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Peirce-Suit of Truth

Why Inference to the Best Explanation and Abduction Are Not the Same and How This Relates to Current Debates in Philosophy of Science and Epistemology

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PEIRCE-SUIT OF TRUTH – WHY INFERENCE TO THE BEST EXPLANATION AND ABDUCTION ARE NOT THE SAME AND HOW THIS RELATES TO CURRENT DEBATES IN PHILOSOPHY OF SCIENCE AND EPISTEMOLOGY

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ABSTRACT. It is well known that the process of scientific inquiry, according to Peirce, is driven by three types of inference, namely abduction, deduction, and induction. What is behind these labels is, however, not so clear, since abduction and induction are often interchanged in meaning or subjected to one another. What's more this scramble is not restricted to these terms but also applies to related notions such as explanation or coherence. The present paper tries to sort this out to some extent with respect to Peircean epistemology and to show how relevant Peirce's logic of inquiry really is if it is properly understood and put in context with current lines of philosophical debate.

1. INTRODUCTION

It is well known that the process of scientific inquiry, according to Peirce, is driven by three types of inference, namely abduction, deduction, and induction. What is behind these labels is, however, not so clear, since abduction and induction are often interchanged in meaning or subjected to one another. The main contention of this paper is that with respect to currently ongoing philosophical debates clearing this jungle is most rewarding. Let us summarise what is at stake.

"Inference to the Best Explanation" (IBE) – often also referred to as "abduction" – has been championed by scientific realists (e.g. Boyd 1990, 360-363**;** Lipton 1991, 167-168**;** Psillos 1999, chap. 4) and currently plays an important role in modern accounts of theory confirmation and epistemic justification within a broadly coherentistic perspective (see e.g. BonJour, 1985; 98-100; Bartelborth, 1996; chap. IV). Partly this was a reaction to the failures of hypothetico-deductivism (not to be reviewed here, see e.g. Glymour 1980; Gemes 1998) and with it the claim that theories could be evaluated in a strictly deductive manner (see Spohn 2001, p. 44). For quite some time Bayesian probability calculus was thought to be able to fill the gap, but Bayesian accounts, too, have proved to be limited (cf., e.g., van Fraassen 1989, chap. 7; Earman 1992**)** Can IBE do the job? Yes and no, as I am going to argue. On one hand, van Fraassen has quite rightly made the point that such inferences "may well be the best out of a bad lot" (1989, p. 143) and thus beg the question. On the other hand, a reconsideration of Peirce's concept of "qualitative induction" (which replaced his early concept of abduction, or "hypothesis" as he then preferred to call it) may nevertheless be adequate, as long as it is not understood in the sense of approximate truth. This concept of induction (or IBE), to my mind, would also match with the sense in which even van Fraassen holds that "(t)he question whether a theory as a whole is true or false makes sense" (1997, p. 529).

This is, of course, only half the story as "abduction" is also relevant in yet another context, namely that of (scientific) discovery. In his later writings Peirce stood on its head his previous account of logical inferences and assigned this new role to abduction (whereas before it had that of IBE, see below, section 2). Hanson (1958) has tried to build on that, but with not much success.[1] At any rate, however, it is clear today that the question of scientific discovery really is a philosophical problem. Whereas in the hay-days of logical empiricism and falsificationism the quest for a logic of scientific discovery was relegated to psychology (see Popper 1959, 30-32), we are now aware that there is more to discovery than merely a selection mechanism operating on "(h)appy guesses" (Hempel 1966, 15, who took it over from Whewell)[2]. The point is not only that the chance of hitting onto some promising hypothesis by random choice is extremely low, but also that theories obviously abstract deeper

structures from the phenomena in their intended scope and thus involve some sort of "emergent novelty"[3] on the part of the one who conceives them. What's more, the way we understand scientific concept formation today in the framework of the structuralist or semantic conception of theories – limited as it may as yet be (see e.g. Suppe 2000; Da Costa & French 2000) – is a far cry from earlier instrumentalistic interpretations (see van Fraassen 1997; Bartelborth 1998, p. 213). These few remarks should suffice to emphasise that the process of scientific discovery deserves an account of its own. I will try to show that Peirce's concept of abduction is a suitable candidate for it, even though Peirce himself did not really get beyond the idea of "happy guessing", as I shall argue below in section 3.

To sum up the two points just made one could say that hypothetico-deductivism – in a broad sense – leaves us with two open flanks, i.e. a missing logic of (scientific) discovery and a missing logic of confirmation or justification. While Popper, Hempel and others once wanted to ground the whole process of testing hypotheses solely on a deductive basis, a systematic hiatus gapes on either side of the deductive argument, begging the question of how we come about with promising theories in the first place and with respect to their overall evaluation.

In what follows I intend to show how these tasks can be fruitfully tackled with the help of Peirce's *trivium* of abduction, deduction and induction. As for IBE I will argue that it can be equalled with Peirce'c concept of "qualitative induction" (that also fits well into a general concept of induction). One crucial result of my analysis will be that abduction and IBE are not the same at all, since abduction covers the process of theory generation (or concept formation) whereas IBE relates to processes of justification.[4] This important distinction also allows us to unravel the conceptual tangle of abduction and induction both with reference to Peirce and in the broader philosophical context.[5]

On top of this, the two "missing links" with respect to hypothetico-deductivism will not only be clearly distinguished, but also dynamically related to each other (in terms of a feedback loop from induction to abduction). What's more, this perspective on the process of inquiry can be extended to include inferential relations over and above a local belief-system (tied together by abduction, deduction and induction), i.e. how it coheres with the whole of – or other parts of – our background knowledge. I believe this may help to further specify and integrate important aspects of concepts like coherence, explanation and unification.

I will start my discussion by recapitulating Peirce's move from his old to his new theory of inferential reasoning (section 2). In section 3 the concepts of abduction and induction will be analysed in their dynamic interaction together with deduction. In terms of a "logic" of inquiry the question arises whether – or in what sense – abduction and induction are logical inferences at all, which will be addressed in section 4. One outcome of this analysis is that there are characteristic feed-back and feed-forward relations between the three types of inferences that also binds induction back to abduction. This circularity, however, is not only not vicious, but also entails a dynamical and contextualised notion of "truth" that is neither absolute nor approximate and which rests on a current version of "direct realism" that Putnam has recently dubbed "natural realism" (section 5). Section 6 discusses abduction in the context of abstraction and the emergence of complex cognitive structures, which I feel is quite rewarding in the context of current accounts of coherence and unification, as is then laid out in section 7.

Before starting off, one caveat should be in order: As for Peirce himself my chief concern is not an historical one. I do not pretend to deliver *the* ultimate reconstruction of "what he really meant" (although I believe my reading yields a rather coherent account of his main ideas), nor do I attempt to resolve certain controversies about his views. What I am aiming at instead is to reveal his extraordinary relevance with respect to current philosophical debates and an integration of major trends in epistemology and philosophy science (maybe also philosophy of mind).

2. PEIRCE'S REINTERPRETATION OF HIS THREE TYPES OF INFERENCE

As is well known, Peirce drew his original concept of abduction (which was then called "hypothesis") from an analysis of Aristotle's classical syllogisms. He found that *modus barbara* allowed for three different types of inference, and called the third one "hypothesis" (the term "abduction" came in only later). This is illustrated by his famous "beans"-example:

DEDUCTION.
Rule. – All the beans from this bag are white.
Case. – These beans are from this bag.
∴ *Result.* – These beans are white.

INDUCTION.
Case. – These beans are from this bag.
Result. – These beans are white.
∴ *Rule.* – All the beans from this bag are white.

HYPOTHESIS.
Rule. – All the beans from this bag are white.
Result. – These beans are white.
∴ *Case.* – These beans are from this bag.
(CP 2.623, 1878)

By the end of the 19th century Peirce had dumped this conception and stopped using the syllogistic form henceforth.

> I was too much taken up in considering syllogistic forms …, which I made more fundamental than they really are (CP 2.102, 1902).

> Now let us remove the scaffolding of syllogistic forms which has served as our support in building up this theory and contemplate our erection without it (NEM IV, 184).

The reason for this change is twofold. On one hand Peirce realised that what he had called "hypothesis" "could not be the reasoning by which we are led to adopt a hypothesis, although I all but stated as much" (CP 2.102, 1902). By that time, Peirce identified "abduction" with the process of generating (novel) explanations for puzzling facts, for which his former account was entirely unfit. On the other hand it became clear that the "hypothetical inference" was not really an inference of its own, but rather a variant of induction.[6] Therefore in his later work, "hypothesis" is renamed as "qualitative induction" (cf. NEM III/2, 874, 1909).

The identification of hypothesis with induction is, however, only half the story as also Peirce's concept of induction had undergone a significant change. Whereas he first regarded both types of inferences as ampliative, he now puts this all down to abduction:

> (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).

So it is not only that "hypothesis" is to be subsumed to "induction", but rather that 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. Consequently Peirce states that he had "more or less mixed up Hypothesis and Induction" (CP 8.221, 1910). How the

mature Peirce conceptualises the dynamical and conjoint functioning of all three types of inference is perhaps best expressed in the following passage:

> Abduction is the process of forming an explanatory hypothesis. It is the 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.
> Deduction proves that something *must* be; Induction shows that something *actually is* operative; Abduction merely suggests that something *may be*.
> Its only justification is that from its suggestion deduction can draw a prediction which can be tested by induction, and that, if we are ever to learn anything or to understand phenomena at all, it must be by abduction that this is to be brought about (CP 5.171, 1903).

In the next two sections I am going to analyse Peirce's revised conceptions of abduction and induction.

3. ABDUCTION, INDUCTION, AND THE DYNAMIC INTERACTION OF THE THREE TYPES OF INFERENCES

In his later logic of inquiry Peirce's concept of abduction stands for the inference from surprising facts to possible explanations as in the generation of scientific theories.

> The abductive suggestion comes to us like a flash. It is an act of insight, although of extremely fallible insight. It is true that the different elements of the hypotheses were in our minds before; but it is the idea of putting together what we had never before dreamed of putting together which flashes the new suggestion before our contemplation (CP 5.181, 1903).

It is one of Peirce's great merits that he has drawn our attention to the creative generation of novel concepts and to the fact that these cannot be generated inductively. Indeed, today we are well aware that ordinary induction can at best produce "more of the same" (cf. e.g. Lipton 1991, p. 16) instead of novel kinds of knowledge, let alone more abstract ones. We also have good evidence that classical inductive generalisations from larger samples hardly play any role in actual science (cf. Hintikka 1992; Suppe 1997). However, the question of how we manage to set forth creative and at the same time – by and large – highly successful hypotheses is not answered by Peirce in a satisfying manner. He only invokes some sort of natural gift (purportedly formed by past evolutionary processes) that allows humans to generate promising ideas (cf. CP 5.172-173).

This does not explain much, but reflects about the state of the art in Peirce's time. Today we have better theories from various sciences – especially thermodynamics and quantum physics – to explain emergent processes in general (cf. Minnameier, 2000a).[7] (And that Peirce's notion of abduction is essentially about discontinuous phase transitions in dynamical epistemic processes has been argued by Pape 1999.) Of course, we cannot and need not deal with such issues here, but one important outcome should nevertheless be stressed, namely the fact that emergent processes turn out to be far less randomly generated than is commonly supposed. Rather are they largely determined by the context in which they take place. As a consequence, abduction ought not to be mistaken for "happy guessing", since in abduction much is down to the proper formulation of a problem and the collection of premises to be accommodated by some theory (see section 4).

Abduction, according to Peirce, yields new insights, but highly speculative and fallible ones (despite our good instincts). The role of *deductive* inferences is to derive the consequences of what is assumed in the hypothesis. Those consequences are logically necessary only on the level of linguistic analysis, but all of what can be deduced depends on the validity

of the premises and necessarily remains within their scope. Hence, testing whether the assumptions and deduced consequences really obtain, and evaluating to what extent they obtain, is the task of induction on Peirce's view.

> Induction consists in starting from a theory, deducing from it predictions of phenomena, and observing those phenomena in order to see how nearly they agree with the theory (CP 5.170, 1903).
> (Induction) has three parts. For it must begin with classification … by which general Ideas are attached to objects of Experience; or rather by which the latter are subordinated to the former. Following this will come the testing-argumentations, the Probations; and the whole inquiry will be wound up with the Sentential part of the Third Stage (i.e. induction; G.M.). which, by Inductive reasonings, appraises the different Probations singly, then their combinations, then makes self-appraisal of these very appraisals themselves, and passes final judgment on the whole result (CP 6.472, 1908).

This establishes an entirely new role for induction, because now it is seen as an inference from theory to facts[8] rather than from facts to theory (or better: "rules" in the latter case, in order not to confuse empirical generalisation with the generation of a theory about such a generalisation). Peirce states that induction is necessarily preceded by abduction and deduction so that the ampliative move usually associated with it is supposed to happen before. Consequently, the inductive step is only about evaluating the suggested hypothesis. As we can see from the above quotation this involves three subprocesses: subsumption of concrete events or situations to abstract concepts, the actual probing, and the final step of judging

> whether the hypothesis should be regarded as proved, or as well on the way toward being proved, or as unworthy of further attention, or whether it ought to receive a definite modification in the light of the new experiments and be inductively reexamined *ab ovo*, or whether finally, that while not true it probably presents some analogy to the truth, and that the results of the induction may help to suggest a better hypothesis (CP 2.759, 1905)

It should be noted that "to regard something as proved" must not be understood in terms of an ultimate proof. Peirce quite explicitely states that

> neither Deduction nor Induction contributes the smallest positive item to the final conclusion of the inquiry. They render the indefinite definite; Deduction Explicates; Induction evaluates: that is all. … (W)e are building a cantilever bridge of induction, held together by scientific struts and ties. Yet every plank of its advance is first laid by Retroduction alone, that is to say, by the spontaneous conjectures of instinctive reason (CP 6.475, 1908).

So, if the results of empirical tests are positive, the governing theory is not proved in a strict sense. Nevertheless we can draw two epistemologically relevant consequences: First of all we may be justified in believing that the initial puzzling situation that led to our tentative theory is really explained by that theory, so that we inductively infer that things actually happened in the way the theory suggests. Secondly, we may be justified in expecting similar effects in similar circumstances, so that we accept the theory as a future guideline for the time being – provided there is no equally good or even better explanation to compete with the one in question.

This result is quite important, because – as far as I can see – the main aspect of induction for Peirce does not consist in inferring the (ultimate) truth of the tested theory, but in forming habits or warranted expectations about the future (see Müller, 1999, 77-80; Hookway 1997, 152; see also section 5). Peirce also discusses recursive modifications of theories which constitutes a succession of loops leading back to generating improved hypotheses (cf. the quotation above).[9] On top of this, (qualitative) induction may result in a theory's refutation as

certain facts one has come to habitually subsume to a given theory prove to be in conflict with it or with other parts of one's background knowledge. Thus induction also brings about the surprising facts that function as input (premises) and as the driving force for novel abductions to some alternative and more comprehensive theory.

I think the particular strength of Peirce's concept of (qualitative) induction is that it is *not* meant to yield certainties, but rather constitutes feed-back and feed-forward links (back to the tested cases and forward to relevant future situations), which – together with abduction and deduction - constitutes a dynamical method for the acquisition of (scientific) knowledge as well as its further application and evolution (cf. also Pape 1999, 258-262). Figure 1 illustrates the basic cycle underlying all these processes, where "t_0" indicates the original cycle (with t_0 being the initial problematic situation and t_0', t_0'', … being the test cases) and t_1 refers to a future situation in which the relevant theory is activated to account for it (which, in case it works well, strengthens this local belief-system or else gives rise to further reflections).

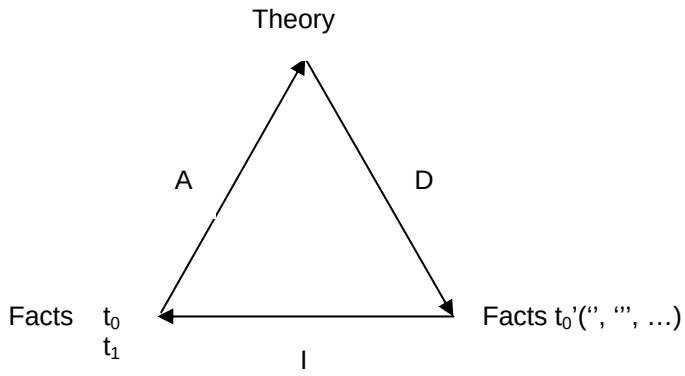

Fig. 1: Induction (IBE) in the context of knowledge application and modification

It should be noted that Peirce clearly does *not* intend induction to infer back *directly* to the theory in question, i.e. to its (approximate) truth; rather are the hypothesised qualities projected back onto their examined instances and forward onto those instances to be as yet encountered (this, to my mind, is also what essentially constitutes the famous "self-correcting" property of reason in general and of induction in particular; CP 5.579; cf. also 5.580-581). Even in his later writings there can be no doubt "that induction is reasoning from a sample taken at random to the whole lot sampled" (CP 1.93, 1896), not to the theory that accommodates this lot. And still more to the point:

> Induction … is not justified by any relation between the facts stated in the premises and the fact stated in the conclusion; and it *does not infer that the latter fact is either necessary or objectively probable.* But the justification of its conclusion is that that conclusion is reached by a method which, steadily persisted in, must lead to true knowledge in the long run *of cases of its application*, whether to the existing world or to any imaginable world whatsoever (CP 7.207, 1901, emphases mine, cf. also CP 6.610).

As Peirce distinguishes different kinds of induction a general remark may be in order, so that yet another aspect of "qualitative induction" can be revealed. On Peirce's account as well as following modern analyses of the concept of induction the main characteristic of this type of inference is not the generalisation from particulars to generalisations, but rather the projection of characteristics from one entity to another (as inference from particulars to particulars is logically equivalent to inference from particulars to generalisations; see Hintikka, 1992, 25; Hintikka & Niiniluoto, 1980).

In the special case of qualitative induction what is projected are the features as implied by the theory. And as already mentioned above, this is equivalent to the former concept of "hypothesis", and not only that – it is also equivalent to what is commonly taken as IBE (for this matter see also Rappaport's [1996] critique of IBE). We infer that, given the collected evidence, the testing situation(s) – as well as the original one – that we have so far treated *as if* a certain theory applied to them *actually are* cases of that theory.

Abduction, on the other hand, covers the process of inventing a theory in the first place, and it should therefore not be confused with IBE. They may nevertheless appear to be similar in the context of ordinary knowledge application, when a habit has already been formed. Suppose someone is found to have lung cancer. The physician might immediately conjecture that the patient is a smoker. Is this not IBE (or Peirce's former "hypothesis")? No, it is not. One reason is that it is only a conjecture, and the patient might just as well have incurred the cancer from an overexposure to asbestos. Abduction has to set forth all these possible hypotheses, induction evaluates them. This leads on to a second reason: Qualitative induction (or IBE or "hypothesis") *presupposes* the theory or rule (see the syllogism above). In other words: The theory has to be (consciously) in mind *before* induction can start doing its job, even in unproblematic cases. (Of course, it has been argued that the abductive inference, too, already presupposes the hypothesis. But I hope to be able to eliminate this possible objection in the following section).

4. ARE ABDUCTION AND INDUCTION LOGICAL INFERENCES?

There have been qualms especially about abduction being a logical inference (see Kapitan 1992 and the literature he quotes; see also Hoffmann 2000). Kapitan argues that the inferential aspect of abductive discovery could be accounted for in terms of deduction and induction, while its specific heuristic function involved no inference. In a rather similar argument, van Fraassen has held that IBE, i.e. Peirce's induction, constituted "no inference at all" (1989, 161). Against these objections I am trying to defend Peirce's claim that all three types of reasoning are indeed inferential.

Let us start with abduction. Critics draw to the account Peirce delivered in his *Lectures on Pragmatism*. This is the inferential form he describes:

> (1) The surprising fact, *C*, is observed;
> (2) If *H* were true, *C* would be a matter of course.
> Hence,
> (3) There is reason to suspect that *H* is true.
> (CP 5.189, 1903)

Kapitan points to the fact that the hypothesis already appears in the premises and concludes that therefore it could not possibly be inferred (1992, 6-7). As for the hypothesis itself he claims it to be a the result of induction (7-11). However, neither of these statements, to my mind, appears to be tenable, because, as I shall argue, the above syllogism is only one part of the overall abductive process and that *H* is not attainable by induction (see also Hoffmann 1999). In order to show this, we have to analyse what constitutes a logical inference.

First of all, "logic" in the present context must, of course and by definition, extend the narrow bounds of deductive logic so that logical inferences are to be regarded as transitions from a set of premises to a conclusion that are warranted in some way (cf. also da Costa and French 1989, 341). The aspect of warrant seems crucial, for inferences, if anything, are not to be arbitrary. This is also stressed by Peirce who defines inference as a "*controlled* adoption of a belief as a consequence of other knowledge" (CP 2.442, emphasis mine). However, accord-

ing to Peirce, there are three steps altogether that are essential to any inference, namely *colligation*, *observation*, and *judgement* (MS 595, 35; CP 2.444; see also Kapitan 1992, 4).

The relevant premises are first gathered and asserted as a single "*conjunctive proposition*" (CP 2.442, cf. also 5.579, MS 595, 30)[10]. The colligated premises are then contemplated and operationally manipulated to produce a new proposition (CP 2.443). This is the phase of "observation", and Peirce says "it appears that all knowledge comes to us by observation" (CP 2.444). Thus observation leads to a creative perception, but one which is forced upon us and as yet uncontrolled (cf. CP 5.181; 7.330-331). Therefore, what has been produced in by observation has to be evaluated in some way in order to count as an inference.

Now what is valid judgement? Peirce's key concept here is mental experimentation upon the icons (i.e. the colligated premises and the conclusion) in order to

> satisfy the mind that the one icon would at all times involve the other, that is, suggest it in a special way ... Hence the mind is not only led from believing the premiss to judge the conclusion true[11], but it further attaches to this judgment another – that *every* proposition *like* the premiss, that is having an icon like it, *would* involve, and compel acceptance of, a proposition related to it as the conclusion then drawn is related to that premiss (CP 2.444).

How are we to understand this requirement? Its main point seems to be that what has to be shown to follow some rule or rationale (i.e. not to be due to some irrelevant association that might have caused the actual result of observation). Judgement thus is a *conscious feedback operation* from the conclusion to the premises which – in the positive case – establishes that the premises really warrant the conclusion in the way peculiar to the special type of inference.

So what, to start with, is the rationale behind abduction? According to Peirce "the hypothesis cannot be admitted, even as a hypothesis, unless it be supposed that it would account for the facts or some of them" (CP 5.189). Hence, a validly abduced idea or theory must explain the facts, i.e. the surprising facts that motivated the abduction must eventually come out as no surprise. To be sure, it may still not be true (as such a judgement requires further deductive and inductive examination); it is only stated that the suggested explanatory hypothesis can account for the facts in the premise. By the way, this also marks the essential difference between accommodation and prediction in Maher's well known analysis of their respective confirmatory values (1988).[12]

Looking back on the definition of abduction quoted above it appears that this only covers the final justificatory step of the overall abductive process and omits the steps of colligation and observation (see also Hoffmann 1999, pp. 278-280). This interpretation is also supported by Peirce's discussion of the role of perception in the overall context of abduction (cf. esp. CP 5.190-194).[13] Under any other interpretation this would be a surprise, since perception is generally regarded as a form of – not as an opposite to – abduction (cf. the quotation at p. 11) and since abduction is, after all, supposed to generate novel concepts and would thus have to involve perception of some kind. Perceptual judgements in Peirce's view "are to be regarded as an extreme case of abductive inferences, from which they differ in being absolutely beyond criticism" (CP 5.181). That is to say, conscious judgement in inferential form is superfluous for mere perception as one immediately accepts what one observes. I therefore conclude that in the context of the abductive inference "perception" denotes the result of the observational process and that in the passage in question Peirce drew not to the overall abductive process, but only to the final abductive step, the abductive judgement as opposed to colligation and observation.

What about the other two types of inference? With respect to deduction the premises are colligated in order to see – by way of observation – whether the abduced theory is consistent with background knowledge and determine what we should expect on its account, i.e. to explore theorems and hypotheses the theory entails.[14] The final judgement, then, has to estab-

lish the conclusion's deductive validity, i.e. to show that it contains no error and thus follows necessarily from the premises (as is done by an explicit deductive calculus). It is important to note that while abduction only relates the suggested theory to the explanation seeking facts, deduction relates it to other relevant background knowledge.

However, deductive truth hinges upon the (still hypothetical) truth of the premises and can therefore not establish factual truth in any way, "it does not lead to any positive knowledge at all, but only traces out the ideal consequences of hypotheses" (CP 7.207, 1901). Thus we are in need of empirical testing to see whether the theory's assertions really hold. Moreover, as we are, especially today, well aware of the shortcomings of hypothetico-deductivism, it is clear that we have to look out for some non-deductive way of confirmation. The hypothetico-deductive method (H-D) allows for only a very weak form of confirmation, i.e. confirmation by eliminating possible falsifiers of a hypothesis *H*. Thus, Fa hypothetico-deductively confirms $\forall x(Fx)$, but not Fb, since Fa is not deductively entailed by Fb (see Gemes 1998, pp. 11-12). This form of confirmation is weak not only because it does not (by itself) allow projection to the untested parts of *H*, but also because there might be competing hypotheses that equally accommodate the available evidence. In a fairly recent plea for eliminative induction, Earman has pointed out that – in a way – Popper had to be turned upside down:

> Popper's account of scientific methodology emphasizes the corroboration of a hypothesis as arising from unsuccessful attempts at falsification of the hypothesis, but for the eliminativist it is successful attempts at falsifying competing hypotheses that count, and the success of inductivism piggybacks on this eliminative success (1992, p.163).

So this is the point where induction – also in Peirce's sense – comes in, and I shall discuss this view of induction in more detail in the following section. For the remainder of this section I only want to concentrate on its specific inferential steps. The premises colligated in induction are those deduced consequences of *H* that may count as evidence for or against *H*. When observed (both actually or in the sense of recollection from memory) part or the whole of the evidence must act as a decisive indicator in order to sanction the inference to the acceptance of *H* or to one particular hypothesis from a set of competing ones (which makes no substantial difference since $H_1 \& \neg H_2$ is logically equivalent to $H \& \neg(\neg H)$). While observation leads to an immediate belief (or disbelief) in a hypothesis, judgement entails its acceptance on rational grounds. These grounds involve an evaluation as to the coherence of *H* and/or the incoherence of possible alternatives with premises and background knowledge. This understanding of induction incorporates holism and a certain kind of coherentism without indulging in Kuhnian relativism (cf. also Suppe 1989, chap. 10). The crucial point is that decisive indicators are no criterion for "absolute knowledge", but for knowledge on the basis of our current background knowledge (i.e. the *present* and *contingent* conviction that things not only are consistent with *H* but have to be the way *H* asserts them to be). Another aspect is that although each bit of evidence might be challenged – and will perhaps be challenged in the future – at the moment when it functions as evidence it must be certain for us (see the following section). Thus, we would have a stage-like process in which *either* the evidence itself begs the question *or* the tested theory, but not both at the same time. This puts us in a position to ward off crude relativism (in fact, further below I shall argue that this type of coherentism is compatible with and incorporates a certain kind of foundationalism).

Of course, there might always be more powerful hypotheses to be discovered in the future. Therefore, one might argue that the certainty or acceptability of induction (or IBE) hinges on what hypotheses are currently at stake – and van Fraassen has warned us that our best explanation may only be "the best out of a bad lot" (1989, p. 143). So long as a certain theory is truly convincing and the evidence is clear, however, this claim would be overly absolutistic, since it requires us to take a God-like position from which one sees what is abso-

lutely true and consequently whether the suggested hypothesis is really the best out of all possible ones or just the best out of a bad lot.[15] I consider van Fraassen's argument invalid, because it runs afoul in just the same way as does the argument of approximate truth in that it presupposes a perspective that is not to be had. In other words, van Fraassen's argument is only valid in the context of "metaphysical realism" and therefore off the point in the present frame of reference.

5. CIRCULARITY AND TRUTH

In the preceding section I have argued against van Fraassen's rejection of IBE, although I accepted his reservations against the idea of approximate truth. I argued that qualitative induction yields a "truth" judgement which is both stricter *and* less strict than approximate truth – stricter, because it requires us to eliminate *all* relevant doubts and accommodate *all* relevant facts at hand (instead of being content with a reasonably good approximation), less strict in placing the whole justificatory burden on our background knowledge and suitable empirical tests generated from it (instead of evaluating theories against purported absolute truths about an ultimate reality behind the perceived phenomena).

However, binding the question of knowledge (or truth) to other (background) knowledge is circular in a certain way. But as I am going to argue, this circularity is not vicious for two reasons. Firstly, there seems to be a hierarchical order of knowledge, to the effect that knowledge is not justified by other knowledge (as classic coherentism would have it), but that higher forms of knowledge are built on already established lower forms (see also the following sections). Secondly, each of the three inferences (especially their judgement bits) involve more than only a feedback loop to the premises. There is also a feed-forward loop leading from abduction to deduction, from deduction to induction, and from induction on to abduction (the latter in terms of perceiving future objects or situations in the light of the newly established concept or theory).

Let us be a little more precise about the dynamical linkage between the three inferences: The abduced theory feeds back to the surprising facts (which it accommodates) and forward to the deductive exploration of its consequences beyond the limited context from which it was inferred. The deduced consequences feed back to the premises (telling us what the premises necessarily entail) and forward to some sort of real-life testing to see if the deduced consequences really obtain. Induction, finally, feeds back to (empirical) hypotheses and evidence at hand (in order to accept or reject the theoretical explanation) and forward to relevant future situation in which the theory ought to apply. So long as our theories work well, they are permanently confirmed (or corroborated) in daily practice, so to speak. But when we come up against a snag, when we realise that there are facts that the theory fails to accommodate, we have "surprising facts" again and the abductive search process will start afresh (at least if we cannot plausibly put the blame on side conditions).

As already emphasised above, knowledge in this sense is dynamical at heart and not static as we still commonly take it to be. Any piece of knowledge that is activated and applied has to provide some guidance for our lives as we continually react according to how we perceive what we encounter.[16] Peirce unmistakably has expressed this very idea in claiming that "all conclusions of reasoning partake of the general nature of expectations in the future" (CP 2.145, 1902) and stressing the formation of a habit as the goal of inquiry in general and truth evaluating processes in particular (cf. CP 2.148). (Note that it is not just the conclusions of reasoning, but also the simplest perceptual judgement that shares this property (cf. CP 5.57, 1903). Elsewhere he argues that such an activation of previously acquired knowledge is to be regarded as an *abduction* (no matter how habitually it is triggered off):

> (W)hatever feature of the percept is brought into relief by some association and thus attains a logical position like that of the observational premises of an explaining Abduction, the attribution of Existence to it in the Perceptual Judgment is virtually and in an extended sense, a logical Abductive Inference nearly approximating to necessary inference (CP 4.541, 1906).[17]

The concept of truth that goes with our – and Peirce's – view might be called "pragmatic truth" (Da Costa & French 1989). According to it a theory is true only on account of background knowledge and relevant evidence at hand, but not in any absolute sense of the term. True theories have to accommodate all our evidence and, as a further requirement, this evidence also has to allow us to reject possible rival theories. If these can be successfully eliminated, we believe in the truth of a theory, although "scientific spirit requires a man to be at all times ready to dump his whole cartload of beliefs, the moment experience is against them" (CP 1.55).

Of course, this has been commonplace, in a way, since Popper's falsificationism. But it is not the same. Earman, in his plea for eliminative induction (which, to my mind, is tantamount to Peirce's qualitative induction), has made the point (quoted above, p. 9) that eliminative induction turns Popper upside down. And there is still more to it. While Popper only speaks of corroboration, we would suggest to speak of confirmation, and hence of truth. While he thinks theories are more or less taken out of the blue, we would hold that they are *inferred* from prior knowledge. And while Popper (together with many realists and anti-realists) categorically differentiates between reality (as it really is) and our human conceptions of it, we claim that cognitive states result from the interaction of an agent (as a natural part of the world) with his or her environment, and thus, *relative to these environmental conditions*, our conceptions may be legitimately, called "true" if the three main requirements (accommodation [via abduction], prediction [via deduction], and elimination [via induction]) are met.[18]

The importance of these distinctions has recently been brought to the fore by Hilary Putnam in his latest stance in the realism debate that he prefers to call "natural realism", i.e. a realism which holds "the objects of (normal 'veridical') perception are 'external' things, and, more generally, aspects of 'external' reality" (1999, 10). The thrust of the overall argument is twofold: On one hand, Putnam rejects the long held idea of an "interface" between the external world and our cognitive powers (in the form of sense data as input of cognitive processes) and thus accepts the notion that we are in unmediated contact with our environment (ibid., 10, 44). On the other hand, he thinks it erroneous to conceive of reality as one "single superthing" (the ultimate entity that would be the "really real" in the sense of metaphysical realism) and suggests that we rather look "at the ways in which we endlessly renegotiate – and are *forced* to renegotiate – our notion of reality as our language and our life develop" (ibid., 9).

Is this all just boiling the problem of realism down to a human measure? No, it is not just that. Rather is the whole question of how we conceive of the reality we experience and the truth of the statements we make about it completely turned around. Truth is not relative anymore to an unattainable ideal (and thus necessarily approximate), but it is now taken as relative to how *we* perceive the world, i.e. we evaluate our claims against what we already have (as evidence) rather than against a metaphysical concept of reality. This is the sense in which we can attain a meaningful notion of truth – albeit still, of course, a regulative one –, and one that appears to be acceptable even for van Fraassen (cf. 1997)[19].

As far as Peirce is concerned, he at least in his later writings does not talk of an approximation to an ideal truth, but only says that "(i)n induction it (i.e. science) simply surrenders itself to the force of facts" (CP 5.589**,** 1898). It may well be that Putnam has suspected Peirce of an absolute view of reality towards which scientists would ideally converge (1992, 84)**,** but Hookway has convincingly argued that this applies – if at all – only to the Peirce of up to 1880 (cf. Hookway 2002). Hookway discusses at length Peirce's notion of truth as convergence and how his conceptions of truth and reality have developed later on. We will leave

it aside here, since the main point at issue seems to lie in epistemic justification (of which convergence of opinions would only be an indicator).[20]

"Truth" in the sense I would suggest means that it relates to a theory *T* iff two conditions are met: (i) The inferential triad leads us to the qualitative induction that *T* is (practically) true, and (ii) the premises employed in the argument are all true, so that our final judgement be not based on false beliefs. This viewpoint is highly problematic with respect to the so-called Gettier problem, but interestingly enough, there have recently been proposals to block the Gettier paradoxes just by way of denying that the justification requirement for knowledge could be met with respect to a false belief. One example is Suppe's[21] non-reliabilist externalism (1997), another is Maher's account of objective confirmation (1996). Both approaches exploit the (previously almost unthinkable) idea that things can be known directly from experience, at a level of certainty that is not "absolute" (i.e. has a probability of 1), but which is still beyond degrees. Maher calls this "practical certainty". He says it is not a matter of calculating the probability of *p* to determine the rationality of doing as if *p* were absolutely true. For practically certain propositions are certain to such an extent that such a calculation is clearly unnecessary, indeed irrational (ibid., 159 f.).[22]

Peirce makes a similar point with respect to his "indubitables". On one hand they are indubitable, on the other they are still necessarily vague (cf. also Johanson 1994, who compares Peirce and Wittgenstein on this issue):

> (W)hile it is possible that propositions that are really indubitable, for the time being, should nevertheless be false, yet in so far as we do not doubt a proposition we cannot but regard it as perfectly true and perfectly certain; that while holding certain propositions to be each individually perfectly certain, we may and ought to think it likely that some one of them, if not more, is false (CP 5.498, 1905).

As already mentioned, another requirement for propositions to be evidence is that they are known "*directly* by experience" (Maher 1996, 160). Now what can that mean? Maher relates to the traditional idea that such knowledge of a proposition be not based on an inference from any other proposition. However, a lot of the evidence that scientists actually use clearly depends on such inferences – like e.g. the advance of the perihelion of Mercury (*A*) that Maher discusses in the context of Einstein's General Theory of Relativity (*G*). If *A* was originally inferred from observations by astronomers, how can it be treated as evidence, especially if being evidence entails being known directly by experience? Maher bites this bullet by giving the whole issue a remarkable turn:

> These superficially conflicting tendencies all find a natural place if we understand claims about directness as relative to a set *S* of propositions that are taken as relevant. A proposition *E* will then be said to be known directly by experience iff it is known by experience and that knowledge is not based on an inference from any proposition in *S*. Usually, in discussions of *G*, the particular observations from which *A* was inferred are not being considered, hence *A* counts as known directly by experience and so can count as evidence. Conversely, if the particular observations were being considered, we would call them the evidence and regard *A* as an inferred proposition, not as evidence (ibid., 161).

This entails that, on Maher's view (as on Suppe's), even highly abstract propositions can be known directly by experience and may count as evidence. With this remark let us once again turn back to Peirce. One key feature of his pragmatism – in contrast to that of James – is that he took external things to be real and directly present in experience – even such abstract entities as laws and other fundamental truths, just in the same way as concrete objects are (cf. Hookway 1997, 160-162).

What we end up with is that once the truth of a proposition is established we can build on it by using it as evidence in another context and thus take it as real (or we could search for

an explanation of this newly-gained aspect of reality at a higher level). This view, to my mind, essentially matches with how Devitt conceptualises truth, reality and their relationship (cf. 1997, chap. 15). Devitt suggests a form of correspondence truth[23] that overcomes the well known problems of its classical version. The "trick" to do this, however, really seems to have been discovered by Peirce in his theory of language (and perhaps independently by Wittgenstein). Instead of devising "correspondence rules" between *signs* and their *objects* he has conceived of semantics as essentially a triadic relationship, involving also an *interpretant* to mediate between *sign* and *object* (cf. e.g. CP 2.274; cf. also Hookway 2000, 106). Unfortunately I can but hint at this point here. Nevertheless I would like to add two remarks: Firstly, this again refers us to a dynamical notion of understanding theoretical concepts or language in general (as depending on their usage rather than their explicit definition), and what's more, analytical philosophers by and large seem to converge in this respect (note e.g. van Fraassen's marked Wittgensteinian tone in his 1997 discussion of Putnam's model theoretic argument)[24]. Secondly, such an approach seems to yield just about what Devitt has demanded, namely to naturalise semantics, just as before we naturalised epistemology" (1997, 296), the consequence of which is "that Realism is settled first, epistemology and semantics afterwards" (ibid.).

6. ABDUCTIVE EXPLANATION AND ABSTRACTION

Towards the end of the preceding section I alluded to the idea of stages in the construction of knowledge. This raises the question of how we abstract higher order concepts from lower order ones and what sorts of stages are to be distinguished. Although Peirce has not developed a "theory" of abstraction, his concept of abduction entails important consequences in this respect. First of all, as abduction leads to novel explanatory knowledge, one has to ask, what makes knowledge explanatory and whether or not all new knowledge, which is of a higher level of abstraction, is necessarily explanatory in some sense**.** In the context of Peirce's later epistemology, abduction does not apply a rule or theory to infer what must have been the case (as is suggested by Hempel's deductive-nomological (DN) account of explanation), but generates such a theory. What's more, there is an important difference between a rule – or a law – on one hand and a theory on the other. McMullin, e.g., has emphasised that laws did not explain anything but rather raise the question of why they hold if they do. "Laws are the explanada; they are the questions, not the answers" (1992, 90). And he continues:

> To explain a law, one does not simply have recourse to a higher law from which the original law can be deduced. One calls instead upon a *theory*, using this term in a specific and restricted sense. Taking the observed regularity as effect, one seeks by abduction a causal hypothesis which will explain the regularity. To explain why a particular sort of thing acts in a particular way, one postulates an underlying structure of entities, processes, relationships, which would account for such a regularity. What is ampliative about this, what enables one to speak of this as a strong form of understanding, is that if successful, it opens up a domain that was previously unknown, or less known (ibid., 91).

McMullin also reminds us that Hempel not only promoted the DN model but also discussed another type of explanation that he called "theoretical explanation" and that in general complied with his own approach (McMullin 1984, 205 f.). Others, too, have taken the same line as McMullin (e.g. Suppe 1989, chap. 6). Therefore it could well be that abduction – as the inference that leads to the wanted theoretical explanations – implies moves from one stage to another in the way the psychologist Jean Piaget has suggested in his cognitive developmental and epistemological theory (cf. esp. his later writings: 1970; 1985, and Piaget & Garcia 1989). According to Piaget's theory lower-stage "facts" are understood and explained from the per-

spective of a higher-stage "theory" (or "assimilated" by it). This cognitive developmental approach, to my mind, is of great interest in the overall context of our discussion, since a given theory can itself be turned into a fact when one reflects on it from a yet more abstract perspective at a higher level, so that "abstract" and "concrete" – as well as "fact" and "theory" – would become relative concepts. This not only meets with our above analysis of "evidence", "realism", and "truth", but also with Peirce's ideas on the relativity of abstractness and concreteness (see Zeman, 1983, 298-299).

But with this note on the relation between Piaget and Peirce let us now see first to Peirce's own reflections on the subject and consider an example that he draws from Mollière's "*Le Malade Imaginaire*" (see CP 5.534; quotes from Mollière taken from Zeman, 1983). In one scene of this comedy a candidate for a medical degree is solemnly asked why opium puts people to sleep. The candidate most sageously replies that opium puts people to sleep "because there is in it a dormitive power whose nature it is to lull the senses to sleep", which in turn is hailed by the board of examiners: "You have responded well indeed, you are most worthy to join our learned brotherhood!"

Of course, "having dormitive power" appears as but a turgid reformulation of "putting people to sleep" (and even today scientists are frequently reproached of merely putting common-sense ideas in pompous words). Peirce, however, objects to this straightforward interpretation and holds that there is an important difference between the two statements, for the second one were more abstract than the first. And with reference to the concept of abstraction he specifies:

> There are two entirely different things that are often confused from no cause that I can see except that the words *abstract* and *abstraction* are applied to both. One is *αφαιρεσις* leaving something out of account in order to attend to something else. That is *precisive* abstraction. The other consists in making a subject out of a predicate. Instead of saying, Opium puts people to sleep, you say it has dormitive virtue. This is an important proceeding in mathematics. For example, take all "symbolic" methods, in which operations are operated upon. This may be called *subjectal abstraction* (NEM III/2, 917, 1904).

Elsewhere he adds that by subjectal abstraction "a transitive element of thought is made substantive, as in the grammatical change of an adjective into an abstract noun" (CP 2.364, 1901)[25]. The transitive element in the case of opium is the *process* of putting people to sleep, which links the two events of taking it in and actually falling asleep. The phrase "Opium puts people to sleep" has this link included, but how it functions, i.e. how opium acts upon the body and produces the dormitive effect, remains in the dark. What's more is that this question does not even arise, because opium is viewed as the cause of falling asleep which seems to explain everything there is to it. The idea of a dormitive power however, sets the stage for an abstract cause, or a reason for opium being a cause of falling asleep. Opium is now seen as something containing a substance that influences our bodily functions in a specific way. Of course, invoking a dormitive power does not explain anything, but it *raises the question* what it essentially consists in, and this indicates a new and indeed more abstract perspective in comparison with the first statement.

In other words, Peirce's principle of subjectal abstraction describes a cognitive operation that leads to higher order explanations of certain facts (whereby those facts can be of any level of abstraction, so that e.g. the reference to opium consumption may explain why a particular person has fallen asleep, whilst the drug's dormitive power may be put down to the chemical properties of morphine (the crucial ingredient of opium).

Thus, (subjectal) abstraction may be regarded as an important feature of the overall process of abduction as a whole, as we have already stated above that abduction is basically about the construction of "theories" to account for certain matters of fact. This allows us to look at Peirce's account of abduction from the perspective of Piagetian equilibration theory

(cf. 1985; Piaget & Garcia 1989), which could enlighten both approaches. And as far as I can see, they are not incompatible in this respect.

As far as Peirce is concerned he can hardly explain how humans manage to actually conceive promising hypotheses (see above, section 3) – and in the light of Piagetian stage theory the problem is twofold: first, how to get from concrete concepts to abstract ones, and second, how to conceive that which really fits the case in question. Piaget's theory has a lot in stock as far as these questions are concerned (especially in connection with a reconstruction in terms of modern thermodynamics and mathematical information theory; see Minnameier 2000b).

There may, of course, be caveats as to the question of reifyng abstract objects. Without going into much detail I would like only to hint at a few points in this respect: Firstly, a stage-like ordering of more and more abstract (or complex) conceptions implies no rough cut distinction between abstract entities on one hand and concrete ones on the other, but leads to the idea that abstractness and concreteness are relative rather than absolute categories. It all depends, to my mind, on inter-conceptual referential conditions. By this I mean that whenever facts are explained by some theory, the theory is abstract relative to the more concrete facts, although those facts may themselves be rather abstract as in the example of the advance of the perihelion of Mercury. Thus, all knowledge would have to be regarded as more or less abstract (or concrete), which would also entail denial of the classical dichotomy between the metaphysical (as abstract) and the physical (as concrete). As a consequence, our knowledge would come out as basically of one kind, whilst important distinctions within that body of knowledge would still be preserved, if not pinpointed, so that the whole issue of how human knowledge is organised is neither flattened out (as in the concept of a non-hierarchical network) nor blurred (as it is on the simple argument of theory-ladenness).

Secondly, we are intuitively inclined to rely more on a good theory – and possibly on the abstract entities it posits – than on our immediate sensual impressions (as in the elementary case of optical illusions). Given that there are no absolutely basic empirical insights, why should we not be as confident in reifying (relatively) abstract entities as we are in reifying (relatively) concrete ones – especially since on the account set forth in this paper abstract concepts emerge from abductive examination of facts of some sort and thus always rest on a factual basis (a basis that begs the question itself whenever we feel a need of explanation).

In this respect it should, finally, also be noted that there have been serious attempts recently to give universals a fresh face (see e.g. Bealer 1993; Tennant 1997). I think the important point is that modern universalists are no naïve realists but simply adhere to a hierarchical (or unificationist) form of coherentism (as is already apparent from the views on theoretical explanation mentioned above). It is a form of realism that only assumes that our knowledge reflects some aspects of the world (though not in a one-to-one mapping relation) and that the more abstract concepts evolve from contradictory – or otherwise explanation seeking – statements on lower levels. This takes us to our last aspect of what Peirce's inferential theory together with a Piagetian stage-theoretical interpretation could offer with respect to the current discussion on coherence and unification.

7. PEIRCEAN PERSPECTIVES ON COHERENCE AND UNIFICATION

In this concluding section I would like to relate my analysis of abduction, deduction, and induction to the current discussion of coherence and unification. These two concepts are often taken as synonyms (see e.g. Schurz 1999), although an important difference between the two is apparent that ought to be highlighted. Whereas coherence applies to the overall organisation of knowledge, unification is discussed as a form of explanation. In what follows I would like to show two things: first, that unification is largely equivalent with abduction (only

that perhaps one would wish to restrict unification to higher level theoretical explanations), and second that coherence applies to the whole of the interaction of the three types of inferences. Thus coherence would come out as a wide concept that includes the narrower notion of unification. For reasons of brevity I will restrict my analysis to two recent approaches, namely by Bartelborth (1996; 1999) and Schurz (1999) The main idea that both authors support is that explanation basically consists in understanding those facts that previously – according to the initial cognitive state of a given subject – was in need of explanation. Now this demands further explication. Let us first examine what kinds of "explanations" fail to meet with such an approach and, for that matter, start with Schurz's account of unification.

Schurz considers two paradigms for (non-purposive) why-explanations: the *nomic expectability* approach (as incorporated in the DN model of explanation) and the *causality* approach, which aims at a list of all causes or causally relevant factors for the explanation-seeking fact. He explains why both are inadequate and gives the following instructive example:

> For instance, "Peter is flying past the window in the third floor, because one second ago he was flying past the window in the fifth floor" is not only a predictively but even a causally adequate argument. Still it is not adequate as an explanation, because the cause is here just as much in need of explanation as the effect. (Schurz 1999, 97)

Being in need of explanation is the crucial concept in this context. This need results from the incoherence of the explanation-seeking fact *P* with background knowledge *C*. With the explanatory answer *A*, *C* is augmented to the effect that *P* can be assimilated to *C+A*. Furthermore, by assimilation Schurz means that P can be inferred from suitable premises in *C+A* (see ibid., 98). This inferential concept of the explanatory relation is clearly equivalent to Peirce's justificatory argument that completes the abductive process (see section 4, above).[26]

With the enhanced cognitive state *C+A*, (previously decohering) *C* and *P* are integrated, or unified. This emphasis on removal of incoherence and integration is the crucial difference in relation to earlier accounts of unification, especially Kitcher's (e.g. 1981; 1989). Kitcher's aim was to find "argument patterns" that could be applied to a wide range of domains as generalised patterns of explanation, and he followed Hempel in his view of explanations as deductive application of those generalised patterns. Thus, the stress is on *deduction* and *induction* (the latter in the form of – possibly unwarranted[27] – generalisation), rather than on *abduction*. Kitcher's reliance on a merely syntactic analysis of abstract patterns and their similarities (Kitcher 1989, 479-480) also reveals this, since a truly abductive approach attempts to establish semantic relations by way of assimilation, i.e. an abductive explanation has to say something about the explained phenomena, not just "model" them in a non-referential sense (see Schurz, 1999, 98-100; Bartelborth 1999, 219). Whereas Kitcher intends to provide a common basis for as many diverse phenomena as possible, abductive approaches work the other way round by starting from local incoherencies which are then tried to reintegrate in a more complex structure.[28]

With this explication of unification we can now turn to the broader concept of coherence. Despite being widely used and discussed, the notion of coherence has so far remained rather fuzzy (see e.g.; Bartelborth 1996, 169-192; 1999, 209-213). One interesting "theory of coherence" has recently been suggested by Bartelborth (1996, 192-199; 1999, 218-222), the main aspects of which seem to be what he calls *systematic* and *relational* coherence.[29] Let us first consider the latter.

Relational coherence addresses the question of how well a belief *p* fits into a belief-system *X*. This is explicated by two conditions, which are called "abduction" and "embedding". According to this, *p* coheres the more with *X*,

(a) [*abduction*]

(i) the more propositions from *X* can be explained or deduced by *p*
(ii) the better the explanations are,

(b) [*embedding*]

(i) the more often p can be derived or explained from *X*
(ii) the better the explanations of *p* are
(Bartelborth 1999, 221; numbering error corrected, G.M.)

In (a) p is the explanans, in (b) it is the explanandum. Thus "embedding" covers what I have called abduction (see our above analysis of "unification"), whilst what Bartelborth calls "abduction" takes on the meaning of IBE and would thus have to be understood as (qualitative) induction in the sense of Peirce (Bartelborth is explicit about his identifying abduction with IBE, c.f. esp. 1996, 138-148). Thus we can conclude that Bartelborth's notion of relational coherence comprises both (Peirce's) abduction and induction. Deduction is also included, so long as induction should be qualitative. However, as the formulation "explained or deduced" suggests, there might also be non-deductive ways of IBE. In this respect one can think of simple enumerative induction that is not based on an explanatory account in the unificationist sense but only on perceived – and possibly contingent – regularities.

Let us now move on to *systematic* coherence. In contrast to relational coherence, which applies to *local* explanatory relations, systematic coherence describes the *global* status of an individual's belief-system, i.e. whether it is highly integrated or divided up into unrelated or only loosely connected subsystems. A high degree of systematic coherence means, according to Bartelborth, that there is a great number of links between individual beliefs or subsystems of a belief-system, (1999, 218). However, he does not specify whether this type of coherence merely depends on the sheer (average) number of links or their distribution. What's more, he does not say anything about the types of relations that matter. This is quite surprising, since he also stresses the importance of theories as "the essential building blocks of any system of justified beliefs. (…) They hold together the otherwise isolated observational beliefs to form a larger model of the world" (1999, 213). And he takes on the same realistic attitude towards theories as Schurz who says that "unification power just is the major criterion for the realistic interpretability of high level theories" (Schurz 1999). Moreover, non-hierarchical forms of coherentism appear to be almost doomed, following e.g. Plantinga's (1993, 97-113) or Bartelborth's own analysis (1993).[30] So what seems to be important is an analysis of how the higher levels build on the lower ones – and first of all what these levels of knowledge consist in. I think it is a major challenge for the involved sciences to reconstruct the hierarchical structure of human cognition beyond the simple and untenable dichotomy of observational and theoretical knowledge.

Thus, with respect to systematic coherence, the complex (in terms of hierarchical integration or unification) should not be confused with the complicated (in terms of mere links). Interesting questions in this respect would be what level of unification is attained in a given belief-system and whether all domains represented in that system are assimilated to this level or rather represent isolated realms in the cognitive architecture. Furthermore, this differentiation also allows us to accommodate very neatly the role of ad hoc hypotheses (that Bartelborth also discusses; see 1999, 219): On one hand, they complicate the belief system (since they just add to the already existing hypotheses), but on the other hand they prevent it from decohering. An integrative, unifying account (i.e. a transition to a higher-order model), however, not only maintains, but enhances the overall coherence while at the same time it reduces the amount of complications in the system (see Minnameier 2000a, 168-175).

This idea of hierarchical organisation would open up yet another important perspective that has already been grasped by Peirce to some extent. It is the question about ways of exploring larger parts of an organised belief-system in order to yield new knowledge and solve specific problems. So far we have only discussed the elementary triad of abduction, deduc-

tion, and induction. But how do we actually direct the abductive searching process towards the promising regions of the overall possibility space that our background knowledge spreads out, in principle. How do we creatively use the knowledge we already have? Sometimes important insights may be ingeniously deduced from previously acquired basic principles or we may come up with brilliant ideas by way of analogy. Here, Peirce's reflections on analogical reasoning and what he calls "theorematic deduction" seem to be highly relevant.

8. IN LIEU OF A CONCLUSION

To end with let me note that the similarity of my paper's title with one of Quine's late works (1990) is not a contingent one. On one hand, much of present day analytical philosophy and much of what I have written in this article actually rests on Quinean insights. Hence, the *Pursuit of Truth* simply goes on. On the other hand, however, one major problem has never been solved by him – a problem that has troubled Quine especially in his last creative period. This is the question of coherentism versus foundationalism and the related problem of internalism versus externalism that is perhaps best documented in his controversy with Donald Davidson (cf. Koppelberg 1998). Quine has always been a coherentist by his holism as well as foundationalist by his empiricism (especially with the behaviourist causal twist he has given it). He has also been an internalist with respect to semantic content and an externalist when it came to epistemic justification. (ibid., 275; see also Koppelberg 1999).

Is there a way out of this predicament? Consider the following statement from Quine (1966, 251-252):

> Having noted that man has no evidence for the existence of bodies beyond the fact that their assumption helps him organize experience, we should have done well, instead of disclaiming evidence for the existence of bodies, to conclude: such, then, at bottom, is what evidence is, both for ordinary bodies and for molecules. (…) We can continue to recognize … that molecules and even the gross bodies of common sense are simply posited in the course of organizing our responses to stimulation; but a moral to draw from our reconsideration of the terms 'reality' and 'evidence' is that posits are not ipso facto unreal. The benefits of the molecular doctrine which so impressed us … and the manifest benefits of the aboriginal posit of ordinary bodies, are the best evidence of reality we can ask (pending, of course, evidence of the same sort for some alternative ontology).

I think Quine was by all means well on the way towards a solution by suggesting a generative (stage-like) epistemology (which also allows for a semantic reading of his disquotational notion of truth similar to the way Putnam reads Wittgenstein; cf. Quine 1990, chap. V, compare Putnam 1999, 64-70). Put in the framework of a suitably updated Peircean approach, such a "stage-theoretical coherentism" could possibly do the job.

NOTES

[1] Hanson stood in a Wittgensteinian relativist tradition that used to be opposed by realists or objectivists. Today, we do not think of these traditions as irreconcilable camps any more, as the "pragmatistic turn" many philosophers of science have taken in recent years makes plain. At any rate, Peirce would not fit into such a simple classification.

[2] In the quoted passage Hempel also tells us that that "(s)cientific hypotheses are and theories are not *derived* from observed facts", and very similarly Popper, too, claims that "there is no such thing as a logical method of having new ideas" (1959, 32). He refers us to "creative intuition" in Bergson's sense (ibid.) and goes even further by ruling that "*no theory of knowledge should attempt to explain why we are successful in our attempts to explain things*" (1971, 189) as he believes humans simply proceed by "trial and error elimination" (ibid., 190).

[3] I have put this in quotation marks because the idea of „emergent novelty" is highly problematic, not only in the way it was introduced by the early British emergentists (see McLaughlin 1992), but also with respect to the modern discussion. As far as I am concerned I prefer to take a strictly physicalist position relating to nonequilibrium thermodynamics supplemented by quantum theoretical perspectives to account for the required intrinsic wholeness (see Minnameier 2000a).

[4] In fact the two types of infereces are fairly often lumped together or at least not kept sufficiently apart, as e.g. in Lipton's account of IBE (1991) or in Bartelborth's coherence theory of justification (1998, pp.220-222, but note his differentiation of explanatory relations on pp. 212-213).

[5] Starting out from the common identification of abduction with IBE there is a widespread ambiguity about the relation between abduction and induction. Harman, e.g., regards induction as a special case of the broader class of IBEs (cf. 1965; also Josephson & Josephson, 1994 18-26, p. 18; Josephson 2000, 31-35; Bartelborth 1996, 144-148), whereas others look at IBE as a subform of induction and consequently distinguish induction in a broad sense (including IBE) from induction in a narrow sense (exluding it; cf. i.e. Thagard 1996, p. 34, van Fraassen 1989, 132).

[6] Peirce had already noticed the resemblance between "hypothesis" and "induction" in earlier years, but the deeper meaning of this became clear to him only after he had abandoned the old frame of reference. Here are two quotes from the earlier period: "All probable inference, whether induction or hypothesis, is inference from parts to the whole. It is essentially the same, therefore, as statistical inference" (CP 5.349, 1869). "(I)nduction, hypothesis, and analogy, as far as their ampliative character goes, that is, so far as they conclude something not implied in the premises, depend upon one priciple and involve the same procedure. All are essentially inferences from sampling" (CP 6.40, 1892).

[7] In Minnameier (2000a) I have analysed the problem of continuity versus discontinuity of developmental processes. The main result is that developmental processes are essentially discontinuous and that nonequilibrium thermodynamics adequately describes theses processes. In order to eventually explain the discontinuous transition as such (at the critical point where the emergence takes place), however, a quantum theoretical interpretation seems necessary. The same applies to connectionist accounts of the emergence of consciousness (or in the present case: higher levels of consciousness) – see e.g. Stapp's *Why classical mechanics cannot naturally accommodate consciousness but quantum mechanics can* (1995).

[8] He makes this opposition very clear when he says: „Retroduction (or abduction, G.M.) and Induction face opposite ways. (...) The order of the march of suggestion in retroduction is from experience to hypothesis. A great many people ... imagine that Induction should follow the same course. ... On the contrary, the only sound procedure for induction ..., is to receive its suggestion from the hypothesis first, to take up predictions of experience which it conditionally makes, an then try the experiment and see whether it turns out as it was virtually predicted in the hypothesis that it would" (CP 2.755; ca. 1905)

[9] Peirce also gives an example of such a „quasi-continuous" process with reference to Kepler's analysis of celestial mechanics (cf. CP 1.74, 1898).

[10] Although an inference „may have but a single premiss" (CP 2.443).

[11] The term „true" is certainly misleading in a general account of inference covering all three special types. I suggest that it ought to be understood in the sense of „valid".

[12] Maher shows that the predictivist thesis – that evidence not known at the time a theory was proposed confirms it more strongly than evidence already known (and thus merely accommodated) – holds. One crucial assumption – if not the most important one – for his derivation is his equation (12*):*

$$P(M_E|EO) = 1.$$

(1988, p. 279), which means that if evidence E is input to hypothesis-generating method M, then M will generate a hypothesis which entails E – so that the probability of a method that produces a hypothesis which entails E (i.e. M_E) is 1. Now this clearly describes the abductive method. E is the evidence that has to be explained or at least accommodated (among perhaps other pieces of evidence). It is the essential task of abduction to produce a hypothesis which entails E, or else it fails and won't produce any hypothesis at all.
Another aspect that perhaps needs to be stressed in this context, is that prediction as such is not enough to really make the difference, but it has to be a somewhat reliable prediction, i.e. a prediction on the basis of a theory.

Therefore prediction and accommodation do not differ in case the method is random (ibid., 277). I remark this, because Maher is not all too precise on this point when he comments his initial coin tossing example with the words: "The difference between the two cases is a difference between prediction and accommodation of evidence" (275). The argument at p. 277 shows that *E* may be predicted and still make no difference.

[13] Note that Peirce not only differentiates between the two, but also claims "that abductive inference shades into perceptual judgment without any sharp line of demarcation between them; or, in other words, our first premisses, the perceptual judgements, are to be regarded as an extreme case of abductive inferences, from which they differ in being absolutely beyond criticism" (third "cotary proposition" in CP 5.182, 1903).

[14] According to Peirce this also includes probability calculus, since the probability of the conclusion is deduced from the probabilities of the premises (CP 2.267-268, 1897; 7.207, 1901). This is interesting in the context of Bayesian logic, which has for long been regarded as a inductive logic. However, today it is being recognised that the logic itself is deductive (cf. e.g. van Fraassen 1989; Earman 1992).

[15] If the badness were apparent even for us, on the basis of our present knowledge, we would just not infer to (the truth of) any of the proposed explanations.

[16] This view of knowledge also allows for a reconstruction in terms of thermodynamic structures far from equilibrium which can also be translated into the language of mathematical information theory (cf. Crutchfield 1994). Elsewhere I have tried to show that this approach can accommodate Piaget's theory of cognitive development (Minnameier 2000b), except for a few problems inherent to both the thermodynamic as well as the psychological account (like that of emergent wholeness) that, at least for a physical account, seem to refer us to quantum theory (see Minnameier 2000a, 154-175).

[17] In a note to his usage of the term abduction Peirce adds that it "includes processes of thought which lead only to the suggestion of questions to be considered, and includes much besides". This means that any activation of relevant background knowledge on the occasion of a certain stimulus, i.e. whenever we perceive something in terms of something else, counts as an abduction – or, as we could also dub it, an abductive application of knowledge already acquired.

[18] The notion of environment needed in this context would require something like a quantum-theoretic interpretation interpretion that is spacially and temporally non-local.

[19] There van Fraassen claims: „The question whether a theory as a whole is true or false makes sense. It just doesn't make sense if we separate this question from the language in which we ask it, our language in use" (1997, 529). Note how Wittgensteinian this claim is and what importance Putnam attaches to this Wittgenstein's concept of truth in the framework of his own "natural realism" (cf. Putnam 1999, 64-70).

[20] The close relationship between Putnam's and Peirce's notions of truth is also revealed by Devitt (1997, 220). (The fact that he discusses Putnam with respect to this latter's then held "internal realism" does not concern this point.)

[21] To be sure, Suppe argues against induction, but he means enumerative induction, not eliminative – or Peirce's qualitative – induction.

[22] It should perhaps be noted that neither Maher's nor Suppe's nor Earman's – nor, of course, Peirce's account allows us simply to infer to the truth of the best explanation *at hand*. Earman, e.g., emphasises that a given field might not be ripe for downright eliminative induction, and he suspects that even in the advanced sciences most fields were in fact unripe (p. 182). Thus, van Fraassen's "bad lot" argument against IBE (see above) would not be valid.

[23] This type of correspondence truth does not seem to be in conflict with the "semantic conception" of theories.

[24] See also Note 19. Like Putnam and many others van Fraassen has turned stronly towards pragmatism in recent years (cf. also 2001). The only thing he refuses is realism. I wonder whether Putnam's "natural realism" were an option for him.

[25] Here he uses the terms "abstraction" and "prescission" instead of "subjectal" and "precisive" abstraction.

[26] Schurz points out that the inferences he has in mind are "correct in a *broad* sense (i.b.s.). We admit as correct (i.b.s) only those inferences which establish an at least partial *information-transfer*, a partial reduction, or as we shall prefer to say, a partial *assimilation* of P to Prem (the premises, G.M.). Thus only deductive or probabilistic inferences and certain variations thereof will count as correct (i.b.s.)" (1999, 95). This meets with Peirce's requirement that, given the suggested hypothesis *H*, the previously surprising fact(s) would be a matter of course,

[27] This problem becomes apparent in Kitcher's difficulties to treat the so-called problem of asymmetry. The pattern alone does not take care of the causal direction, so that in the famous flagpole example the height of the pole can be derived from the length of the shadow just as well as the other – the causal – way round. What this shows is that a pattern may work perfectly well even where it is not appropriate.

[28] It should be noted that unification in this sense not only complies with Peirce's account of abduction, but also with Piaget's equilibration theory, which assumes recursive disequilibration – or decoherence – on one stage of reasoning and reintegration on the next stage from a higher-level perspective.

[29] There are two more aspects, namely *incoherence* and *justification*. The role of incoherence as establishing a need of explanation has already been discussed going into Bartelborth's further explication seems dispensable in

the present context. Justification, on the other hand, is the result of coherent argumentation and thus is basically a synopsis of the other three.

[30] Bartelborth elsewhere discusses hierarchical versus holistic conceptions of (scientific) knowledge and opts for weak hierarchical relations that allow for the kind of circularity that the structuralist notion of T-theoreticity entails (1993). However, the dynamical interaction of the purported levels seems at yet to be subject to some controversy, as the debate about the role of causal versus inferential processes in belief formation reveals (see Koppelberg 1999, who advocates the causal approach against Bartelborth and Lehrer).

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