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Hansonian and Harmanian Abduction as Models of Discovery

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Abstract

In this article I compare two varieties of abduction as reconstructive models for analyzing discovery. The first one is “Hansonian abduction”, which is based on N. R. Hanson’s formulations of abduction. The other is “Harmanian abduction”, the Inference to the Best Explanation (IBE) model, formulated especially by Gilbert Harman. Peter Lipton has analyzed processes of discovery on the basis of his developed form of Harmanian abduction. I will argue that Hansonian abduction would be, however, a more apt model for this purpose. As an example, I will reconstruct, in a Hansonian manner, Ignaz Semmelweis’ research on childbed fever and compare it to the IBE reconstruction of Lipton. I argue that Hansonian abduction is in accordance with Lipton’s aim of taking into account the distinction between actual and potential explanations on the one hand, and between likely and lovely explanations on the other. I maintain that a developed version of Hansonian abduction combined with loveliness gives an important, new conceptual means for analyzing processes of discovery.

1. Introduction

There are two basic ways of understanding the term ‘abduction’ in philosophy of science nowadays. One starts from Charles S. Peirce’s use of the term as a third mode of inference besides deduction and induction, which is connected to the generation of new ideas. This use has continued in debates concerning the “logic of discovery” (e.g., Nickles 1980a; Kapitan

1992; Paavola 2004a). In the following, I shall call this basically Peircean use of the term, ‘Hansonian abduction’, because Peirce’s own versatile writings have left also room for other interpretations of the term (Paavola 2004b). Further, Norwood Russell Hanson was an important forerunner in the 20th-century philosophy of science who researched the “logic of discovery”. Another way of using the term ‘abduction’ is to treat it as a synonym for the Inference to the Best Explanation (IBE) –model (see e.g., Psillos 2002). Peirce is one part of the background for IBE, but more importantly IBE is based on Gilbert Harman’s writings and formulations in the 1960s (e.g., Harman 1965). Instead of discovery, the discussions regarding IBE or “Harmanian abduction” (as it can also be called) have usually concentrated on the issues connected with realism (e.g., Day & Kincaid 1994).

Nowadays, these two lines of abduction are often taken more or less as one with the emphasis on IBE (e.g., Lipton 2004; Walton 2004). Gerhard Minnameier (2004) has, however, recently argued that these two senses (which he calls ‘abduction’ and ‘inference to the best explanation’) should be separated more clearly because Peirce’s abduction (here, Hansonian abduction) is the mode of inference that is intended to be “creative”, that is, to lead to new knowledge whereas IBE typically starts from already established hypotheses and is, in that sense, more akin to Peirce’s induction. Hence Hansonian abduction has greater potential in accounting for the early phases of scientific investigation. Minnameier maintains that this distinction would bring new light on several current problems in philosophy of science, such as issues concerning realism, or concepts such as coherence and unification.

In this paper, I will defend the idea that Hansonian and Harmanian abduction (i.e., IBE) should be separated, and illustrate some benefits of such a move. I will do so by analyzing Peter Lipton’s (2004) version of IBE as a model for discovery without trying to evaluate Lipton’s extensive model of IBE as a whole. I shall concentrate on those elements that concern discovery and deal with other issues only in passing. Lipton has argued that IBE *is* an

apt model for analyzing processes of discovery, especially in contrast to the traditional hypothetico-deductive (HD) model of inquiry. As an example, I will analyze Ignaz Semmelweis' research on childbed fever (also known as "puerperal fever"). I argue that there are elements both from Hansonian abduction and IBE in Lipton's model, but by using Hansonian abduction a more comprehensive account of Semmelweis' methodology can be given. I shall propose that Hansonian abduction—though in need of refinement-- is a better way of doing justice to the meaning of potential explanations and "loveliness" which Lipton emphasizes.

2. Lipton's model of IBE for discovery

Peter Lipton (2004, revised second edition; first edition, 1991) asserted that the model of Inference to the Best Explanation (IBE) is still nowadays very often taken as a slogan and should be developed further to "a principled account of the way we actually go about making non-demonstrative inferences" (Lipton 2004, 142; also pp. 2, 208-9). IBE is an advance, especially over the hypothetico-deductive model of inquiry, when it comes to discovery (Lipton 2004, 67, 82-91). The basic idea of IBE would still be the same as in Gilbert Harman's formulation:

"In making this inference one infers, from the fact that a certain hypothesis would explain the evidence, to the truth of that hypothesis. In general, there will be several hypotheses which might explain the evidence, so one must be able to reject all such alternative hypotheses before one is warranted in making the inference." (Harman 1965, 89; cf. e.g., Lipton 2004, 1, 56)

Lipton has developed Harmanian abduction (IBE) further, 1) by specifying *the model of explanation* on which IBE should lean, 2) by distinguishing between *actual* and *potential*

explanations, and 3) by proposing a distinction between *likeliness* and *loveliness* of explanations.

First, IBE seems to presuppose a good understanding of the model of explanation that is used because the link between IBE and the theory of explanation is so close (although this is not a necessary requirement – see Lipton 2001, 100). Lipton’s own basis for IBE is the causal model of explanation and the model of contrastive explanations, that is, explanations that answer questions of the form “Why this *rather than that*?” Scientists, in general, do not explain phenomena as such, but only some aspects of them, relative to some foils.

Secondly, Lipton (2004, 57-59) argues that one has to distinguish between actual and potential explanations. Potential explanations are like actual explanations, except for a truth requirement. The idea is that IBE cannot be inference to the best of *actual* explanations. Such inference is not plausible, nor usually made in our real-life practices of inference. It would imply that we always end up to actual--i.e., true--explanations with IBE. As Lipton writes: “[o]ur inductive practice is fallible: we sometimes reasonably infer falsehood” (ibid., 57). So we must assume that IBE is Inference to the Best *Potential* Explanation.

Thirdly, Lipton proposes a distinction between the likeliest and the loveliest explanations. Likelihood is connected to the most warranted explanation and “speaks of truth” whereas the standards of loveliness create “potential understanding” (p. 59). Lipton points out that IBE requires something more than just likelihood; his model of IBE is thus Inference to the *Loveliest* Potential Explanation. His intention appears to have the following basis: in order to provide something different from traditional deductive and inductive inference, explanatory considerations should be taken into account. Explanatory considerations, for Lipton, fall under the category of *loveliness*, bringing into the foreground, the special role of explanation in IBE (Lipton 2004, 60-1; cf. Psillos 2002, 617).

Lipton has developed his rendering of IBE explicitly in the direction of discovery by drawing a contrast with the traditional hypothetico-deductive (HD) model of inquiry. According to the HD-model, theories and hypotheses guide our observations. The way theories and ideas are generated is not so important, rather how they are tested. IBE (and abduction in general) turns this approach upside down. IBE proponents admit that there is no inductive route from observations to (higher-level) theories and hypotheses; but maintain that it is explanatory considerations, starting from the time one makes observations and examines phenomena that guide the search for hypotheses and ideas. So we have to discern other non-deductive forms of inference besides induction.

Lipton has developed a “two filter version” of IBE (ibid., 67, 148-151): First, explanatory considerations operate in this phase where one makes a selection of the best explanation from the candidates. This is how IBE is usually interpreted. But second, explanatory considerations also operate in the prior phase where one generates these candidates. Explanatory considerations can help one come up with a “short list” from which the best (and actual) explanation is to be selected. The phase of production of a short list is crucial for consideration because scientists cannot go through *all* possible explanations; that would simply take too long, if not forever (ibid., 149). Explanatory considerations, according to Lipton, may help to generate those background beliefs that help to restrict the range of candidates (ibid., 138-140, 150-1).

3. Hansonian abduction

In 1950s and 1960s N. R. Hanson applied Charles Peirce’s conception of abduction to analyze processes of discovery (e.g., Hanson 1958, 1961). Hanson’s basic idea was that although there are no mechanical rules for making discoveries, the pattern of undertakings and deliberation through which a scientist makes a discovery can be conceptualized (see Hanson 1961, 21-22).

According to Hanson, the inductivist model and the hypothetico-deductive model of inquiry are both logics for “the finished research reports,” which do not capture the essential phase of methodology, that is, processes leading to discovery. A basic formulation of abduction (see Peirce CP 5.189; Hanson 1958, 86) is the following:

The surprising fact, C, is observed;

But if H [an explanatory hypothesis] were true, C would be a matter of course,

Hence, there is reason to suspect that H is true.

Abduction is a mode of inference starting from an anomalous fact, proceeding to an explanatory, plausible hypothesis and accepting that hypothesis provisionally.

Hanson’s (and Peirce’s) formulations of abduction have been a target for criticism (e.g., Nickles 1980b, 22-25; Kapitan 1992) partly because of some inadequacies in his (their) formulations, and partly because, for a long time, the pattern of events--and the deliberation--leading to discovery was held to be impossible to analyze with conceptual models. I maintain that this Hansonian programme of interpreting abduction as a logic of discovery--meaning a conceptual way of analysing the undertakings involved in discovery – can and should be developed further (see Paavola 2004a). Abduction is taken to be a weak form of inference that relates to the first phase of inquiry. This “weakness” means that abduction is supposed to give plausible candidate hypotheses, not necessarily true explanations, which then have to be verified and tested by other means, that is, processes of deduction and induction (Peirce CP 6.469-473, 1908; 7.202-219, 1901).

I have argued that one important aspect of abduction as a logic of discovery is to interpret it from the strategic point of view (Paavola, 2004a; cf. Hintikka, 1998). In Peircean terms, this means that not just “*critic*” but also “*methodeutic*” is relevant; that is, a theory of abduction from the processual point of view must be further developed (Paavola, 2004b). The whole process of inquiry has to be taken into account, not just the relationship between

premises and conclusion. The bare formulation of abduction cited above gives too narrow a picture of the procedure. While making inquiries, the inquirer takes several “moves” of reasoning into account at the same time and tries to anticipate subsequent developments. From a methodological point of view, abductive moves should be seen as a part of an ongoing cycle of research. Background theories and suppositions guide the search for new ideas, but at the same time data and “surprising phenomena” instigate that search. Abduction is not just about one move from a surprising fact to an explanation but rather the deliberate way inquirers “press on” for new explanations on the basis of background theories and data requiring explanation, often in very long-term processes (Hanson 1958, 2-3, 88). Hansonian abduction is not connected to any specific model of explanation although the aim of finding “patterns” (Hanson 1958) in the data has connections to the idea of explanatory unification (Bartelborth 1999; Minnameier 2004).

Within this “abductivist” model, abduction is only relatively the first phase of inquiry because it is a part of an on-going cycle of research where all the information available is used, when promising new ideas are searched for (then tested or verified). From the point of view of testing and justification, a hypothesis has to be rejected if test results do not confirm it. But from the point of view of an abductive search, hypotheses that end up being rejected can still give elements and clues for new ideas. Abduction is a weak form of inference where clues and background information instigate and guide the search for new hypotheses and provide tentative constraints on the type of hypotheses searched for. It is not basically a one-step argument from an anomalous phenomenon to an explanatory hypothesis, but a web of arguments where anomalous phenomena, as clues, and background information, help one to find tentative suggestions, which operate as heuristic guides and restrictions for further search.

4. A comparison of Hansonian abduction, IBE, and Lipton's IBE

Hansonian abduction and basic versions of IBE have a lot of common. Both start from the assumption that it is essential to recognize a mode of inference that differs from deduction, and also from induction as it is typically or traditionally interpreted (Hanson 1961; Harman 1965). And in both of them, this mode of inference is closely related to explanation. Despite these basic similarities, there are also clear differences between them (see Minnameier 2004).

IBE starts from an assumption that it is a basic model for *all* non-deductive reasoning, induction being a special case of it (see Harman 1965), whereas (Hansonian) abduction, in the tradition of Peirce, is intended to be a third main mode of inference besides deduction and induction. This is connected to another difference. With IBE, the inquirer is supposed to end up to the *true* explanation although in non-deductive manner, whereas abduction is meant to be the first phase of inquiry, after which the hypothesis is tested or verified with deduction and induction. Hansonian abduction is a weaker form of inference than IBE; the inquirer gets plausible or potential candidates for further research, whereas IBE is supposed to provide the actual (or true) hypothesis. In Hansonian abduction, the generation and justification of hypotheses are separated whereas in IBE there is no such difference because candidate explanations and the data they are supposed to explain are usually taken as given. Abduction concentrates more on the *process* of inquiry starting from the idea that it is the first phase; especially if strategic and “methodeutic” aspects of abduction are emphasized (see Paavola, 2004a). Proponents of IBE have not focused on the process. In this sense it has *not* been so much an inference *to* the best explanation as an inference *that* the best explanation is true, which has led to debates if, or on which conditions it is a valid way of reasoning. This difference has meant that IBE has been important in debates concerning scientific realism; that is, whether one has reasons or justification to think that our best explanations or theories are true (see e.g., Day & Kincaid 1994), whereas abduction has been used in debates

concerning the logic of discovery; that is, whether one has any means to conceptualize how new ideas are generated (e.g., Nickles 1980a; Kapitan 1992; Paavola 2004a). None of these differences between IBE and abduction are categorical, but they indicate clear, underlying contrasts between IBE and abduction.

On the basis of these differences, one may argue that Lipton's way of developing or interpreting IBE is basically Harmanian, but in a way that has moved towards (what is here called) Hansonian abduction. Lipton's model is Harmanian IBE because it is based on the idea that the inquirer is using the data to get the *best* explanation by selecting from *competing* explanations and ending up at a *true* (or actual) explanation (if successful) (Lipton 2004, 1, 56). But Lipton has apparently moved from a "standard" version of IBE in at least three ways. According to him: 1) IBE should also deal with processes of discovery, and discovery and selection of candidate explanations should be distinguished; 2) actual and potential explanations should be distinguished, and IBE is basically an inference to the best potential explanation; and 3) explanatory considerations, i.e., issues concerning loveliness, are to guide one in discovery. Lipton acknowledges that IBE has been a rather passive, or static model of reasoning where we "simply infer whatever seems the best explanation of the data we happen to have" (Lipton 2004, 83; also p. 90). Lipton wants a more "active" model that takes into account how explanatory considerations guide the whole process. He also clearly wishes to develop IBE as a weaker form of inference than the standard versions by making a distinction between actual and potential explanations, and between likely and lovely explanations.

Although Lipton's IBE has moved closer to Hansonian abduction, it is still more akin to IBE than to Hansonian abduction. In Hansonian abduction, the generation of hypotheses is different from the testing or subsequent use of these hypotheses. Abduction is essential in the phase of generation, but it results only in potential or plausible hypotheses that should be tested or verified mainly through deduction and induction. Lipton makes a somewhat similar

distinction with his “two filter mechanism”. First a “short list” of plausible candidates is chosen, and then the best among them is selected (Lipton 2004, 59, 68, 149). In Lipton’s version of IBE, these two phases are, however, more similar to each other than in Hansonian abduction because, in both phases, IBE is said to obtain. Another difference is that in IBE the selection phase starts when the inquirer has several candidate explanations available from which to choose or eliminate other except the best one. In Hansonian abduction, the inquirer mainly produces and tests hypotheses one by one. In general, hypotheses are not so much compared with each other as tested with additional data. It is also beneficial to produce alternative hypotheses within Hansonian abduction because these alternatives can guide the search for new ideas and suggestions. But the point is not so much to produce alternatives from which to choose the best as to be able to produce a new fertile hypothesis if the previous one is not satisfactory.

Lipton emphasizes a distinction between actual and potential explanations. The notion of potential explanations comes close to the weaker Hansonian abduction. But the use of potential explanations is still problematic. It is not clear whether Lipton’s idea is that the candidates are potential, and the best one actual, or whether the best explanation is only potential also. There are some remarks that appear to make the latter interpretation the correct one. Inquirers, says Lipton, do not always end up with actual explanations; they can reasonably infer falsehoods, that is, merely potential explanations that turn out to be false (ibid., 57-8). One could argue that this is the situation in inductive reasoning in general¹, but Lipton clearly wants to emphasize this more explicitly. Thus it seems that we have to take into account that the best explanation is “only” potential. But this is problematic because the basic idea of IBE is that we have reasons to think that the best explanation is true. Consistent with this point, Lipton remarks, “According to the Inference to the Best Explanation, then, we

¹ I want to thank one of the reviewers of emphasizing this.

do not infer the best actual explanation; rather we infer that the best of the available potential explanations is an actual explanation” (ibid., 58). Interpreted in this way, however, there is not much difference from the standard versions of IBE – the inquirer has various candidates; when he/she chooses the best among them, he/she is supposed to get the actual explanation.

A basic similarity between Lipton’s IBE and Hansonian abduction is the idea that explanatory considerations are an important guide when candidate hypotheses are generated. For Lipton, this means that “loveliness”, not just “likeliness” is an important element in IBE. Lipton admits that the criteria and theory of loveliness should be developed further (2004, 71 see also p. 121). The basic proposal (and assumption) is that loveliness determines, at least in part, likeliness (see Lipton 2004, 61, 71, 121; see also Psillos 2002, 617). But this is also problematic. Eric Barnes (1995), who has analysed Lipton’s criteria for loveliness in some detail, discerns various criteria for loveliness (mechanism criterion, precision criterion, unification criterion, and elegance and simplicity criterion) (ibid., 257-265). Thus, an explanation is better and more lovely if it can specify the mechanism by which the phenomena in question were caused, or more precise details of the explanandum, or if it is more unificatory, simple or elegant than its rivals. Barnes’ argues, however, that detailing such criteria of loveliness do not, however, actually improve Lipton’s theory because loveliness does not determine likeliness but just the opposite. The best explanation reveals the relevant causal history behind the phenomena (ibid., 270), and if the explanation does this, it offers also most potential understanding, i.e., it is most lovely. Barnes also argues that one can have precise, or unificatory, or elegant and simple explanations—lovely ones, to all appearances-- but which are not good explanations because they are not likely. Lipton’s own example is conspiracy theories. They are often lovely but very unlikely, and for that reason they are not usually very good explanations, and rarely the best ones. On the other hand, it seems that, sometimes, best explanations do not fare well against these criteria of loveliness;

for example, messy and complicated explanations can be more “lovely” than simple explanations. Lipton himself presents two basic counter-arguments against loveliness (Lipton 2004, 70, 142-148): 1) “*Hungerford’s objection*”; loveliness is too subjective and interest relative to be an objective criterion for reasoning; 2) “*Voltaire’s objection*”; what reasons do we have to believe that the loveliest explanation would also be the most likely to be true; why would we think that we live in the loveliest of all possible worlds?

I cannot delve into this question in detail here, but I maintain that loveliness is more promising in relation to Hansonian abduction than in IBE. Loveliness is difficult for IBE because the criteria for loveliness, which are at the same time criteria for the best explanation, should be, if not a guarantee, at least a strong indication of the truth of an explanation (Lipton 2004, 60-61). The dilemma for IBE, then, seems to be the need to emphasize loveliness along with the special character of IBE. But doing so undermines IBE’s credibility as a plausible form of inference to truth. Abduction fares better in these respects because it is, from the start, a weaker form of inference than IBE, and it separates the generation and justification of hypotheses more clearly. Loveliness is no guarantee of finding the *best* explanation, but it can be an ancillary criterion when searching for promising candidates or ideas. It can be an indication of a good, potential hypothesis, although the hypothesis must then be tested with other means. I think Barnes is right in pointing out that likeliness is more important when it comes to evaluating how well good explanations succeed as “final” explanations. In the Peircean/Hansonian approach, this requires deductive and inductive means, and testing and verification of hypotheses. But Lipton rightly points out that in itself likeliness is a poor guide in the process of discovery. To discover something new means that likely explanations are not enough. Likeliness can thus be one criterion when new ideas are searched for, but not the only one.

According to Hansonian abduction, the overall goal might be to find an explanation that would be lovely in relationship to all the relevant data, but the ideas might first relate to some specific details (that is the way detectives use clues). It is doubtful, I would argue, whether there can be any strict criteria for loveliness, at least in a quantitative sense, but still it appears that an explanation is lovelier than another if it explains some minute details; or even more so, if it explains several details. This scope of explanation is also how “as a matter of course” may be interpreted in the basic formulation (see chapter 3) of abduction. The inquirer can tentatively try a hypothesis that is lovely in relation to some specific anomalous fact, but the goal is to find a hypothesis that would explain things “as a matter of course” in connection to *all* the relevant data, and would also furnish details about how things happen according to this hypothesis (see Paavola, 2004a). As an answer to “Voltaire’s objection” above: loveliness is no guarantee that we have found a likely explanation, and it might be that the most lovely explanation is not true, but still, loveliness is one good (strategic) guide when true and likely explanations are searched for. Loveliness might also be an interest-relative and subjective matter, depending on the clues and background theories of the inquirer, but this is not a problem for objectivity if the abductive phase is only a preliminary phase in inquiry.

In IBE there is a tendency to take candidate hypotheses as given, and maintain that the best explanation is chosen by eliminating competing explanations until the best one is found. This results in a “bad lot problem” for IBE. How can we know that an actual explanation is among those alternatives, from which the apparently ‘best’ is chosen? The best from wrong or bad alternatives is not an actual or true explanation, and how can we know when we are in this situation? The solution according to *Hansonian* abduction is to maintain that there is no such “lot” or “pool” from which hypotheses are chosen. The inquirer produces new explanations and various lines of inquiry until she or he finds one that appears to be satisfactory (after applying tests or using some other criteria). And with abduction, the

inquirer essentially concludes that phase of inquiry with potential hypotheses which have to be tested and developed through deductive and inductive means to become something that the inquirer can consider (at least at that time) actual explanations.

One strength of IBE in relation to Hansonian abduction is that it takes the comparison of candidate hypotheses into account. Such explicit comparison is missing in the basic version of Hansonian abduction. I propose that this aspect is, however, implicit in the Hansonian model although it should be made explicit when a processual viewpoint on abduction is developed further (e.g., in Paavola 2004a). In any case, a comparison is more “local” in Hansonian abduction than in IBE, that is, it concerns more one hypothesis at a time than a selection from many evident alternatives. The starting point for abduction is surprising or anomalous phenomena which indicate--provide a supposition that--there are no convincing explanations known for them. When a candidate explanation is produced, it is done implicitly in comparison to alternatives; if better explanations are evident, two basic alternatives are open: either to give up one’s alternative, or to develop it further to be a better one.

5. Semmelweis’ case from an abductive point of view

A comparison between IBE and Hansonian abduction will become clearer through considering an illustrative case. Ignaz Semmelweis' research on childbed fever is suitable because C. G. Hempel (1966) previously reconstructed it as an example of the hypothetico-deductive model of inquiry, and Lipton (2004) uses it as a central case to delineate IBE. I shall first summarize this dramatic story in a detailed way, not just because it is a good case for illustrating aspects of non-demonstrative reasoning within inquiry, but also because my aim is to delineate “abductive behaviour” (cf. Lipton 2004, 210), including those restrictions, clues, and searches which, I hypothesize, operated during Semmelweis’ research.

A Hungarian physician, Ignaz Semmelweis (1818-1865), discovered the cause for the disease that killed thousands of mothers and newborn babies all around the world. Working during 1844-1849 in a Viennese maternity hospital as an assistant, he carried out his research over several years. Childbed fever was a special concern for Semmelweis because, at the first clinic of the hospital where he and all the medical students worked--in contrast to the second clinic where midwives worked-- there were systematically more cases of childbed fever. Semmelweis was very much devoted to his work, as shown by his eager research efforts, which included conducting autopsies in the nearby pathological department to find out more about anatomy. At that time (1844), there seemed to be no way of avoiding or controlling the disease, nor of ascertaining its causes. There were many kinds of theories presented before and during Semmelweis' time about childbed fever. One general belief was that childbed fever was some sort of an epidemic disease caused by some "atmospheric-cosmic-terrestrial changes" (Semmelweis 1983, 65). To Semmelweis, these explanations did not seem believable because it would have been a mystery how these epidemic factors were systematically more influential in the first clinic compared to the second clinic and to the surrounding countryside. Semmelweis became convinced that the reason must be some sort of "endemic" factors, that is, factors operating in the first clinic (ibid., 67-69). Such "endemic" factors were attractive as potential causes for the disease, but many of these causes faced a similar and fatal difficulty; if they were the cause for childbed fever, there should have been no difference between clinics, or the mortality should have been higher in the *second* clinic. For example, *overcrowding* was one hypothetical explanation, but because of the bad reputation of the first clinic, the second clinic was more crowded since everyone wanted to avoid the first clinic (ibid., 69). People were very frightened to come to the hospital, especially to the first clinic because of its reputation. It was suggested that this fear could be a cause of the disease. But Semmelweis reasoned that a psychological factor like fear could not

have caused those physical changes that childbed fever caused, and it would not have explained how the difference between clinics originated (ibid., 70-1). One specific hypothesis was that fear caused by the religious practices could be the cause for the fever. That was because in the first clinic, but not the second, the Catholic priest and an assistant with a bell giving the last rites, regularly went through the clinic, which of course caused much anxiety. Semmelweis managed to change this practice but it did not lessen the mortality (ibid., 71-73). Because obstetricians and medical students worked in the first clinic and midwives in the second, it was suggested that the difference was because of these students' rougher manners in examining the patients that could have caused the difference. Semmelweis could not believe in this explanation because the birth itself was much harder experience for women, and so should have caused childbed fever in itself. *Bad conditions, ventilation, and diet* were also blamed, but there was no difference in these to the disadvantage of the first clinic (ibid., 74-5). Also various kinds of factors related to mothers' organism before, during and after the birth were suspected (see ibid., 76), but the problem was that these were equal in both clinics. During his research, Semmelweis "became aware of other inexplicable circumstances" (ibid.). If the period of dilation was extended the risk of getting childbed fever rose clearly. This was curious because it happened very often to those mothers who delivered their first baby, and who were otherwise young and healthy. Also newborn babies died of the childbed fever, and Semmelweis reasoned that many of those explanations offered could not pertain to newborn babies. Also so-called "street-births," births that happened on the way to the hospital, and premature births seemed to protect from the childbed fever, which was also odd because in general they were a risk both for the mother and for the child (ibid., 80-83). Some other strange details had also caught Semmelweis' attention; very often mothers in the first clinic got the disease so that it appeared in rows, whereas in the second clinic these cases were sporadically distributed to the whole clinic (ibid., 83). Various commissions were set up to

investigate the difference between the clinics. One of them (at the end of 1846) came to a conclusion that male students, particularly foreigners, were causing the disease with their rough manner of examination (ibid., 83-4). The number of foreigners allowed to do their practical training in Viennese, was reduced. The mortality rate reduced for a while, but then rose again. At this time, Semmelweis was desperate to find the cause or remedy, and “like a drowning person grasping at a straw” (ibid., 87) even changed the customary delivery position in the first clinic (even though he didn’t believe that this could be the cause) because it was different compared to the second one. But nothing helped.

A decisive clue came on March 1847, when his colleague Kolletschka died of blood poisoning from a puncture wound in the finger, received while conducting an autopsy. Semmelweis realized that the cause of the childbed fever might be a similar kind of a blood poisoning:

“Day and night I was haunted by the image of Kolletschka’s disease and was forced to recognize, ever more decisively, that the disease from which Kolletschka died was identical to that from which so many maternity patients died.” (ibid., 88)

Semmelweis tested this hypothesis by arranging a chemical wash for all who examined mothers at his clinic. After a while, the mortality rate lowered dramatically. The cause was that doctors and medical students brought “cadaveric matter” (or as we now say, microbes and bacteria) to the mother's bloodstream while doing examinations. This was not understood at the time and not even admitted for long after Semmelweis’ research partly because the germ theory of disease was not known. So in many lying-in hospitals, including the one where Semmelweis worked, doctors (and assistants like Semmelweis) conducted medical research and dissections in the autopsy room. After that, without doing any disinfection of

hands (or, at best, using only soap and water), they came to conduct examinations of mothers in lying-in departments and deliver their babies.

This cadaveric hypothesis also explained many phenomena that Semmelweis knew. It operated in the first clinic; it made the role of medical students and foreign students understandable (they often made examinations in the morgue before coming to the clinic, as did Semmelweis himself); it explained how babies also got the disease (through a common bloodstream): it explained why premature births and street-births protected from the disease (because then there were fewer chances of getting into medical examinations), and it accounted for cases where the disease occurred in rows (medical examinations were made in rows by students and doctors) (*ibid.*, 98-101). This was not the end of his research on childbed fever. After a while, the mortality rose again dramatically (*ibid.*, 93). Semmelweis realized that the pus or “cadaveric matter” from the wounds of patients, not just from the autopsy room, could also cause the childbed fever. Matter from a festering cervical cancer of one mother in the first clinic infected many women before Semmelweis realized that this also caused childbed fever.

I maintain that Semmelweis’ case fits nicely with Hansonian abduction. Semmelweis was systematically and persistently, for many years, searching and testing explanations in order to address his main concerns: How can one reduce the cases of the childbed fever, and what is the cause of this disease? A basic “anomalous phenomenon” for him was the difference between clinics although during his research, he collected various kinds of information, clues, and curious phenomena that he considered in seeking new hypotheses (see above). He was always striving for new, plausible candidate hypotheses when previous ones were not successful, until he found an effective way of reducing the mortality of the childbed fever and a plausible explanation for it. Semmelweis constructed tentative hypotheses in relationship to observations and facts using these as triggers or instigating factors. Even those

hypotheses that were most unlikely for him were triggered by observations and facts (for example, the religious sacrament really caused fear among the patients, and the delivery position was different in the two clinics).

Important from an abductive point of view are various tentative restrictions that helped him to find fertile candidate hypotheses (Semmelweis 1983, 84-86). He was convinced that the mortality was caused by endemic factors related to the first clinic. He also knew that medical students and especially foreign students seemed to be more involved with this disease than midwives. Further, he had also come to the conclusion that psychological explanations were not plausible because that would have made it curious how unborn babies could get the disease. These and other constraints were tentative because Semmelweis could not be sure whether there were some extra causes that would have made these supposed restrictions wrong (for example, suppose that the way babies got this disease had been different from that of their mothers). But these restrictions and clues helped Semmelweis to notice that the “decisive” hypothesis, prompted by Kolletschka’s death, was so promising. The hypothesis fitted nicely with these restrictions and made many previously puzzling phenomena understandable.

The process of finding the cadaveric hypothesis is in itself a good example of an abductive inference. In its basic form, abduction is an inference from some constellation of curious or characteristic features (with a proviso that these features would follow from some hypothesis) to a conclusion that this kind of a hypothesis is a good candidate explanation in this specific case (see Paavola, 2004b, 256-257).

P(features) ₁	}	P ₁ are similar to P ₂
H(hypothesis) ₁ → P ₂		
∴ Maybe H ₁ (or something that is similar to H ₁)		

In this sense, in typical cases, abduction is reasoning backwards from some effects to their potential causes, or to the potential type of a hypothesis. Semmelweis realized that symptoms in Kolletschka's death (lymphangitis, phlebitis, bilateral pleurisy, pericarditis, peritonitis, and meningitis) were similar to those in the childbed fever, and drew an abductive conclusion that maybe the cause was also similar (Semmelweis 1983, 87-88).

I maintain that criteria connected to loveliness were important for Semmelweis when he searched for new hypotheses. The cadaveric hypothesis is a good example. The idea that childbed fever could be caused by some sort of a blood-poisoning, was lovely in relation to the symptoms of the childbed fever. For Semmelweis, it was lovely also because it appeared to provide a detailed mechanism that explained the difference between the two clinics of the hospital. This hypothesis also provided an explanation for many small and curious details: why there were sometimes cases of the disease in rows, why street-births protected from the disease and so on.

In abduction, the use of strategies in the course of inquiry is essential. During his inquiries, Semmelweis did *not* use all the evidence or information when searching for new hypotheses. He often used just some parts of the information in order to get some new potential ideas to be tested or evaluated; information like the fear caused by religious sacrament, or the diet of the hospital. These hypotheses were unlikely in themselves, but would have been lovely explanations related to these specific details.

The cadaveric hypothesis was still only a hypothesis and a potential explanation before it was tested although it became very attractive to Semmelweis after he considered it (Semmelweis 1983, 88). *After* these tests, there was good evidence that it was a likely hypothesis. The antisepsis helped to reduce the cases of the disease dramatically. When cases of childbed fever then again rose clearly after the case of the cervical cancer, the hypothesis

was so lovely for Semmelweis that it is no wonder that he immediately came up with a revised explanation, one with more details and broader application.

In the hypothetico-deductive (HD) reconstruction, Hempel emphasizes, rightly, that there are no mechanical rules from data to hypotheses (Hempel 1966, 10-18). Instead of inductively collecting data and generalizing on the basis of them, Semmelweis tested various kinds of hypotheses during his research. The HD-model fits well with the idea of *theory-ladenness* of observations and inquiry, i.e. that theories and hypotheses guide inquiry, even those problems that are dealt with (see *ibid.*, 12). But as Lipton's IBE and Hansonian abduction both emphasize, this idea leads to a view that one-sidedly emphasizes the role of theories and hypotheses over observations and empirical findings, and neglects rational elements involved in the phase of generation of hypotheses (Lipton 2004, 82-84). Theory-ladenness, and the testing of hypotheses are only a half of the story (cf. Bartelborth 1999, 212). The aim of finding a way of reducing childbed fever guided Semmelweis' research, and he used observational clues and restrictions when generating hypotheses (see above). Even the (un)lucky accident of Kolletschka's death, which brought out the cadaveric hypothesis, was guided by observations (symptoms of the blood-poisoning and of childbed fever) and previous tentative restrictions. It can then be said that theories and hypotheses are also "*observation-laden*". In abductive methodology, theory-ladenness and "observation-ladenness" are not alternatives; both are operative in dynamical processes of inquiry, as Hanson emphasized. Inquirers using abductive reasoning reflect and "press on", often for many years (Semmelweis' research as presented above lasted over 3 years) from surprising data to patterns of explanation, but these explanations must be tested and verified; and they crucially influence how things are seen and interpreted (Hanson 1958).

Lipton's IBE reconstruction of Semmelweis' case is based on the idea of a contrastive explanation; the difference between two clinics was a central starting point for his research.

According to IBE, first a pool of candidate explanations are generated, and by eliminating others, the best is chosen (if it is good enough). Lipton does not explicitly state what the pool of candidates for Semmelweis was, but it seems that it is a three-fold classification of hypotheses presented in the start of Lipton's reconstruction (Lipton 2004, 80): 1) hypotheses that did not mark a difference between the clinics, and were for that reason rejected (for example, epidemic influences), 2) hypotheses that did mark a difference, but where the elimination of the putative cause did not affect to the mortality (for example, the hypotheses concerning fear caused by the religious sacrament), 3) hypotheses that did mark a difference and where the elimination of the putative cause eliminated the difference in mortality (the "decisive" hypothesis was of this type). A central characterization is "Through the use of judiciously chosen experiments, Semmelweis determined the loveliest explanation by a process of manipulation and elimination that left only a single explanation of the salient contrast" (Lipton 2004, 90).

Lipton's IBE comes very close to Hansonian abduction, especially in his reconstruction of Semmelweis' case, but there are also clear differences. IBE cannot properly take into account the actual *process* of inquiry (although this is Lipton's aim; see Lipton 2004, 142, 208-9). Semmelweis' starting point included data other than the contrast between clinics; he did not use a "pool" of candidates, and he did not find the best explanation by eliminating competing hypotheses. These aspects belong to a reconstruction done afterwards, so they are not wrong as such, but they do not describe the process of Semmelweis' research.

Semmelweis used all kinds of clues and information as help when he searched fertile hypotheses. These helped him to find tentative restrictions and clues. So it was *not* so much two "filters" (generation and elimination) that guided his research as various kinds of restrictions that helped him to delimit the search for new ideas in the successive phases of his inquiry. And he did not produce any "pool" of candidates from which he would have

eliminated the candidates. Rather he generated alternative explanations, which he tested and evaluated one by one until he found an explanation that worked. Before the cadaveric hypothesis, Semmelweis did not choose any explanation as the “best”. As Lipton points out, this was because he considered these hypotheses to be wrong; so he was not comparing them but testing them one by one (Lipton 2004, 75-80). And he did not simply choose the cadaveric hypothesis because it seemed lovely or looked like the *best* explanation of those alternatives but because relying on it helped him to reduce the mortality and gave him a plausible explanation of the causes of childbed fever. It passed the crucial tests he arranged. He did not have a pool of hypotheses at hand even after the cadaveric hypothesis, as shown by the fact that he had to broaden his theory. So, from the point of view of discovery, he proceeded abductively, specifically in a Hansonian manner, pursuing new kinds of tentative explanations and theories which were suggested by various kinds of information providing clues and restrictions for the inquiry.

6. Conclusion

All in all, I maintain that Hansonian abduction is a better model than IBE (Harmanian abduction) when processes of discovery are to be conceptualized. Harmanian abduction is more akin to a situation where alternative candidate explanations are available, and the debate is *whether* we have, or on what grounds, reasons to think that best of these is also true. In this paper I am not analyzing the merits of IBE, or comparing it to Hanson’s or Peirce’s accounts from this justificatory point of view. I argue that IBE is not so amenable to a processual viewpoint as Hansonian abduction (cf. Hintikka 1998, 507-8). Lipton makes an important effort to develop IBE so that it would take more processes of discovery into account, especially by making distinctions between actual and potential explanations and between lovely and likely explanations. I maintain, however, that these distinctions would be more

useful within such an “abductivist” methodology, where Hansonian abduction is prevalent in the phase of discovery. Hansonian abduction is a “weaker” mode of inference than IBE, a weakness which, from the point of view of discovery, becomes its strength. Lipton’s IBE still focuses on opening the black box of our *inductive practices* (Lipton 2004, 71, 164) whereas Hansonian abduction concentrates on opening the black box area of discovery of fertile candidate hypotheses.

A limitation of these kinds of methodological models (IBE, the HD-model, and Hansonian abduction) is that they create a picture of inquiry as if it were only reasoning. In actual cases, many other forces and motives besides reasoning drive inquiry. Material, psychological, social, cultural, biological matters are largely omitted from this analysis although it may be maintained that they also have an important role in guiding and constraining the abductive search for new ideas (see Paavola & Hakkarainen 2005). But there is a role for “pure” abductive methodology. The hypothetico-deductive model of inquiry has emphasized such things as luck or “happy guesses” as an important element in discovery (Hempel 1966, 15; cf. also Lipton 2004, 81, 169). A Hansonian abductive analysis of Semmelweis’ research points out that although there were elements of luck in Semmelweis’ research, they were part of an overall processes of inquiry that may be mapped conceptually. By emphasizing the systematic role of clues and constraints in the search for fertile ideas, the abductive model bring to life the old saying: “Chance favors the prepared mind.”

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