduction is Second- and abduction First (Staat 1993, 234).

Staat finds support for such an interpretation largely from Max Fisch’s discussion of Peirce’s shift in focus “from the classification of the forms of inference to the functioning of inferences, of the several forms in successive stages of inquiry (Fisch 1986, p. 392; Staat 1993, 233).⁹

4 Abduction Is Not IBE

4.1 *Peirce at the Crossroad*

We have seen above briefly how Peirce’s views on the problem of classifying inferences developed throughout his career. Early Peirce was quite convinced that the classification of sciences should be based on the classification of arguments, so, it is all too natural to find the parallel developments in Peirce’s views on the classification of arguments and the classification of sciences. However, Peirce had to face the problem of conflating induction and abduction even at the early stages in those developments. As a consequence, we can even detect how seriously Peirce struggled with this problem in both his changing views of the classification of arguments and the classification of sciences. Finally, there was a fundamental

⁹Similar line of thought is found in Fann (1970). For he writes, “An interesting point to be noted is the fact that up until the end of the 19th century, Peirce always listed the three modes of inference according to degrees of certainty, namely: deduction, induction, and hypothetic inference. After he came to regard them as the three stages in an inquiry the list became: abduction, deduction and induction” (Fann 1970, p. 31). It may not be a coincidence, since Fann (1970) was written as his M.A. thesis under Max Fisch in 1963, according to Fann’s Preface to the monograph.

change in Peirce's views on the interrelationships between deduction, induction, and abduction. Previously they were thought to be different modes of inferences. Now they became the different stages in inquiry. Such a radical change must also have made a corresponding change in Peirce's ultimate classification of sciences.

What implications do these lessons from Peirce's own experiments have for our current agenda in abduction studies? I think, broadly speaking, these lessons clearly support my predilection for those who are in favor of non-explanatory abductions and for those who still pursue the logical form of abduction. Since, in later Peirce's views, abduction is one of the stages of inquiry rather than one of the three modes of inference, it seems extremely difficult, if not impossible, to correlate certain parts of his 1903 classification of sciences with deduction, induction, or abduction. Rather, given any field of research, we are supposed to face abduction, deduction, and induction, closely interrelated with each other. Probably we do not have to preclude the possibility of characterizing the given field of research as more abductive, more deductive, or more inductive once and for all. However, it would be strange or pointless to allow only explanatory abductions, and thereby to classify them meticulously.

For those who are in favor of the logical form of abduction and non-explanatory abductions, there are two possibilities. Firstly, they may aim at a classification of sciences in terms of an entirely new classification of the different types of interrelationship among abduction, deduction, and induction. Of course, here the fundamental assumption would be that different individual sciences represent different types of interrelationship among abduction, deduction, and induction. Alternatively, they may assume that there is a uniform interrelationship among abduction, deduction, and induction, as Firstness, Secondness, and Thirdness, respectively. Then they may try to classify abduction. The first approach, which seems to be Peirce's own way in his later career, still has the potential for contemporary philosophy of science. The second alternative approach may not be entirely different from that of Schurz, except for the fact that the former, unlike the latter, would also cover non-explanatory abductions.

In fact, it seems to be the case that Peirce himself at least once was at the crossroad to make a serious decision over the alternatives just suggested. For, Peirce seriously discussed the problem of distinguishing between the three different types of abduction in Peirce (1878). Peirce wrote:

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 a hypothesis.

Fossils are found; say, remains like those of fishes, but far in the interior of the country. To explain the phenomenon, we suppose the sea once washed over this land. This is another hypothesis.

Numberless documents and monuments refer to a conqueror called Napoleon Bonaparte. Though we have not seen the man, yet we cannot explain what we have seen, namely, all

these documents and monuments, without supposing that he really existed. Hypothesis again (CP 2.625; W 3, p. 326).¹⁰

It is important to note that it is early Peirce who presented this interesting classification of abduction. To the best of our knowledge, there is no comparable classification of abduction made in later Peirce. Probably, it is “The Logic of Drawing History from ancient Documents” (1901) [CP 7.162–7.255], where we might expect to find the later Peirce’s classification of abduction, if any. For its sixth section is entitled as “Abduction, Induction, and Deduction” (CP 7.202–7.207). If Peirce did not attempt such a classification here, it is even more strange, because in the next section he provided us with an interesting classification of induction into three different kinds: i.e., crude induction, quantitative induction, and qualitative induction (CP 7.208–7.222). According to Goudge, sometimes Peirce speaks of “qualitative induction” as “abductive induction” (Goudge 1950, pp. 158–159).¹¹

After all these years, we are still struggling with exactly those problems Peirce had to face. Further, in this struggle, Peirce is still pointing to a possible way out. For, thanks to his illuminating suggestion of treating “qualitative induction” as “abductive induction”, we may invoke an extremely important issue: Is abduction nothing but “inference to the best explanation (IBE)”?

Evidently, this question pertains to two of the most urgent current agenda in the study of abduction: (1) Is classifying abduction compatible with the search for the logical form of abduction, and (2) could there be any non-explanatory abduction? If abduction is just IBE, the problem of the logical form of abduction is nothing other than that of the logical form of IBE. If abduction is IBE, there would be no more mind-boggling for non-explanatory abduction: How could there be any “Non-explanatory IBE”?

¹⁰Originally published as “Deduction, Induction, and Hypothesis”, *Popular Science Monthly* 13, (August 1878), 470–82. Curiously, it is hard to find substantial discussion of this text in the huge literature on Peirce. Goudge (1950) contrasts the three different kinds of abduction as referring to (1) “unobserved facts”, (2) “facts not only unobserved but physically incapable of being observed by the investigator”, and (3) “entities which in the present state of knowledge are both factually and theoretically incapable of being observed” (Goudge 1950, p. 196).

¹¹For example, Peirce wrote: “But suppose we wish to test the hypothesis that a man is a Catholic Priest, that is, has all the characters that are common to Catholic priests and peculiar to them. Now characters are not units, nor do they consist of units, nor can they be counted, in such a sense that one count is right and every other wrong. Characters have to be estimated according to their significance. The consequence is that there will be a certain element of guess-work in such an induction; so that I call it an *abductive induction*” (Peirce’s emphasis) [CP 6.526]. Can we say that the later Peirce was more concerned with studying how the three stages of inquiry, i.e., abduction, deduction, and induction, were intermingled or intertwined than classifying each stages? Can we say further, even when classifying induction or deduction, it was the role of the abductive element that secured the basis of such classifications? The answers to these questions should be positive at least to the case of induction, as we discussed above. Perhaps, we might expect the same situation in the case of deduction. Can’t we say, for example, that Peirce’s distinction between theorematical and corollarial deduction is also based on some elements of abduction? The recent discussions of the role of abduction in mathematical reasoning may point to the same conclusion.

4.2 Abduction and IBE Conflated

There is a huge amount of literature on IBE. Starting with Gilbert Harman's influential article "Inference to the Best Explanation" (Harman 1965), it culminates with Peter Lipton's book devoted to IBE (Lipton 1991, 2004). Harman didn't bother with the possible differences between abduction and IBE, for he states as if there are merely terminological differences between them:

"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, 88–89).¹²

Lipton is not different from Harman in this regard. For he writes:

According to Inference to the Best Explanation, our inferential practices are governed by explanatory considerations. Given our data and our background beliefs, we infer what would, if true, provide the best of the competing explanations we can generate of those data (so long as the best is good enough for us to make any inference at all).

Inference to the Best Explanation has become extremely popular in philosophical circles, discussed by many and endorsed without discussion by many more (For discussions, see, e.g., Peirce [sic], 1931, 5.180–5.212, esp. 5.189; Harman 1965; Brody 1970; Hanson 1972, Chap. 4; Thagard 1978; Cartwright 1983, essay 5). Yet it still remains much more of a slogan than an articulated account of induction" (Lipton 1991, p. 58).

There is no substantial change in Lipton (2004), but he corrected the misspelled name of Peirce and added a few more references: "Ben-Menahem 1990; Vogel 1990; Day and Kincaid 1994; Barnes 1995; Rappaport 1996; Bird 1998; Psillos 2002)" (Lipton (2004), p. 57).

The textbooks or reference works of philosophy of science, with very good reasons, simply follow suit of such eminent debates among leading philosophers in equating abduction with IBE. For example, James Ladyman writes:

Inference to the best explanation, which is sometimes called *abduction*, is the mode of reasoning that we employ when we infer something on the grounds that it is the best explanation of the facts we already know (Ladyman 2002, p. 47).

Influential philosophers tend to contribute articles in the purview of their expertise for reference works, thereby popularizing their own views as authentic. And Lipton is not the exception to this quasi-rule. For, he simply repeats the same

¹²Cf. Atocha Aliseda's interesting comments: "On the other hand, some authors take induction as an instance of abduction. Abduction as *inference to the best explanation* is considered by Harman [Har65] as the basic form of non-deductive inference, which includes (enumerative) induction as a special case.

This confusion returns in artificial intelligence. 'Induction' is used for the process of learning from examples—but also for creating a theory to explain the observed facts [Sha91]. Thus making abduction an instance of induction. Abduction is usually restricted to producing abductive explanations in the form of facts. When the explanations are rules, it is regarded as part of induction" (Aliseda 2006, p. 34).

views in both *A Companion to the Philosophy of Science* and *The Routledge Companion to Philosophy of Science*.¹³

Commenting on Lipton's treatment of abduction in the passage quoted above (Lipton 1991, 2004), Campos notes:

For instance, Peter Lipton, like Gilbert Harman before him (1965, pp. 88–89), simply cites, without argument, Peircean abduction as one of the antecedent, and apparently insufficiently developed, accounts of inductive IBE (Lipton 1991, p. 58; Campos 2011, 419–420).

He also counts Lipton's appropriation as "inaccurate" on the ground that "it is not based on any systematic comparison of these concepts" (ibid.).

4.3 *The Difference Between Abduction and IBE*

Of course, there have been some attempts to revolt against such a trend. Gerhard Minnameier, for example, clearly contrasts abduction with IBE in terms of their functions. While abduction is for the generation of theories, IBE is for their evaluation:

Peirce characterizes abduction as the only type of inference that is *creative* in the sense that it leads to new knowledge, especially to (possible) theoretical explanations of surprising facts. As opposed to this, IBE is about the acceptance (or rejection) of already established explanatory suggestions. Thus, while abduction marks the process of generating theories – or, more generally, concepts – IBE concerns their evaluation. However, if this is so, then both inferential types relate to entirely different steps in the process of knowledge acquisition (and, as I also try to show, of Knowledge application) (Minnameier 2004, 75–76).

According to Minnameier's rather persuasive explanation, the later Peirce realized that previously he "more or less mixed up Hypothesis and Induction" (CP 8.221, 1910) (Minnameier 2004, 78). There are at least two interesting points that forced Peirce to change his mind. On the one hand, he came to realize that what he had called previously "hypothesis" is rather a variant of induction, so, he even renamed "hypothesis" as "qualitative induction" (cf. NEM III/2, 874, 1909;

¹³“The model of inference to the Best Explanation is designed to give a partial account of many inductive inferences, both in science and in ordinary life. One version of the model was developed under the name “abduction by Charles Sanders Peirce early in the twentieth century, and the model has been considerably developed and discussed over the last 25 years (see Peirce 1931–1958). Its governing idea is that explanatory considerations are a guide to inference, that scientists infer from the available evidence to the hypothesis which would, if correct, best explain that evidence” (Lipton 2000, p. 184); “The model of inference to the best explanation (IBE) is designed to give a partial account of many inductive inferences, both in science and in ordinary life. One version of the model was developed under the name “abduction” by Charles Sanders Peirce early in the twentieth century, and the model has been considerably developed and discussed over the last four decades (e.g., Harman 1965; Thagard 1978; Day and Kincaid 1994; Barnes 1995; Psillos 2002; Lipton 2004). Its governing idea is that explanatory considerations are a guide to inference, that scientists infer from the available evidence to the hypothesis which would, if correct, best explain that evidence” (Lipton 2008, 193).

Minnameier 2004, 78). On the other hand, as Minnameier pins down, “what used to be called ‘induction’ in the sense of leading from facts to a theory about those facts would now have to be regarded as an ‘abductive’ inference” [ibid.]. He seems to find the ground for such an interpretation from the late Peirce’s realization that induction “never can originate any idea whatever. Nor can deduction. All the ideas of science come to it by the way of Abduction” (CP 5.145, 1903; Minnameier 2004, 78).

Many philosophers seem to agree with Minnameier in distinguishing abduction from IBE by their primary functions: i.e., abduction as generating theories from IBE as evaluating them. Magnani presents this distinction as two different senses of abduction: i.e., abduction as hypothesis generation and abduction as hypothesis generation and evaluation:

As stated above, there are two main epistemologico/cognitive meanings of the word abduction: (1) abduction that only generates plausible hypotheses (*selective* or *creative*)—this is the meaning of abduction accepted in my epistemological model—and (2) abduction considered as *inference to the best explanation*, that also evaluates hypotheses by induction. In the latter sense the classical meaning of selective abduction as inference to the best explanation (for instance in medicine, to the best diagnosis) is described by the complete abduction–deduction–induction cycle. This distinction needs further clarification (Magnani 2009, p. 18).¹⁴

Campos also appeals to the same idea, for he claims that

Peircean abduction is an in-depth account of the process of generating explanatory hypotheses, while IBE, at least in Lipton’s thorough treatment, is a more encompassing account of the processes of both of generating and of evaluating scientific hypotheses (Campos 2011, 420).

Recently, Mackonis claims that both those who “do not conceptually discriminate between IBE and abduction or use the term ‘abduction’ as standing for IBE” and those who “argue that IBE and abduction are conceptually distinct” are wrong. The former is wrong, because “there is more to IBE than mere abduction”. The latter is wrong, because “this stance is also an exaggeration: two concepts are indeed related” (Mackonis 2013, 2). Whether Mackonis is right or wrong in such an assessment, what is important for the present purpose is that he also finds the difference between abduction and IBE exactly at the same place as Minnameier, Magnani, and Campos do:

¹⁴Regarding these two senses of abduction, Magnani gives some further comments, which also deserve serious examination: “It is clear that the two meanings are related to the distinction between hypothesis generation and hypothesis evaluation, so abduction is the process of generating explanatory hypotheses, and induction matches the hypothetico-deductive method of hypothesis testing (1st meaning). However, we have to remember (as we have already stressed) that sometimes in the literature (and also in Peirce’s texts) the word abduction is also referred to the whole cycle, that is as an inference to the best explanation (2nd meaning)” (Magnani 2009, p. 18).

The general aim of abduction is to generate a possible explanatory hypothesis. ... The general aim of IBE is both to generate an explanatory hypothesis and to assess it (Mackonis 2013, 3).

If so, it is safe to conclude that, at least in one important sense, abduction and IBE are clearly distinguished: abduction is for generation of hypotheses or theories, while IBE is for evaluating them.

This conclusion might have far-reaching implications. For example, it could cause a serious trouble for Schurz and his project of classifying patterns of abduction. Since he identified without argument abduction with IBE (*supra* Sect. 2.1), it could be the case that what he classifies is not the patterns of abduction but “the patterns of IBE”. Furthermore, if Minnameier is right in his claim “that IBE is to be broadly identified with what Peirce calls (qualitative) induction” (Minnameier 2004, 76), that means, Schurz could be classifying the patterns of (qualitative) induction rather than abduction. Of course, as Magnani points out, there is indeed a sense that IBE can be called “abduction”, i.e., abduction as hypothesis generation and evaluation. Still, this consequence could be detrimental, if not damaging, to Schurz’s intention.

4.4 IBE Cannot Be Abduction in GW-Model

The only consolation to Schurz is the fact that Peirce himself was struggling with the problem of distinguishing abduction from induction, all too frequently conflating them, as we saw above.¹⁵ Further, Minnameier’s point that IBE is broadly identifiable with Peircean (qualitative) induction may hold just for Peircean induction not for induction in the twenty first century philosophy of science.¹⁶ But would the situation become a bit more favorable to Schurz?

¹⁵In addition to our discussion in Sect. 3.2, please note what Magnani points out about this matter. “As Thagard has pointed out (1988, p. 53) the question was controversial in Peirce’s writings too. Before the 1890s, Peirce discussed the hypothesis as follows: “Hypothesis is where we find some very curious circumstance which would be explained by the supposition that it was the case of a certain general rule, and thereupon adopt that supposition” (Peirce 1931–1958, 2.624). When Peirce replaced hypothesis with abduction he said that it “furnishes the reasoner with the problematic theory which induction verifies” (Peirce 1931–1958, 2.776). Thagard ascribes to the editors of Peirce’s work the responsibility for having clouded this change in his thinking by including discussions of hypothesis under the heading of “Abduction”, “[...] obscuring his shift from the belief that inference to an explanatory hypothesis can be a kind of justification to the weaker view that it is only a form of discovery” (Magnani 2009, p. 18).

¹⁶In this connection, please refer to Minnameier’s disclaimer regarding his project (Minnameier 2004, 77).

In his review of Magnani (2009), Woods indeed draws our attention to these issues. He writes:

An important question is the closeness of the oft-asserted tie between Magnani's abduction and inference to the best explanation (hence most versions of the AKM-schema.) Magnani is careful to emphasize that abduction is not intrinsically explanationist (10, 71, 292; cf. 6, 9, 18, 68). Hintikka (2007) makes the useful point that whether abduction is inference to the best explanation depends on the sense of explanation on offer. If, for example,

... explaining an explanandum E is to derive it from[a] ... background theory T plus a number of contingent truths A that are relative to E and that have to be found in order for an attempt to explain E to succeed (p. 41),

then it is easy to see that abduction is not at all explanationist. Let Hintikka's background theory T play the role of the AKM-schema's knowledge-base K and let Hintikka's derivation relation be the AKM \rightarrow . Similar pairings hold for the GW-model, except that Hintikka's derivation relation is now the GW-attainment relation. In Hintikka's explanation schema, the A_i are contingent truths. But in the abduction schemata they are hypotheses; and they would be disqualified as hypotheses were their abducer to advance them as truths. We have it then that, for this rather widely held notion of explanation, abduction cannot be inference to an explanation (Woods 2011, 242).

As one can imagine, Woods is here touching upon a very subtle and sensitive issue. For, insofar as Magnani subscribes himself to the AKM-Model, one might be presume that he would be somewhat sympathetic with IBE. Even though he makes it crystal clear that he takes the sense of abduction as hypothesis generation, he still admits that there is another sense of abduction, i.e., abduction as hypothesis generation and evaluation. In other words, at least in Magnani (2009), he seems to be rather tolerant to the tendency to call IBE "abduction". As we quoted above (the text for footnote 10), he reminds us of the fact that if we emphasize the whole abduction-deduction-induction cycle, IBE is closely related to abduction. His reminder of "selective abduction as inference to the best explanation (for instance in medicine, to the best diagnosis)" seems quite pertinent. Further, he never forgets to mention the fact even in Peircean corpus we find the occurrences of the term "abduction" referring to the whole cycle.

However, in Magnani (2009), Magnani also discusses explicitly the impossibility of fully identifying abduction with IBE. For example, he writes:

Through abduction the basic ignorance – that does not have to be considered total "ignorance" – is neither solved nor left intact: it is an ignorance-preserving accommodation of the problem at hand. As I have already stressed, in a defeasible way, further action can be triggered either to find further abductions or to "solve" the ignorance problem, possibly leading to what it is usually called the inference to the best explanation. It is clear that in this framework the inference to the best explanation – if considered as a truth conferring achievement – cannot be a case of abduction, because abductive inference is constitutively ignorance preserving. In this perspective the inference to the best explanation also involves – for example –the generalizing and evaluating role of *induction*. Of course it can be said that the requests of originary thinking are related to the depth of the abducer's ignorance (Magnani 2009, p. 68).

As is evident, Magnani here responding to the ignorance problem Gabbay and Woods (2005) highlighted so much with the GW-Model.

Then, we seem to have a tension in Magnani's stance toward IBE. Woods notes, as was quoted above, "Magnani is careful to emphasize that abduction is not intrinsically explanationist (10, 71, 292; cf. 6, 9, 18, 68) (Magnani 2009)" (Woods 2011, 242). Further, he seems to remind Magnani of the fact that the impossibility of identifying abduction with IBE can be shown by AKM-Model as well as by GW-Model. Be that as it may, there is no doubt that here Woods invites Magnani to clarify his position toward IBE.

Recently, Magnani made some important observations on the GW-model of abduction in this regard:

It is clear that in the framework of the GW-schema the inference to the best explanation— if considered as a truth conferring achievement justified by the empirical approval — cannot be a case of abduction, because abductive inference is constitutively ignorance-preserving. In this perspective the inference to the best explanation involves the generalizing and evaluating role of *induction* (Magnani 2013, pp. 5–6).

Though formulated in extreme clarity, as far as basic ideas are concerned, there is no news here. That is understandable, because Magnani (2013) is a serious exercise on the ignorance problem with much broader perspective, in which the identifiability of abduction with IBE is just one of the issues. Probably, Magnani still wants to ponder on the right place for IBE in the study of abduction. In this regard, it seems significant to face the same issue from Magnani's Eco-Cognitive Model (EC-Model) of abduction (Magnani 2013, Sect. 1.2).

Indeed Magnani notes that in abduction problems the abducer "has to produce the best choice" among the many guessed hypotheses, and quotes Woods' comment on the GW-model: "It is extremely difficult to see how this is done, both formally and empirically. Clause (9) (in the GW-model) is a place-holder for two problems, not one. There is the problem of finding criteria for hypothesis *selection*. But there is the prior problem of specifying the conditions for *thinking up* possible candidates for selection. The first is a 'cutdown' problem. The second is a 'fill-up problem'; and with the latter comes the received view that it is not a problem for logic" (Woods 2011, p. 243 [emphasis added]; Magnani 2013, pp. 6–7). According to Magnani, "[h]ere we touch the core of the ambiguity of the ignorance-preserving character of abduction" (ibid., p. 7).

1. $T \nmid \alpha$ [setting of T as anepistemic target with respect to a proposition α]
2. $\sim(R(K, T) \text{ [fact]})$
3. $\sim(R(K^*, T) \text{ [fact]})$
4. $H \notin K$ [fact]
5. $H \notin K^*$ [fact]
6. $\sim R(H, T) \text{ [fact]}$
7. $\sim R(K(H), T) \text{ [fact]}$
8. If $H \rightsquigarrow R(K(H), T) \text{ [fact]}$
9. H meets further conditions S_1, \dots, S_n [fact]
10. Therefore, $C(H)$ [sub-conclusion, 1–9]
11. Therefore, H^c [conclusion, 1–10]

(GW-model; Woods 2011, 241; Gabbay and Woods 2005, p. 342f).

Indeed, Magnani further notes (Magnani 2013, p. 888) “Because the cognitive processes of generation (fill-up) and of selection (cut down) can both be sufficient—even in absence of the standard inductive evaluation phase—to *activate* and accept [...] an abductive hypothesis, and so to reach cognitive results relevant to the context (often endowed with a knowledge-enhancing outcome, as I will illustrate below” [...]. In these special cases the best choice is immediately reached without the help of an experimental trial (which fundamentally characterizes the received view of abduction in terms of the so-called ‘inference to the best explanation’). Not only, we have to strongly note that the generation process alone can suffice, like it is demonstrated by the case of human *perception*”.

In these case the process performs a knowledge enhancing result not necessarily intertwined with plausibilistic concerns, such as consistency and minimality, realizing a kind of immediate inference to the best explanation. Though intriguing, it is obviously beyond the scope of this chapter to discuss EC-model.

5 Concluding Remarks: Abduction Is Everywhere

I identified the two most controversial issues in the project of classifying abduction, i.e., (1) Is classifying abduction compatible with the search for the logical form of abduction, and (2) could there be any non-explanatory abduction? I took a somewhat positive stance to both in this chapter. I wanted to get some further hints from Peirce on these issues, especially from his attempts to classify inferences as well as from his classifications of sciences. Though suggestive and insightful, I found that Peirce himself had a hard time to distinguishing between abduction and induction. As a crucial issue, bearing on both controversial issues in classifying abduction, I finally discussed rather extensively whether inference to the best explanation is abduction. At least tentatively, I arrived at the conclusion that, insofar as abduction is for generating hypotheses or theories, while IBE is for their evaluation as well as generation, IBE is induction rather than abduction. Also, I noted that IBE cannot be abduction according to the GW-model of abduction, due to the ignorance problem. If I am on the right track, then it is hard not to be skeptical about some recent attempts to classify different patterns of abduction, for those attempts simply assume that IBE is abduction. In other words, the alleged classifications of the patterns of abduction might be rather that of the patterns of induction. In that sense, the project of classifying abduction is yet to get off the ground. All the more so, if we remember the fact that abduction is everywhere rather than confined in the narrow realm of natural science.

Probably, it may not be meaningless to quote Campos’s views on the bad consequences of conflating IBE and abduction:

What is lost, overall, in blurring the distinction between abduction and induction by claiming that Peircean abduction is IBE? Beyond the formal distinction between two forms

of inference, we lose clear sight of the different aims of scientific reasoning—conjecturing versus evaluating—at different stages of inquiry. As a result, we also lose focus on providing a deeper account of hypothesis formation alone, apart from questions of the scientific assessment and evaluation of those hypotheses, once they have been proposed. We lose emphasis on thoroughly describing the mark of abduction—the act of bringing relevant, often innovative, concepts to bear in creative ways on the plausible explanation of previously unexplained phenomena (Campos 2011, 441).¹⁷

It is by no means, however, my intent to devalue the achievements of the recent attempts to classify the patterns of abduction. Of course, it is mandatory as well as interesting to compare Peirce's classification with Schurz's classification of abduction in connection with the problem of the classification of sciences. Those critics of non-explanatory abductions may elaborate and perfect their classification of explanatory abduction. Still they may get further hints or insights from the problem of classifying sciences, whether they be explanatory or non-explanatory. Those friends of non-explanatory abductions may adopt some invaluable results in Schurz's classification of explanatory abductions in their own classification of both explanatory and non-explanatory abductions, and their classification of sciences. In other words, it seems that the possible controversy between the friends and enemies of non-explanatory abduction is not as serious as it might appear. Even if the enemies of non-explanatory abductions are right in that Peirce himself was an explanationist exclusively interested in classifying explanatory abduction, that does not necessarily hinder or prohibit our search for the classification of both explanatory and non-explanatory abduction. In that spirit, it is certainly promising and fascinating to pursue the role and functions of abductive reasoning in mathematics (Magnani 2009, pp. 72–76; Gabbay and Woods 2005, pp. 125–128), or to study legal and interpretative abductions in practical areas (Gabbay and Woods 2005, 2010a, b, c). Anderson's discussion of artistic abduction is yet another clear example (Anderson 1987, pp. 62–64).

My denial of the identity of abduction and IBE should already have freed us from the prison of explanationist philosophy of science. Abduction is not peculiar to science, let alone IBE in science. According to Peirce, every inquiry, if not inference, is deduction, induction or abduction. I also noted above how closely these three types of inquiries or inferences are intertwined. Furthermore, Peirce himself took a very broad concept of inference. For him, every sign activity was an inference. We may develop this line of thought a bit further, thereby insinuating the intolerable narrowness of those who have tried to confine abduction to explanatory science. In Sect. 3, I tried to fathom Peirce's mind regarding the problem of the logical form of abduction and the problem of classifying abduction in connection with his other life-long problem of the classification of science. However, it is now clear that I in fact dealt with only the problem of the classification of natural

¹⁷Throughout this chapter, I tried to make clear how much I value Schurz's achievement in his attempt to classify abduction. Even so, there still might be people who doubt whether I have been unfair to Schurz. I would kindly ask them not to forget the fact that Schurz explicitly equated abduction with IBE.

science. I might have dealt with a bit more than that. Still, in view of any one of Peirce's classifications of science in any period, it is evident that I failed to do justice to many scientific disciplines: psychology or economics, for example. Of course, there are also other huge intellectual disciplines such as history or law to which I did not pay due attention. Furthermore, Peirce wanted to place philosophy within his classification of science. But, if deduction, induction, and abduction tend to be intertwined, and if inference is understood as broadly as possible to cover any sign activity, the business of classifying abduction cannot ignore abduction in any of these disciplines.¹⁸

It is with a somewhat surprising observation about the project of classifying abduction that I will conclude. One might assume that in this project we are supposed to divide and subdivide the given territory of abduction. Some recent attempts, such as Schurz (2008), seem to fit this way of understanding the project of classifying abduction, for they implicitly confine their interest to science, if not more narrowly explanatory science. As we can witness, however, there are other attempts that tend to expand and amplify the project of classifying abduction. By introducing the distinction between explanatory/non-explanatory abductions, Gabbay and Woods (2005) made a sure step toward that direction. By introducing his multiple distinctions between different kinds of abduction, i.e., selective/creative,

¹⁸In some sense, such a failure to deal with the problem of classifying abduction in some disciplines other than natural science is partly due to Peirce himself. Let us think about ethics as a clear example. There is no doubt that ethics is a huge field in which abductive reasoning must have a significant role. Nevertheless, it is hard to find a text where Peirce analyzes the structure of abductive moral reasoning. Curiously enough, philosophers and historians of philosophy have discussed Peirce's classification of philosophy much more extensively than his classification of natural science [for example, see Anderson (1995), Kent (1987) and Goudge (1950)]. Thus, if Peirce had discussed the problem of classifying abduction in morality, it would have been studied enthusiastically. We have no intention to plunge into this Peircean scholarship here. According to most scholarly opinions, Peirce seems rather reluctant to discuss problems of morality in spite of his extensive reading of moral philosophy. As Krois reports, "[f]or most of his life Peirce rejected explicitly the possibility and wisdom of developing a 'philosophical' ethics and so he never wrote a work entitled 'Ethics' (Krois 1994, p. 27). Also, it was quite late when Peirce placed ethics as one of the normative sciences within his classification of science. In his 1892 classification, ethics is found together with political economy, poetry, music, and games under the rubric of "practical psychics". According to Kent's commentary, here not only Comte's principle is working, but the broad divisions themselves are "not very different from Comte's own", where ethics is together with theology, politics, law, and etiquette "subsumed under sociology" (Kent 1987, pp. 94–95). It is only in Peirce's classification of 1897, when ethics appears, with logic and metaphysics, as philosophy (Ibid., p. 104). Finally, together with esthetics and logic, ethics is presented as one of the normative sciences in his classification around 1902 (Ibid., p. 115). For these reasons, it is hopeless to learn from Peirce how to classify abduction in morality.

On the other hand, recent studies of abduction have extended their reach far enough to cover morality as well as all other disciplines other than natural science. Magnani's recent work such as *Morality in a Technological World: Knowledge as Duty* (Magnani 2007) or *Understanding Violence* (Magnani 2011) is a clear example. Gabbay and Woods's *The Reach of Abduction: Insight and Trial* (Gabbay and Woods 2005) opens an entirely new field, i.e., the study of non-explanatory abduction. In particular, they discuss abductive reasoning in law, which they continue to do so in their subsequent work (Gabbay and Woods 2010a, b, c; Woods 2010).

theoretical/manipulative, and sentential/model-based, Magnani (2001) and (2009) took one further radical step. For, starting with Peirce's insight of treating any sign activity as inferential, by fully exploiting his multiple distinction of different kinds of abduction, Magnani found instances of abduction not only in science and any other human enterprises but also in animals, bacteria, and brain cells. Probably, there may not be any tension between Schurz and others' project of classifying abduction in sciences and Magnani and others' project of classifying abduction in broader contexts. No matter how we understand the project of classifying abduction, it will deepen our understanding of abduction. It is interesting to note that Peirce himself was involved in both projects of classifying abduction, i.e., classifying abduction in science and classifying abduction in broader contexts. This clearly indicates the necessity of studying how to classify signs in connection with classifying abduction as long as we want to go with and beyond Peirce. But such a study is far beyond the scope of this chapter.

References

- Aliseda, A. (2006). *Abductive reasoning*. Dordrecht: Springer.
- Anderson, D. R. (1986). The evolution of Peirce's concept of abduction. *Transactions of the Charles S. Peirce Society*, 22(2), 145–164.
- Anderson, D. R. (1987). *Creativity and the philosophy of C. S. Peirce*. Dordrecht: Martinus Nijhoff Publishers.
- Anderson, D. R. (1995). *Strands of system: The philosophy of Charles Peirce*. West Lafayette, Indiana: Purdue University Press.
- Atkins, R. K. (2006). Restructuring the sciences: Peirce's categories and his classifications of the sciences. *Transactions of the Charles S. Peirce Society*, 42(4), 483–499.
- Barnes, E. (1995). Inference to the loveliest explanation. *Synthese*, 103, 251–277.
- Ben-Menahem, Y. (1990). The inference to the best explanation. *Erkenntnis*, 33, 319–344.
- Bird, A. (1998). *Philosophy of science*. London: UCL Press.
- Brody, B. A. (1970). *Readings in philosophy of science*. Englewood Cliffs, NJ: Prentice-Hall.
- Campos, D. G. (2011). On the distinction between Peirce's abduction and Lipton's inference to the best explanation. *Synthese*, 180, 419–442.
- Cartwright, N. (1983). *How the laws of physics lie*. Oxford University Press.
- Day, T., & Kincaid, H. (1994). Putting inference to the best explanation in its place. *Synthese*, 98, 271–295.
- Fann, K. T. (1970). *Peirce's theory of abduction*. The Hague: Martinus Nijhoff.
- Fisch, M. H. (1986). K. L. Ketner & C. J. W. Kloesel (Eds.), *Peirce, semeiotic and pragmatism: Essays by Max H. Fisch*. Bloomington: Indiana University Press.
- Friedman, H., & Simpson, H. (2000). Issues and problems in reverse mathematics. *Computability Theory and Its Applications; Contemporary Mathematics*, 257, 127–144.
- Gabbay, D., & Woods, J. (2005). *The reach of abduction: Insight and trial, a practical logic of cognitive systems* (Vol. 2). Amsterdam: North-Holland.
- Gabbay, D., & Woods, J. (2010a). In Gabbay et al. (Eds.), *Logic and the law: Crossing the lines of discipline* (pp. 165–201).
- Gabbay, D., & Woods, J. (2010b). In Gabbay et al. (Eds.), *Relevance in the law* (pp. 239–261).
- Gabbay, D., & Woods, J. (2010c). In Gabbay et al. (Eds.), *The law of evidence and labelled deduction: A position paper* (pp. 295–331).

- Gödel, K. (1944). Russell's Mathematical Logic. In P. Schilpp (Ed.), *The philosophy of Bertrand Russell* (pp. 123–153). The Tudor Publishing Company: New York.
- Gödel, K. (1990). Remarks before the Princeton Bicentennial conference on problems in mathematics. In S. Feferman, et al. (Eds.), *Kurt Gödel collected works* (pp. 150–153). Oxford: Oxford University Press.
- Goudge, T. A. (1950). *The thought of C. S. Peirce*. New York: Dover.
- Hanson, N. R. (1972). Observation and explanation: *A guide to philosophy of science*. London: Allen and Unwin.
- Harman, G. (1965). The inference to the best explanation. *The Philosophical Review*, 74(1), 88–95.
- Hintikka, J. (2007). *Socratic epistemology: Explorations of knowledge-seeking by Questioning*. Cambridge: Cambridge University Press.
- Hoffmann, M. H. G. (2011). 'Theoric transformations' and a new classification of abductive inferences. *Transactions of the Charles S. Peirce Society*, 46(4), 570–590.
- Kapitan, T. (1997). In Houser et al. (Eds.), *Peirce and the structure of abductive inference* (pp. 477–496).
- Kent, B. (1987). *Charles S. Peirce: Logic and the classification of the sciences*. Kingston and Montreal: McGill-Queen's University Press.
- Krois, J. M. (1994). C. S. Peirce and philosophical ethics. In Parret, H. (Ed.), *Peirce and value theory: On Peircean ethics and aesthetics* (pp. 27–37). Amsterdam/Philadelphia: John Benjamins Publishing Company.
- Ladyman, J. (2002). *Understanding philosophy of science*. London/New York: Routledge.
- Lipton, P. (1991). *Inference to the best explanation*. London/New York: Routledge.
- Lipton, P. (2000). In Newton-Smith (Ed.), *Inference to the best explanation* (pp. 184–193).
- Lipton, P. (2004). *Inference to the best explanation* (2nd ed.). London/New York: Routledge.
- Lipton, P. (2008). In Psillos & Curd (Eds.), *Inference to the best explanation* (pp. 193–202).
- Magnani, L. (2001). *Abduction, reason, and science: Processes of discovery and explanation*. New York: Kluwer.
- Magnani, L. (2007). *Morality in a technological world: Knowledge as duty*. Cambridge: Cambridge University Press.
- Magnani, L. (2009). *Abductive cognition: The epistemological and eco-cognitive dimensions of hypothetical reasoning*. Berlin: Springer.
- Magnani, L. (2011). *Understanding violence: The intertwining of morality, religion and violence: A philosophical stance*. Berlin: Springer.
- Mackonis, A. (2013). Inference to the best explanation, coherence and other explanatory virtues. *Synthese*, 190(6), 975–995.
- Magnani, L. (2013). Is abduction ignorance-preserving? conventions, models, and fictions in science. *Logic Journal the IGPL*, 21(6), 882–914 (special issue on formal representations in model-based reasoning and abduction).
- Magnani, L. (2015). Naturalizing logic. Errors of reasoning vindicated: Logic reapproaches cognitive science. *Journal of Applied Logic*, 13, 13–36.
- Minnemeier, G. (2004). Peirce-suit of truth—Why inference to the best explanation and abduction ought not to be confused. *Erkenntnis*, 60, 75–105.
- Park, W. (2012). Abduction and estimation in animals. *Foundations of Science*, 17, 321–337.
- Park, W. (2014). How to learn abduction from animals?: From Avicenna to Magnani. In L. Magnani (Ed.), *Model-based reasoning in science and technology: Theoretical and cognitive issues* (pp. 207–220). Heidelberg/Berlin: Springer.
- Peirce, C. S. (1866). "Classification of the Sciences [1866]", 357.26-8. The Charles S. Peirce Papers: Manuscript Collection in the Houghton Library. The University of Massachusetts Press, Worcester, MA (1966), Annotated Catalogue of the Papers of Charles S. Peirce. Numbered according to Richard S. Robin. Available in the Peirce Microfilm edition. Pagination: CSP = Peirce / ISP = Institute for Studies in Pragmatism.

- Peirce (1878). Deduction, Induction, and Hypothesis (CP 2.619–2.644). In Peirce, C. S. (1931–1958). In C. Hartshorne & P. Weiss (Vols. I–VI), and A. W. Burks (Vols. VII–VIII) (Ed.), *Collected papers* (8 Vols). Cambridge, MA: Harvard University Press
- Peirce (1901). “The Logic of Drawing History from ancient Documents”, [CP 7.162–7.255]. In Peirce, C. S. (1931–1958). In C. Hartshorne & P. Weiss (Vols. I–VI), and A. W. Burks (Vols. VII–VIII) (Ed.), *Collected papers* (8 Vols). Cambridge, MA: Harvard University Press
- Peirce, C. S. (1903). Instinct and Abduction, CP 5.171. In Peirce, C. S. (1931–1958). In C. Hartshorne & P. Weiss (Vols. I–VI), and A. W. Burks (Vols. VII–VIII) (Ed.), *Collected papers* (8 Vols). Cambridge, MA: Harvard University Press
- Peirce, C. S. (1931–1958). In C. Hartshorne & P. Weiss (Vols. I–VI), and A. W. Burks (Vols. VII–VIII) (Ed.), *Collected papers* (8 Vols). Cambridge, MA: Harvard University Press (Abbreviated as CP).
- Psillos, S. (2002). Simply the best: A case for abduction. In A. C. Kakas & F. Sadri (Eds.), *Computational logic: Logic programming and beyond 2002* (pp. 605–626). Berlin: Springer.
- Rappaport, S. (1996). Inference to the best explanation: Is it really different from Mill’s methods? *Philosophy of Science*, 63, 65–80.
- Russell, B. (1973) The regressive method of discovering the premises of mathematics [1907], in: D. Lackey (Ed.), *Essays in analysis* (pp.45–66). London: George Allen and Unwin.
- Schurz, G. (2008). Patterns of Abduction. *Synthese*, 164, 201–234.
- Simpson, S. G. (1999). *Subsystems of second order arithmetic*. Berlin: Springer.
- Staat, W. (1993). On abduction, deduction, induction and the categories. *Transactions of the Charles S. Peirce Society*, 29(2), 225–237.
- Thagard, P. (1978). The best explanation: Criteria for theory choice. *Journal of Philosophy*, 75, 76–92.
- Thagard, P. (1988). *Computational philosophy of science*. Cambridge, MA: MIT Press.
- Thagard, P. (2010). (Review of) L. Magnani, abductive cognition: The epistemological and eco-cognitive dimensions of hypothetical reasoning. *Mind & Society*, 9(1), 111–112.
- Thagard, P. (2012). *The cognitive science of science: Explanation, discovery, and conceptual change*. Cambridge, Mass.: The MIT Press.
- Vogel, J. (1990). Cartesian skepticism and inference to the best explanation. *The Journal of Philosophy*, 87, 658–666.
- Wheeler, M. (2004). Is language an ultimate artifact? *Language Sciences*, 26, 693–715.
- Williamson, J. (2003). Review of Lorenzo Magnani, abduction, reason, and science: Processes of discovery and explanation. *British Journal for the Philosophy of Science*, 54, 353–358.
- Woods, J. (2010). In Gabbay et al. (Eds.), *Abduction and proof: A criminal paradox* (pp. 217–238).
- Woods, J. (2011). Recent developments in abductive logic. *Studies in History and Philosophy of Science*, 42(1), 240–244 (Essay review of L. Magnani (2009). *Abductive cognition. The epistemologic and eco-cognitive dimensions of hypothetical reasoning*, Heidelberg/Berlin: Springer).

Abduction in Context

The Conjectural Dynamics of Scientific Reasoning
Park, W.

2017, XIV, 263 p. 10 illus., Hardcover

ISBN: 978-3-319-48955-1