NATURE OF ABDUCTION: STRUCTURE, PROBLEMS, JUSTIFICATION

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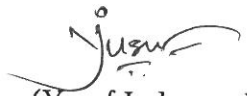
YUSUF INDOREWALA

CENTRE FOR PHILOSOPHY
SCHOOL OF SOCIAL SCIENCES
JAWAHARLAL NEHRU UNIVERSITY
NEW DELHI 110067
2016

Date: 26/07/2016

DECLARATION

I, Yusuf Indorewala, do hereby declare that the dissertation entitled, "THE NATURE OF ABDUCTION: STRUCTURE, PROBLEMS, JUSTIFICATION," submitted by me for the award of the degree of Master of Philosophy to Jawaharlal Nehru University, is my bonafide work and it has not been submitted by me or by another else, in part or full, for any other degree or diploma of this or any other University.



(Yusuf Indorewala)

Centre for Philosophy
School of Social Sciences
Jawaharlal Nehru University
New Delhi – 110067

Date: 26/07/2016

CERTIFICATE

This is to certify that the dissertation entitled, "THE NATURE OF ABDUCTION: STRUCTURE, PROBLEMS, JUSTIFICATION" submitted by Yusuf Indorewala for the award of the degree of Master of Philosophy of Jawaharlal Nehru University, is her bonafide work and to the best of our knowledge it has not been submitted by her or by anyone else, in part or full for any other degree or diploma of this or other University.

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Manidipa Sen

CHAIRPERSON

(Dr. Manidipa Sen)

Chairperson
Centre for Philosophy
School of Social Sciences
Jawaharlal Nehru University
New Delhi - 110067, INDIA

Manidipa Sen

SUPERVISOR

(Dr. Manidipa Sen)

SUPERVISOR
Centre for Philosophy
School of Social Sciences
Jawaharlal Nehru University
New Delhi-110067

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Introduction

The present study is largely occupied with the exposition of a mental process Charles Sanders Peirce animatedly named *abduction*. For its manifest ubiquity, it has been largely ignored in philosophical discussion, only to be revived, albeit in a different form, until very recently. I prefer to undertake the inquiry by emphasizing abduction as a *mental process*, to set the frame of reference through which I wish to discuss it. This frame of reference excludes for example, a formal investigation of the logical character of abduction. Furthermore, the study does not pretend to a historical orientation. Although I draw extensively from Peirce's writings to inform most of its conceptualization, and to defend that conceptualization against rival theories, I do not pretend to be truly representing Peirce's own thoughts, and I am certainly not tracing the evolution of his thoughts, or the idea of abduction itself. To my knowledge the fullest development of abduction as distinct concept of reasoning happened at the hand of Peirce. The only other philosopher to investigate it anywhere near the depth with which Peirce did was William Whewell,[1] who called it by the more familiar label, induction.

Abduction as such concerns the initial conception of an hypothesis or “the eureka moment” as Larry Laudan laconically describes it.[2] That process was of no interest to the leading currents of philosophy of sciences within the period it was proposed, who didn't think it was an integral component of scientific reasoning. The one aspect on which both the positivist camp characterized by Rudolf Carnap, and anti-positivist camp, with Karl Popper as its leading figure, agreed, was in their rejection of the process of the invention of hypothesis as a proper subject for logical investigation. Popper wrote;

> The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man— whether it is a musical theme, a dramatic conflict, or a

1 Whewell, *The Philosophy of the Inductive Sciences*.
2 Laudan, *Science and Hypothesis*. p.182

> scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.[3]

These remarks have several important features. Peirce was certainly not engaged in “empirical psychology,” and decidedly was not interested only in the psychological aspects of discovery. His interest in abduction stemmed precisely from the fact that it was relevant to the understanding of the logic of scientific knowledge. Scientific invention is not simply a matter of intuition, or a guidance from innate store of concepts and capacities, nor is it reducible to poetic insight, inimical to careful rational investigation. It belongs to the functioning of what Rochel Gelman terms *non-core domains,* or non-native mental structures. These are acquired in the process of deliberate and painstaking skill acquisition. Gelman elaborates, that “non-core domains lack initial representational resources...,” that is, resources from inborn cognitive structures, and to master it, “one has to both mount a structure and collect data that constitutes the knowledge in the domain.”[4] Thus, science needs to be learned and that learning is hard. It grows upon acquired representational structures, permitting the acquisition of knowledge which is actively and consciously serviced in the process of rationalization and invention. We can probe the “essence” of scientific knowledge to unravel its features, and ask how they developed. To the extent this is possible, it can illuminate the methods for narrowing the search field, and shorten the time needed to land upon new discoveries. This was Peirce's main preoccupation, a project he termed pragmatism, once defining it as “the logic of abduction.” [5.196]

In spite of this, Peirce did not make strict separation to exclude or include a certain region of study from examination and analysis, if it had a bearing on what he was doing. Thus, he did not dismiss the psychological (cognitive) aspects of abduction, but tried to extrapolate its features, in the same strain as Immanuel Kant, from whom he was deeply influenced. Though abduction as such has the greatest bearing and significance in the analysis of the mind, Peirce left its specifics underdeveloped, and often a confused mix, partly from the fact that he

3 Popper, *The logic of scientific discovery*. p.7
4 Rochel Gelman, “Innate Learning and Beyond.” p.226

engaged in the program over the span of 50 years. But perhaps more importantly because the precedent for a serious investigation of mental processes was unavailable in his time so it was unclear how one might actually carry the program out.

In contemporary discussions in the philosophy of sciences, some camps have been especially weary of giving a due consideration to abduction, mainly from a misunderstanding of what that program entails. The misunderstanding is strikingly evident in the following remarks of Larry Laudan;

> Indeed, if we look carefully at the writings of many 18th- and 19th-century philosophers of science (including Hartley, LeSage, Priestley, and Peirce), we frequently find a concern with modes or methods of discovery which are quite unlike enumerative induction. I shall call these 'self-corrective logics of discovery'. Such 'logics' involve the application of an algorithm to a complex conjunction which consists of a predecessor theory and a relevant observation (usually one that refutes the prior theory). The algorithm is designed to produce a new theory which is 'truer' than the old. Such logics were thought to be analogous to various self-corrective methods of approximation in mathematics, where an initial posit or hypothesis was successively modified so as to produce revised posits which were demonstrably closer to the true value.[5]

The only truth in these comments is that Peirce was emphasizing a method for discovery which was “quite unlike enumerative induction.” But the term “method” itself is misleading. It suggests for example, that there exists some unique algorithm for generating true hypothesis, so that in effect, it may be possible to construct a manual to do sciences. Peirce rejected the possibility of such a method out of hand.[6] Furthermore, abduction is certainly no “algorithm” for reaching the truth. It is, shorn of all supplementary assumptions, nothing but a *guess*.

5 Laudan, *Science and Hypothesis*. p.187.

6 He says, “There would be no logic in imposing rules, and saying that they ought to be followed, until it is made out that the purpose of hypothesis requires them.”[7.202]. All numbered references of this form refer to Charles Hartshorne, Paul Weiss, and Arthur Burks, *Collected Papers of Charles Sanders Peirce*.

Peirce's contention was that the guess could not partake of the character of an enumerative induction, because (1) it is not clear how an enumerative induction could account for fundamental unobservable (to the senses) postulates that we take for granted in modern sciences, like the subatomic particles, quasars etc., (2) any such simple rule would itself imply a truth preserving algorithm, which was rejected by assumption. Therefore, whatever the exact character of abduction, it certainly does not conform to any known inductive rule. That was precisely the point of postulating a distinct process.

Another important concern, with which this work does not directly engage, is what an abduction tells us about the nature of our minds. Conversely, we can ask, which available theory of mind accommodates abduction like inferences? Jerry Fodor[7] has critically evaluated this question, to conclude that the best available theory of the mind, the computational theory, does not confess how abduction should be accounted in terms of it. This is because, in every other type of inference, inductive or deductive, the conclusion seems to depend on local syntactic properties of the inference, precisely the kind of mental process the computational theory of mind was meant to handle. But abduction seems to require a global sensitivity. That is, conclusions of an abductive argument are sensitive to extra syntactic features. Thus, whether abduction can be incorporated in the computational theory of mind, remains an open question.

My main purpose in this work is to lay out the context from which the need for a distinct form of amplitative reasoning emerges, and to separate out that form from closely related, but not identical conceptions of it. Peirce's idea of abduction has been misunderstood far more routinely, than might be supposed at a first glance. Therefore, a primary task is to clarify what it is about, and separate it from what it is not.

In this quest, few distinctions are worth making, which might not have been made very explicit in the discussion. One is the distinction between the context *discovery*, the context of

7 Fodor, *The Mind Doesn't Work That Way*.

pursuit, and the context of *justification*.[8] The context of discovery concerns all the processes which engender an hypothesis, and facilitate its conception. The context of pursuit involves assenting toward a particular hypothesis, once conceived, as being worthy of further investigation. Finally, the context of justification probes into the problems related to believing some hypothesis true, and providing reasons for why it is believed true. In discussions on the Logic of Discovery (Chapter 2), the debates rage between understanding abduction either in the context of discovery or the context of pursuit. In discussions about the nature of an Inference to the Best Explanation (Chapter 3), abduction is confused between the context of discovery and the context of justification. The distinction also serves as a layout of the theme, so that the Chapter 1 is focuses mainly on *discovery*, Chapter 2 on *pursuit*, and Chapter 3 on *justification*.

A distinction must also be maintained between *pre-selection* and *post-selection* of hypothesis. When we use abduction as Peirce's meant to use it, we talk about hypothesis selection as pre-selection. Here, selection means the propensity of the mind to *distill* and *prefer* a few among a maze of logically conceivable hypothesis. This process for the most part is unconscious, and is sometimes alluded to as a preference for *simpler* hypothesis. On the other hand, philosophers who wrest faith in Inference to the Best explanation talk about *post-selection* of theories. Once a scientist has formulated a plausible set of hypothesis, they select one among the set which best satisfies extra empirical virtues, manifested by their explanatory power. This process, in contrast to pre-selection, is carried out consciously, by the checking for the satisfaction of the hypothesis with ones judgment of a best explanation. Sometimes, simplicity is also spoken of in this context, but here simplicity means *logical* simplicity, which requires the evaluation of how complex the principles which are used to explain something are, in comparison with the complexity of phenomenon to be explained. An explanation is *logically simpler*, when the principles are less complex than whatever it is that needs explaining. The distinction between pre-selection and post-selection with respect to simplicity considerations is illustrated by the following remarks of Peirce;

8 The distinction is to the credit of Laudan, *Science and Hypothesis*..

> Modern science has been builded after the model of Galileo, who founded it, on *il lume naturale.* That truly inspired prophet had said that, of two hypothesis, the *simpler* is to be preferred; but I was formerly one of those who, in our dull self-conceit fancying ourselves more sly that he, twisted the maxim to mean the logically simpler, the one that adds least to what has been observed....It was not until long experience forced me to realize that subsequent discoveries were every time showing I had been wrong, while those who understood the maxim as Galileo has done, early unlocked the secret....that it is the simpler Hypothesis in the sense of the more facile and natural, the one that instinct suggests, that must be preferred. [6.477]

One immediate point of notice is the emphasis on *instinct* which suggests that we are not here talking about a deliberate and conscious process. Secondly, although the remarks seem to indicate post-selection, in that we already have two contending hypothesis among which we are to make a selection, Peirce is actually expressing a deeper principle of thought. Whether a hypothesis occurs to your mind or someone else's, search for the most instinctive suggestion, and let the mind unconsciously do all the work. Peirce is proposing for general scientific knowledge what Chomsky proposes for human language. We have an instinct to discriminate a grammatical from ungrammatical sentences, without requiring conscious thinking to figure it out. Likewise, we have an instinct which tells us what a good hypothesis is, without consciously applying the principle of parsimony to it.

In Chapter 1, I will try to illuminate the inadequacy of the familiar inductive generalizations in capturing how new ideas are arrived at. Specifically, when we understand “ideas” to mean the unobservable postulates of theoretical sciences. I will also there look at how Peirce classified the distinct forms of reasoning – abduction, deduction and induction – and regarded abduction as the first step, and induction as the last step in theory construction. Even if it is abduction and not an induction which does what an induction claims to do, we can understand how special insights about the nature of abduction arise from examining some philosophical issues related to induction. I will propose for example, that abduction is the principle that sets limits on our inductive practices. Subsequently, I will go on the examine what an abductive capacity

implies about the human mind and the structure of our knowledge.

Chapter 2 will situate abduction in the context of, what is called, the *Logic of Discovery*, and try to clarify what that phrase means, and how it relates to abduction. My proposal is, that under one view of the idea of the Logic of Discovery, abduction does have a place, but the program of the Logic of Discovery is not entirely identifiable with abduction. Even if it is often claimed to the contrary, Peirce did not exclusively understand an abduction as purely a logical matter. But that point raises apparent contradictions within his account of abduction. It mainly centers on two rival descriptions of abduction as an insight and as a logical inference. I will try to support the view that Abduction exhibits a dual nature of being both an insight and a inference at once, provided we loosen up the strict conception we have of a logical inference.

Finally, I will detail the nature of justification of an abductive inference, against the justification of a distinct notion which has come to be called *Inference to the Best Explanation* (IBE). In contemporary debates in the philosophy of sciences, IBE has come to be identified as abduction, but I want to suggest that the identification is inaccurate. None of the uses of IBE, barring one, conform to Peirce's usage of abduction. Even in the case of that one conception of IBE which does conform with an abduction, there are general arguments to show that the rule could not be what it claims to be. Furthermore, I will also show how IBE is subject to many intractable problems, which are not of the same nature or order afflicting abduction, and why for that reason too, the two notions must be separated.

Structure

The two distinct forms of reasoning that go under the labels of 'induction' and 'deduction' have properties which are all too familiar. We derive, it is said, all of our contingent truths from the one, and necessary truths from the other, with trade-offs for uberty. Conclusions arrived at by means of an induction may lead to falsehoods, even if drawn from correct observations, with the advantage of expanded scope. The opposite is true of deduction; scope is limited to the premises, but room for false conclusions from true premises is eliminated in the process. I propose to deal in this chapter with what Charles Sanders Peirce identified as a third kind of reasoning. He called it, at varying points through a period of 50 years, *presumption*, *hypothesis, abduction* or *retroduction.*[9]

Abduction as Peirce proposed it, like induction, is a form of *amplitative* and *non-monotonous* inference. An inference is amplitative when the scope of its conclusion is not limited by the premises. That is, the premises do not fully determine what the conclusion will be. Monotonicity is the property whereby conclusions of an argument remain unchanged upon the introduction of new premises to the argument. Take the following arguments;

(a) Charles wears a ring; so he must be married.

(b) James is a Rights activist only on Mondays; today is a Friday; therefore today James is not a rights activist.

Charles' ring wearing makes it plausible that he must be married, but does not force it. He could be wearing the ring because he is fond of rings. However with a deductive argument like (b), we are forced to accept that James is not a rights activist today, on the pain of contradiction, if we accept both that James is a rights activist only on Monday, and today is not a Monday. So (a) is amplitative while (b) is not. Now suppose we add an additional story to (a)

9 My own terminological preference accords with later Peirce in referring to this inferential rule as *abduction*. Nevertheless I will sometimes substitute it with *hypothesis* and *retroduction* to preserve contextual cogency, only to italicize *hypothesis* where it means *abduction* and use normal case where it is intended by its usual designation in science.

which says; “Charles found his ring by the lakeside.” This amendment tips the balance somewhat against the conclusion that Charles is married. He could still be married of course, but we can no longer *justify* that conclusion on the basis of his ring wearing alone, having now known that he did not acquire it from his wife. There is no such effect to the conclusion of the argument in (b), even when we accept contraries like “James is a reluctant rights activist” or “James would rather not work on any day.”[10] We can still say, in spite of those amendments, that James is not a rights activist. As a result, (a) is non-monotonous, and (b) is monotonous.

With the properties of amplitativity and non-monotonicity common to both induction and abduction, we might ask, what separates an induction from abduction?[11] In recent times, Gilbert Harman has marked out the separation by pointing to the *explanatory value* of conclusions arrived at through abduction.[12] But if we turn to Peirce's notion of abduction, explanatory value certainly does seem salient, but there is much more to find besides. I find the consideration of all the scattered, often inconsistent remarks Peirce made regarding abduction indispensable to its understanding. I wish to flesh them out to propose a picture I have in mind.

I will discuss the broad differences between an abductive and inductive inference immediately in the next section, subsequently, detailing the more subtle differences between the two. I will then sketch a preference for Peirce's classification of inferences, against the standard inductive picture of science. In Peirce's scheme, abduction is the first step in scientific reasoning, which originates new ideas for examination. We can call this the stage of discovery, where abduction is employed. Induction on the other hand for Peirce belonged to the final stages of the reasoning process. It's role being mainly to test an abductive suggestion, and lend it justification. We can call this the justificatory stage, to which induction properly belongs. In Section 2, I will show another way of understanding abduction, which functions as a

10 Notice of course adding a contradictory premise “James is not a right activist” would change the conclusion, but contradictions are trivial counterexamples to the point.

11 This contrast occupies the main theme of this chapter, as opposed to the contrast between amplitative reasonings (both induction and abduction) and explicative reasoning (deduction). For a lucid account of that division, see Stathis Psillos, “Abduction: Between Conceptual Richness and Computational Complexity.”

12 Harman, “The Inference to the Best Explanation.”

dissolution of Nelson Goodman's paradoxes of induction. Section 3 will be concerned with the impossibility of making abductive inferences without optimality presuppositions or *canalization*. It will analyze the necessity of stipulating prior restrictions on the variation of plausible hypothesis, to account for the possibility of scientific knowledge.

1. Induction and Abduction

Upon introduction of heat to a quantity of water, it turns into steam. You infer after a few observations of the transformation, that water will turn into steam, under similar circumstances in the future. You might infer somewhat differently, that water taken from a tap, or a pond will likewise become gaseous, when treated the same way. Based on the conjunction between the liquidity of water and its transformation into steam, you might infer, further still, that another liquid, like alcohol, might transform into steam when it is subjected to heat. In the first case, the generalization moves from the present to the future. In the second, it moves from one case, to all cases of the class under question. In the last case, the inference proceeds from singular to singular instances. Peirce called all the three kinds of inferences by their usual designation, *induction*. In his words;

> Induction is where we generalize from a number of cases of which something is true, and infer that the same thing is true of the whole class. As, where we find a certain thing to be true of a certain proportion of cases and infer that it is true of the same proportion of the whole class. [2.624]

You encounter a geyser on a trip to Iceland and infer that there must be a heat source underground turning water into steam, possibly a volcano. You do this to explain hot steaminess of the water erupting from the ground. The connection between steam and volcanic activity might have been made either by experience, testimony or by inspiration. Precisely how, is at present irrelevant to understanding this *hypothesis* or *abduction*, as Peirce labeled that class of reasoning upon which one depends when drawing the above conclusion;

> [*Hypothesis*] is where we find some surprising fact which would be explained by

> supposing that it was a case of a certain general rule, and thereupon adopt that supposition. [2.623]

Lets render the two arguments again into simple consequential statements;

[I] A quantity of water before me turned to steam when heated, therefore all water turns to steam when heated.

[A] Water from the spring is hot and gaseous, therefore it must be heated by a volcano.

Taken in this form, few contrasts may be noted. Only [A] answers a why question; why do I see steam erupting from the ground? Equally it answers, why is the spring water hot? The questions could be multiplied, but these suffice to bring the point through. On the other hand, [I] only generalizes the truth of an observation (that water turned to steam when heated) from one case to every case (all water will turn to steam when heated). In this sense, abductions are explanatory and inductions are projective. Furthermore, [I] signifies similarity across a range of unobserved cases. Observing several instances of the transformation of water into steam, leads me to expect a similar transformation tomorrow, or any time thereafter. Reasoning signified by [A] points to possibilities that often bear no resemblance to the phenomenon observed. Furthermore, no experience need be required to make it. Hot steamy water from a spring leads to an inference about volcano's which are neither observed, nor suggested directly by anything observed. These considerations led Peirce to remark, that an induction;

> ….infers the existence of phenomena such as we have observed in cases that are similar...while *hypothesis* supposes something of a different kind from what we have directly observed, and frequently something which it would be impossible for us to observe directly. [2.640]

Therefore, abductions are creative and inductions expansive. Relatedly, one might say, abductions personify novelty, inductions personify similarity.

If an abductive inference terminates into a conclusion about the existence of an entity or phenomenon not directly observed or even observable – as often happens – the grounds for its proposal must by definition be conceptual. Abduction, says Peirce, "makes it start from the facts, without, at the outset, having any particular theory in view..." while induction "makes its start from an hypothesis...without at the outset having any particular facts in view..."[7.218]. Thus Abduction leads *to* the acceptance of an hypothesis, while induction is reasoning *from* an hypothesis. One might list many more contrasts with little to add to understanding of the core differences, once the basic point is grasped. In sum, abductions are explanatory, creative and conceptual. Inductions are projective, expansive and empirical.

Examples above are deliberately chosen to emphasize the contrast between reasonings Peirce deemed separately classifiable. In his early work, he remarks, "the analogy of hypothesis with induction is so strong that some logicians have confounded them," [2.632] sometimes leading to the suspicion that abduction is nothing but a label for a sub-class of induction. The *locus classicus* of this view is Hans Reichenbach, who writes that "the method of induction...will always remain the genuine method of scientific discovery" where induction is "the logical relation of the new theory to the known facts."[13] But this begs the question against the inductivist, since there are innumerable theories bearing a "logical relation" to a given set of facts.[14] Reichenbach further believes in the possibility of decomposing any amplitative inference into an induction. He writes "[i]t is the great merit of John Stuart Mill to have pointed out that all empirical inferences are reducible to the *inductio per enumerationem simplicem."*[15] Induction by simple enumeration are inferences of the form "All X's are Y's," from having observed x_1, x_2, x_3.... which are all of them Y's. But stipulating an abductive inference presupposes non-reducibility of at least two different forms of amplitative inferences.

Thus, Reichenbach's assertion runs counter to Peirce when he proposed that it is really abduction and not an induction which describes the process of hypothesis construction. Let us

13 Hans Reichenbach, *Experience and Prediction*. pp.382-383.
14 We will observe this point in more detail as the underdetermination argument in the section below.
15 Hans Reichenbach, *Experience and Prediction*. p.389.

examine the consequences of assuming Reichenbach's view. As a preliminary, we define induction as reasoning from an instance of a member within a class to the whole class and abduction as reasoning to an explanation. We can now put to test the idea, that abduction can be subsumed as a subclass of induction. The reduction presupposes that any conclusion known to follow from an abduction may equally be arrived at by means of an induction. That is to say, the inductive method can co-opt whatever it is that an abduction is supposed to allow. Consider the following procedure of the inductivist to account reasoning of type [A];

[I-D] Everywhere hot springs are heated by volcanoes, therefore this hot spring must be heated by a volcano.

We come to the same conclusion as [A] by doing the following. We always observe volcanic activity in the neighborhood of a geyser, and infer a connection between them.[16] We then project the concomitant relation to all cases involving geysers, thereby landing on the first part of [I-D]**.** Observing a new hot spring, we deduce its origins along the lines of [I-D]. Nothing other than two basic and familiar inferential systems were needed to unravel the mystery.

There are distinct advantages to this approach. If it works, by the application of the principle of parsimony, you simply shave away abduction as a separate postulate. Secondly, [I-D] could be processed under the standard Hypothetico-Deductive model,[17] and we would have a robust account of scientific practice.

Still, the [I-D] account gives a very limited picture of our actual inferential practices. There are clear cases where we reason to similar conclusions in virtual absence of the knowledge of

16 We leave aside for the moment that painful problem concerning how such a connection is derived.

17 The Hypothetico-Deduction (HD) model, is a confirmation theory, with a schema identical to that of the Deductive-Nomological (DN) of explanation, (See chapter 3 for discussion). The HD model employs a deductive schema consisting of a hypothesis as a premise, and an observational statement as a conclusion, to demonstrate the truth of the hypothesis because a conclusion may be derived from it deductively. it has the form; E implies H; H therefore E, which will be recognized as the fallacy of *affirming the consequent.* Peirce's earlier formulations of abduction resembles the HD schema, with one big difference. Unlike the HD schema, abduction is not meant to show the a hypothesis is *confirmed* because it finds a natural place in the argument, but that it may be suggested as a *possible conjecture*.

major in [I-D], as for example, when we make the inference;

> **[A*]** Water from the spring is hot and gaseous, therefore there must be a heat source below [volcano].

On its own, this is a perfectly legitimate and ubiquitous inferential type. You infer someones presence from fresh footprints in the sand. A mechanic infers the possibility of break failure from creaking sounds in the break paddle.[18] Unlike [I-D], an abductive argument in [A*] requires no prior knowledge about volcanoes. We of course cannot know what a volcano is before we observe one. The point is, we can infer to the existence of relevant conditions which explain the observation, quite apart from the specificities of those conditions. On the other hand, supposing we knew about volcanoes, the inference would simply be;

> **[A]** Water from the spring is hot and gaseous, therefore it must be heated by a volcano.

And would still be different from [I-D] being made along the procedure defined under [A*], only this time, with prior knowledge of characters relevant to the identification of volcanoes. It is some approximation of this procedure which enables us to infer, that Napoleon Bonaparte once existed, or that our universe came into being from the Big Bang. If these points should still seem unimpressive, ask how are those “hypothesis ever to be replaced by an induction?” [2.642]

There is no disputing someone who admits the process but choses to call it induction instead of abduction. William Whewell for example, meant by an induction what Peirce meant by an abduction. But Reichenbach and Mill think all amplitative inferences are characteristic of a sort of sophisticated projection from a series of observations (induction by enumeration). Thus, on this view, we really do learn about the Big Bang and Napoleon from a kind of inductive generalization. The broad outline sketched above makes it difficult to see how this

18 The last example is due to Lipton, *Inference to the Best Explanation*.

can be effected. When we closely examine the needed requirements to make the kinds of inferences which bring us knowledge of Napoleons existence, or lend credence to the occurrence of the Big Bang, we find that the analysis departs quite radically from any received understanding of induction. I will proceed to demonstrate this point immediately.

We might imagine a minature version of scientific process. Taking induction to mean any of the three forms alluded to above, and making only minimal assumptions about abduction, meaning by it, as Peirce sometimes did, a *presumption* or a *guess,* lets analyze the steps we make in solving a problem such as the one below (Figure 1).

The goal is to search for a hypothesis which will explain the rule to which all six figures on the left conform, and a different rule to which figures on the right conform. How do we figure out the correct answer?

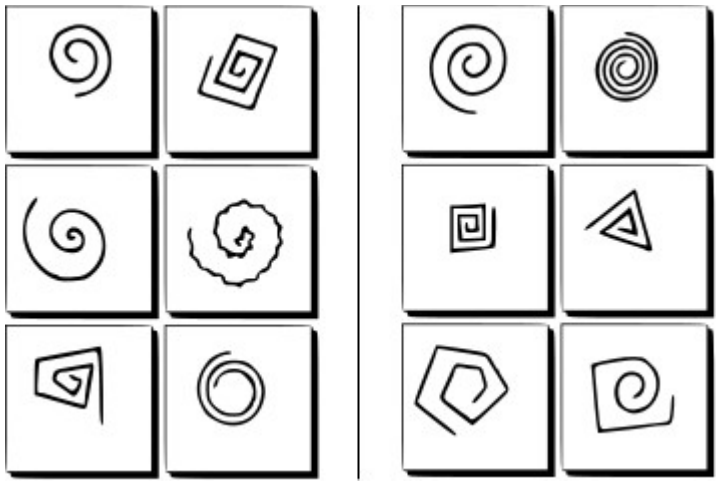

Figure 1[19]

The first step is naturally to imagine a condition (here, a rule) from which the six squares on the left would *necessarily* follow *and* those on the right would *not* follow and vice versa. So a first guess may be, that the figures on the left are traced *clockwise* inside out. The figures on

19 Image courtesy for all Bongard figures, Harry Foundalis, “Index of Bongard Problems.”

the right on the other hand are traced *anti-clockwise* inside out. Next, we would deduce all the consequences of making that assumption which relevantly apply to our problem. This is a purely logical step. We note for example, that a change in the shape of the helix's makes no logical difference to our assumption so long as directionality is conserved. Thus, in spite of the different forms the helix's take, they obey the rule we conjectured, making them logically consistent with it. Finally, we ask whether the prediction made on the basis of the rule actually holds for every shape. Depending on idiosyncratic variations in memory, a person may be able to process an abduction from a few squares and check for the rule on each of the others, one by one. Here, asking if the rule “trace clockwise inside out for left side” is generalizable on every square on the left and the converse generalizable on the right is an inductive step.

Few points are worth noting. The first step involved guessing, albeit, guessing under a constraint. One obvious constraint is that the pattern is different for different sides. The guess required a kind of imagination. We ask – if we were to make the process explicit to ourselves – what would explain the shapes assuming such and such a form? Or equivalently, we ask, which rule would entail all the shapes on the left leaving out the shapes on the right, and vice versa? It is precisely an explication of this process that we find in Peirce's characterization of abduction;

> **[Ạ]** The surprising fact, C, is observed
> But if A were true, C would be a matter of course
> Hence, there is reason to suspect that A is true. [5.189]

It is not plain at all from this characterization, how an induction could lead us into learning about the principles which govern the shapes, or the world, when we do real science. What it does enable us to learn, is whether the rule governing some shapes, which we happened to see, also holds for every other shape. Using still another example, this time from Peirce;

> A certain anonymous writing is upon a torn piece of paper. It is suspected that the author is a certain person. His desk, to which only he has had access, is searched, and

> in it is found a piece of paper, the torn edge of which exactly fits, in all its irregularities, that of the paper in question. It is a fair hypothetic inference that the suspected man was actually the author. The ground of this inference evidently is that two torn pieces of paper are extremely unlikely to fit together by accident. [2.632]

The inference in play here, he points out, is called by philosophers who confuse the two kinds of reasoning, “induction of characters,” a misleading description, since “characters are not susceptible to simple enumeration like objects [and]... characters run in categories.” We make the inference without exhaustively listing all the characters, but pick a few we deem relevant to the case. An induction could only allow the inference “that the two pieces of paper which matched in such irregularities as have been examined would be found to match in other, say slighter, irregularities.” (2.632) Abduction jumps from the appearance of the paper to its ownership. That is its distinguishing mark. The claim we began with earlier, *that abduction is nothing but another type of induction*, looks less persuasive on closer inspection. We could of course explain and redefine induction so that inferences like [A̤] are incorporated, but as we will see, useful analytic issues arise when we adopt Peirce's classification instead, and understand induction as a last step in the process of science.

2. The Limit on Hypothesis

The induction in the example above was not particularly daunting, and raised no peculiar problems. An important reason for this is because we had there a universe of *complete* evidence needed to solve the problem, which admitted a unique solution. Actual empirical sciences are almost never so fortunate as regards evidence. The data at hand always insufficiently represents the theory which entails it, or equivalently, the theory is underdetermined by the evidence. There are innumerable ways we can conceive the world consistent with what we know about it. Should we stick to observing the night sky with unaided vision, as the Ionian's did, we would not think it astonishing that the earth is cylindrical, or a dodecahedron, or an inverted pyramid, the flat side of which we occupy. To take from Peirce;

> Consider the multitude of theories that might have been suggested. A physicist comes across some new phenomenon in his laboratory. How does he know but the conjunctions of the planets have something to do with it or that it is not perhaps because the dowager empress of China has at that same time a year ago chanced to pronounce some word of mystical power or some invisible jinnee may be present. [5.172]

We can get a more lively picture of the underdetermination of theory, by another Bongard problem. This time, we conceal some squares to simulate the condition with limited data (figure 2).

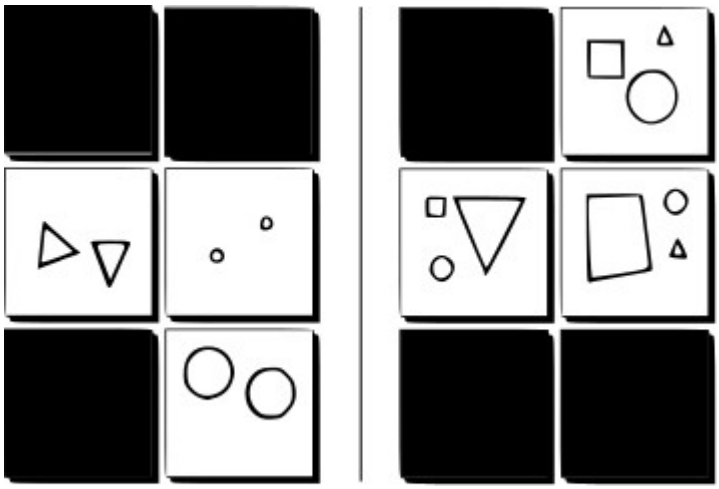

Figure 2

Here, three hypothesis are compatible with the data (open squares);

(1) Left: Contains only triangles and circles. Right: Contains triangles, circles and squares.
(2) Left: Triangles and circles are never paired. Right: Triangles and circles are always paired.
(3) Left: The shapes are all identical in size. Right : At least one shape differs in size.

The three hypothesis all of them confirm the data equally well. Thus, the data underdetermines which theory uniquely explains it. Selection from among (1), (2) and (3) constrains both what we should expect and what we should look for. And an expectation is nothing else but the psychological manifestation of an induction. We expect let us say (3) will hold for all cases, because we have characterized the situation in terms of it, and hope that characterization will hold for newer cases. Should our expectation be disappointed, as it would be if we were to select either (1) or (2) (see figure 3), we would have to make readjustments on our initial conjecture, or abduction, to try and guess another rule which wont be disappointed. This implies that induction has another role beside generalization. Should an induction fail to hold, it could prompt a search for further constraints. The emotive manifestation of a failed expectation is *surprise*, which Peirce embedded as an integral component of an abductive inference in **[Ạ]**. A surprise can often prompt new discoveries. Such was the case when perturbations in the orbit of Uranus prompted Alexis Bouvard to hypothesize the existence of a large body, later discovered to be Neptune. Alternatively, it could provoke a revision of the initial hypothesis. For example, the anomalous precession of mercury on Newtonian assumptions was better explained on Einsteins General relativity, eventually leading to its adoption.

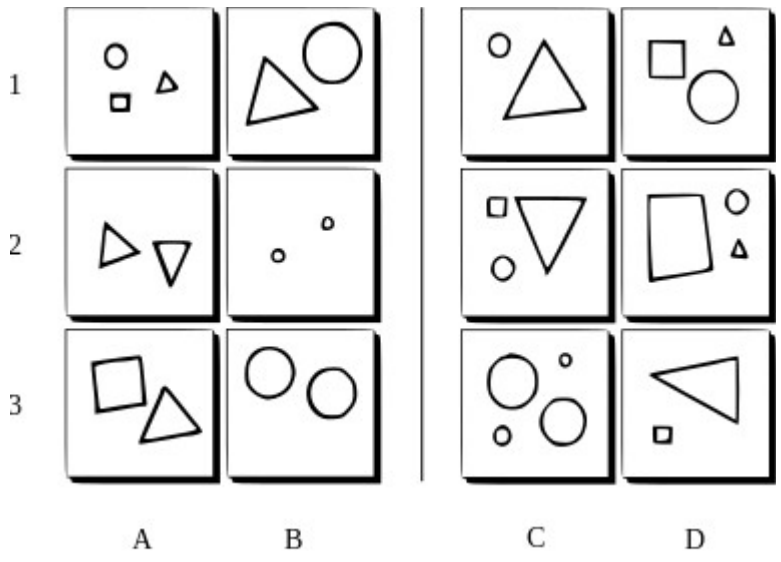

Figure 3 : (1) is overruled by A3, while (2) is inconsistent with B1 and C3

2.1 The riddle of induction

We are then faced with a problem. How do we know when an induction is warranted? Call this problem, with Nelson Goodman the *inductive riddle*. Standard responses appeal to theoretical virtues long established through the practice of science that range from simplicity considerations, coherence, consilience, avoidance of ad-hoc theory etc. Given two contending theories which equally account the data, if one of them satisfies one or few of these criteria better than the others, that one is to be selected. The problem is, they are available only when we are faced with competing theories, and both are known to have accounted relevant phenomena. It is a picture of scientific process that emerges naturally from inductivist accounts, because it deals with the justificatory stage.

How well does it fare as a basic picture of scientific progress? Not too well, I think. As an answer to the under-determination problem, it seems rather conveniently crafted. If there are innumerable theories to account data, and we only ever select one true theory, implications of following the above procedure should be that we enlist all conceivable theories numbering infinity, and test them one by one for their satisfaction of extra empirical virtues. That process is neither feasible nor carried out in fact. Scientists only ever suggest a few hypothesis that seem plausible, and it is those – if they ever reach that stage – which are finally considered for selection. But this implies there is some restriction at the stage of *proposing* a candidate theory. This is the target area of abduction, the adoption of theories because they seem conceptually plausible, well before all the data is in.

One rather simple way to grasp the idea of pre-selection of theory is straightening out the implications of Nelson Goodman's *The New Riddle of Induction*. He discovers a basic puzzle beneath the *instantial* model of confirmation – a procedure salvaging the intuition about induction, that we project observed patterns onto unobserved cases, past, present or future.[20] The model simply seeks confirming evidence for inference of the form “All A's are B's,” and takes them to lie in cases where an observed A is also B. When I see A that is a B, it is evidence confirming the statement “All A's are B's.”

20 This is the third type of an induction we discussed in the Section 1.

The puzzle goes as follows. Consider a case where all observed emeralds before a certain time *t* are green. The hypothesis “All emeralds are green” is thereby supported at time *t*. We can elaborate the case as long as we like by adding emerald *a*, emerald *b*, emerald *c* and so on as confirming evidence for that hypothesis. Goodman continues;

> Now let me introduce another predicate less familiar than "green". It is the predicate "grue" and it applies to all things examined before *t* just in case they are green but to other things just in case they are blue. Then at time *t* we have, for each evidence statement asserting that a given emerald is green, a parallel evidence statement asserting that that emerald is grue. And the statements that emerald *a* is grue, that emerald *b* is grue, and so on, will each confirm the general hypothesis that all emeralds are grue. [21]

Right up to time *t*, things go as expected. Emerald *a* is either green or grue, emerald *b* is green or grue, in one or the other language. After time *t* however, given the definitions, the next emerald is either green or grue, that is, blue. We now have a situation where we get confirming evidence for blue emeralds from having observed only green emeralds. The problem illustrates two important facets concerning induction. What kind of rule should block out the problem cases and leave the good ones? In this light, is it ever possible to carry out a valid inductive inference? One answer to both questions is that *projectible* predicates, that is, predicates which produce law-like statements, or statements that have resulted from successful application of induction, are really determined by our store of background knowledge. This is the view, that having had sufficient experience in recognizing cases where induction succeeds and where it does not, we acquire a sense of those predicates that work and those that do not. There are two problems with this answer. In the first place, as hinted above, the inductive story never tells you how a hypothesis was arrived at. How did a scientist learn to sieve out workable hypothesis from unworkable ones? His answer, by induction. That is to say, an induction based on a theory that worked, begging the question against him. The second problem is the response does nothing to address the fact that scientists working in totally different conditions with a

21 Goodman, *Fact, Fiction, and Forecast*.

different data set and experience (background knowledge) converge upon very few theories as science develops. But according to the background knowledge story, we should get very different world views.

A alternative view of the problem has been suggested by Fodor and Chomsky[22] along the lines that looks immediately obvious, once properly understood, and almost a matter Peirce would have taken for granted. To escape Goodman's problem is to find a rule that somehow blocks the formation of some such predicate as “grue” to leave just the right ones. Given that rule however, it is possible to produce a counter example, as Goodman shows over and over. We therefore proceed to block that with some new rule. But to try to block them simply means we are selecting desired cases from undesired ones. Hence, the list of hypothesis we consider are per-ordered, in terms of preference. That is to say, even before we seek to extract a pattern from the available data, we are predisposed to choose a pattern for projecting the data. What the nature of this preference is, represented by such an algorithm is essentially the problem of abduction, or the problem of seeking the principles which guides our inductive practices.

If the discussion above is on the right track, the problem of the *inductive riddle*, is equivalent to the problem, which abductive conjectures would not be disappointed? This is because asking whether an expectation will be satisfied, is to ask if I have formed the right expectation in the first place. Posed this way, we see immediately that the question is hardly genuine. The most powerful indictment against this line of questioning came from Goodman himself. To ask if we can know in advance if our prediction is true is asking for “prevision,” rather than a philosophical analysis. Good or bad induction must ultimately be borne out by experiment, under the belief that our instinctive capacities won't lead us astray.

2.2 Parsimonious Pre-selection

What is the nature of this predisposition to pre-select the right hypothesis from the wrong ones? A general answer exhausting every way we conceivably do this, is still well beyond

22 Massimo Piattelli-Palmarini, *Language and Learning: The Debate between Jean Piaget and Noam Chomsky*. p. 259-261.

reach. Goodman's paradoxes at least give us a clue, that we do in fact search a certain kind of hypothesis, which, as Peirce said on many occasions appeal to our “instincts.”

The insight may be given some flesh and blood to demonstrate the much touted preference for “simpler” theory. How does one compose the following figure from more basic units?

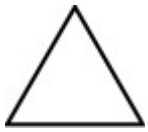

Figure 4: A simple Triangle

One answer, and perhaps the most pervasive, would be this;

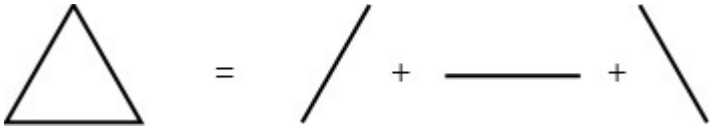

Figure 5: Minimal Parsing in action

We imagine the basic units to be simple line segments, joined end to end to make the triangle. But this is not the only possible solution. We could do it to the same effect, in infinitely different ways. Take two other solutions;

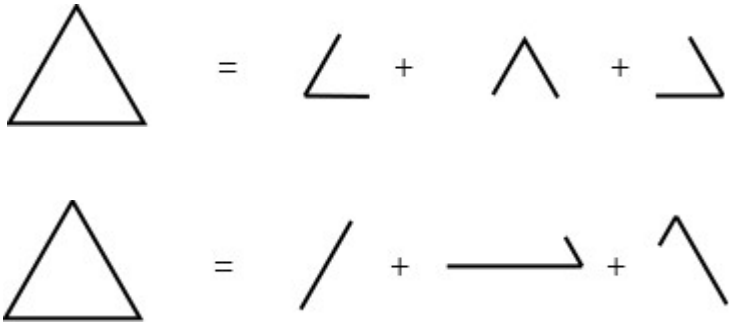

Figure 6: More complex parsing

But the solution in Figure 5 somehow seems to have a greater appeal than either of the

possible way's of parsing shown in Figure 6.[23] It is not only a matter of appeal however. It is also relatively hard to think of solutions that depart from the kind of simplicity of the first example. The process beneath an abductive inference likewise inclines us to admitting only certain kinds of solutions, whatever the exact configuration of those preferences are. It functions in something like the way represented in these examples, only on a higher order of abstraction; because visual cues are just a component of scientific theories.

3. Canalization

In the abstract, in the face of underdetermination of theories, the success of science is a puzzling phenomenon. How do we account and explain anything when there are any number of ways it could be done? We could not hit hypothesis at random, the odds against acquiring knowledge are too great. Peirce writes;

> Think of what trillions of trillions of hypotheses might be made of which one only is true; and yet after two or three - or at the very most a dozen guesses, the physicist hits pretty nearly on the correct hypothesis. By chance he would not have been likely to do so in the whole time that has elapsed since the earth was solidified [5.172]

His answer was that abduction is a kind of an *instinct* guiding us ever closer to the truth;

> It is certain that the only hope of retroductive reasoning ever reaching the truth is that there may be some natural tendency toward an agreement between the ideas which suggest themselves to the human mind and those which are concerned in the laws of nature. [1.80]

Leaving aside the difficult questions about how we could have adapted such capacities, we might search for the necessary conditions to make this ability possible and intelligible. I want to argue that the idea is not as exotic as it might look at a first glance, and has parallels in

23 For a detailed and insightful discussion on “minimal parsing,” see Harry Foundalis, “Fundamental Principles of Cognition.”

biology.

The remarks in the preceding section, if correct, show that there must be some preordering of hypothesis if an induction is even to be possible. That is to say, we must have a fixed array of choices from which an abduction selects. Undoubtedly, in the course of experience, our choices must alter. We are required to assume that they do not alter chaotically, at the pain of contradicting our initial contention that there is an agreement between the mind and laws of nature. Hence, while experience shapes what we deem a plausible explanation in some context, it operates within limits.

The underlaying limit setting principle is termed *canalization*.[24] It was used by C.H. Waddington, to explain why certain traits within an organism differ as little as they do within a species, in spite being subject to varying conditions in the environment and even variation in the genotype. The former is straightforward. No two organisms will experience the exact same conditions throughout the course of its development, and yet they would not, as a result, exhibit the effects of the environmental impacts very dramatically. Similarly, the genotype "absorb a certain amount of its own variation without exhibiting any alteration in development."[25] For instance, the "wild" (natural) type of a Drosophila, show a strong constancy which breaks down in mutants. This is a common feature across organisms, among which it will be found, that those belonging to the same specie, when collected, are "as like as peas in a pod." The advantages of canalization, Waddington explains, is that "it ensures the production of normal, that is, optimal, type in the face of unavoidable hazards of existence"[26]

We can extend the analogy for our purposes and recognize, that groups of scientists working in completely different settings with a different set of data, yet converge upon similar hypotheses. The hypothesis proposed would then be, in the relevant problem domain, "as like as peas in a pod." The evidence for this is not hard to muster. First of all, there is the crude

24 The term is used by Noam Chomsky to account the possibility of general learning and language acquisition. Chomsky and McGilvray, *The Science of Language*..
25 Waddington, "Canalization of Development and the Inheritance of Acquired Characters."
26 Ibid. p.564.

observation. In a very short span of time we have landed upon successful theories. Peirce remarks;

> Man has not been engaged upon scientific problems for over twenty thousand years or so. But put it at ten times that if you like. But that is not a hundred thousandth part of the time that he might have been expected to have been searching for his first scientific theory. [5.172]

Cases where discoveries were made independently and simultaneously by different people also abound. The discovery of Cartesian Geometry by Rene Descartes and Pierre de Fermat, of Calculus by Newton and Leibniz, of Oxygen by Jospeh Priestly and Antonine Lavoisier, the theory of evolution by Charles Darwin and Alfred Russell Wallace etc. It is not only simultaneity of discovery that confirms convergence. We also observe that many scientists readily adopt completely new 'paradigms' even across scientific revolutions, barring some initial resistance. Cases in point are the theory of Relativity and Quantum Mechanics. Both are widely regarded as violative of common sense intuitions, yet both are nevertheless taken to be, equally widely, correct representations of the workings of the inanimate world. When proposed, refuting evidence was ignored in the case of the one, and general skepticism as to the logical cogency of the other was set aside.[27] These observations make a strong case for the truth of canalization of our knowledge about the world, which is in turn explained by the

27 In a correspondence with Einstein in 1923, Max Born mentions the successful replication of the Michelson-Morley experiment which purportedly confirmed the existence of aether winds – the original Michelson experiment had failed spectacularly in this – and urges him to respond publicly explaining why the experiment must be wrong. The curiosity in this event is that both Einstein and Born took it for granted, without even looking at the experimental procedure, that the results must be incorrect. See Born, “The Born-Einstein Letters.”p45. for the exchange. Einstein was a leading critic of the entire conception of Quantum Mechanics, because he believed it was conceptually incoherent. Leonard Susskind explains the argument as a debate between Einstein and Neils Bohr (who was one of the leading defenders of the theory); “Our ordinary intuition about physics systems is that if we know everything about a system, that is, everything that can in principle be known, then we know everything about its parts. If we have complete knowledge of the condition of an automobile, then we know everything about its wheels, its engine and its transmission, right down to the screws that hold the upholstery in place. It would not make sense for a mechanic to say, “I know everything about your car but unfortunately, I can't tell you anything about any of its parts. But thats exactly what Einstein explained to Bohr – in quantum mechanics, one can know everything about the system but nothing about its individual parts...”Leonard Susskind and Art Friedman, *The Theoretical Minimum: Quantum Mechanics*. p.xii.

abductive instinct.

In spite of the preceding considerations, it might seem a bit too strong to say that man has a natural propensity toward the truth. The debates between realism and anti-realism in the philosophy of science at least indicate that the question of truth is not settled in as straightforward a way it looks here. While those disputes rage on specific epistemic assumptions, it is worth remarking canalization does not logically imply anything much as regards truth. It is true that Peirce's version of abduction involves strong presuppositions about canalization toward truth.[28] But they are really independent notions. To say that scientific theories are channeled along a relatively fixed pathway does not tell us where the pathway leads. It is perfectly possible for example, that all our best theories are nothing but articles of faith, which appear true from a point of view, but in fact may be quite off course. Alternatively, it is possible that we respond to our best theories instinctively *as if* they were true, because we cannot conceive how it could be otherwise. Unless we are mystics, it is perfectly sensible to respond in this way to the appeal of our instincts.

What are the specifics of canalization with respect to human knowledge? The answer must involve detailing the nature of the abductive instinct, which would require some understanding of the configuration of our cognitive system (relevantly, how hypotheses are preordered in

28 And expressly emphatic as well. He says “A man must be downright crazy to deny that science has made many true discoveries.”[5.172] In the hierarchy of mysteries of the universe, I do not think this particular claim ranks so far above some other puzzling curiosities. Jerry Fodor mentions a closely related one, with respect to mental representations which are computational; “....just as being truth preserving is the characteristic virtue of *computations* as Turing understands them, so too it is the characteristic virtue of mental processes as rationalists understand them. One true thought tends to lead to another in the course of cognition, and it is among the great mysteries about the mind how this could be so.”Fodor, *The Mind Doesn’t Work That Way*. p.18. But Peirce gave really bad arguments to explain how it is the case that we have the capacities to figure out the truth. He says “The manner in which [Man] comes to have....knowledge seems to me tolerably clear. Certain uniformities, that is to say certain general ideas of action, prevail throughout the universe, and the reasoning mind is [it]self a product of this universe. These same laws are thus, by logical necessity, incorporated in his own being.” [5.603] Chomsky exposes the absurdity; “Here, it seems clear that Peirce’s argument is entirely without force and that it offers little improvement over the preestablished harmony that it was presumably intended to replace. The fact that the mind is a product of natural laws does not imply that it is equipped to understand these laws or to arrive at them by “abduction.” There would be no difficulty in designing a device (say, programing a computer) that is a product of natural law, but that, given data, will arrive at any arbitrary absurd theory to “explain” these data.”Chomsky, *Language and Mind*. p.85

some context). Unfortunately, I have no specific proposals. At present, we satisfy ourselves that Peirce meant to account the possibility of the development of science along these lines, when he proposed instinct as an answer to acquiring true knowledge.

Conclusion

Abduction is a distinct capacity, not reducible, or intelligibly re-describable in the form of well recognized amplitative inferences. We have seen, the operation of mind we perform to “get at” those kinds of knowledge are not decomposable into simple generalizability rules. That on the most standard understanding of induction, we could not have acquired knowledge of basic historical facts (like the fact that Napoleon existed), fundamental postulations of science (Big Bang for instance), or even basic rule governed problems like the Bongard puzzle. The capacity we rely on for these cognitions nonetheless partakes of the nature of non-demonstrative inference, prompted by instinct.

Furthermore, we are required to assume, in the face of underdetermination of theory by evidence, a kind of channeled growth of knowledge, without which it would not be possible to learn anything. An abduction is an inference which operates within this channelized structure.

Problems

In the previous discussion, I used abduction ambiguously to mean a kind of *reasoning*, and a kind of an *instinct*, without resolving which one of these descriptions is most suited to the cognitive process under question. In doing so, I was mirroring the ambiguity in which Peirce himself left the issue. As it turns out, resolving abduction into either an instinct or an inference or both at once has turned out to be a vexatious problem. In the following, I side with the opinion that abduction, in Peirce's view, was both an instinct – or insight – and reasoning. I will do this by drawing a distinction between the outward form of an abductive argument which conforms with traditional reasoning, and the mental processes beneath it, which is more akin to a perceptual judgment. Abduction, encompass both these elements.

The different ways in which Peirce often wrote about abduction have raised independent interpretations of his project of the classification of reasoning in sciences, which are either partial characterizations, or plainly wrongheaded, in my view. Norwood Hanson's explorations of the Logic of Scientific Discovery (henceforth LOD), falls in the former camp, and Larry Laudan's criticisms of both Hanson and Peirce falls in the latter camp. I will track these issues in Section 2. Harry Frankfurt has charged Peirce's notion of abduction with incoherence, when understood either as the source of new ideas, or as an inference for *adopting* new hypothesis. He does land upon a positive alternative for what abduction could be, which I will not take up, mainly because I think Peirce's own conception can be justified against Frankfurt's charges. I will set about to do this in Section 3. Finally, I will demonstrate how the dual features of abduction interact to bring new knowledge from thinking of the process as a kind of *simulation*.

1. Preliminaries

The major themes I will discuss in this chapter, emerge from either conforming with, or in opposition to, an intuition about the process of suggesting new hypothesis, which is captured succinctly by the following remarks of Karl Popper;

> ….there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process….every discovery contains ‘an irrational element’, or ‘a creative intuition’, in Bergson’s sense.[29]

Now Peirce's writings on abduction, tend to obscure the boundaries Popper is drawing between a “creative intuition” and a “logical method,” in the act of discovery, and therefore tends to be imprecise as to whether abduction is an insight, or a type of logical inference. This is perhaps why it has been possible for Laudan and Frankfurt to interpret Peirce to have been denying Popper's intuition. This is also why, I think, reading abduction as either strictly insight or logical inference falls afoul of Peirce's own views. Curiously, Hanson shares Laudan and Frankfurt's assumptions as to Peirce's meaning of abduction, but he does not share Popper's intuition about discovery. He thereby defends abduction as a *logical method* of arriving at new ideas.

There is a further distinction that Popper makes in the mentioned comment, which I think may be crucial to understanding Peirce's sense of abduction against Laudan, Frankfurt or Hanson's interpretations. This is the distinction between the *method* of discovery, and *logical reconstruction* of discovery. I am inclined to think Peirce would wholly agree with Popper in rejecting any logical method, or rules to strike upon new ideas, but he would disagree with him in rejecting a logical reconstruction of that process. This distinction may be at the center of understanding the “dual” character of abduction, as both an insight and an inference. Having cleared up the different commitments to different intuitions and usage of terms among the four philosophers I want to deal with here, we can delve deeper into the specific arguments they make.

2. Logic of Discovery?

So was Peirce really seeking a *logic of discovery* when he tinkered with abduction? K.T Fann and Norwood Hanson seem to think so. Fann writes, that “the process of constructing and

29 Popper, *The logic of scientific discovery*. p.8.

selecting a hypothesis," or the subject matter of LOD, is a reasonable affair, "susceptible of a logical analysis." There is more in it for the logician than simply "analyzing the arguments and supporting already invented hypothesis."[30] He draws this moral from Peirce, when he wrote "each chief step in science has been a lesson in logic."[5.363] Thus, Fann seems to think that Peirce is suggesting a logical inquiry for the emergence of new ideas. In this way he takes abduction to be essentially a search for the logic of discovery.

Similarly, Hanson distinguishes the "(1) reasons for accepting an hypothesis H, from (2) reasons for proposing H in the first place,"[31] where (2) applies to the process of discovery and is presumed by him a logical inquiry on its on terms. This is because the kinds of reasons given for the *acceptance* of an hypothesis are of a logically different sort from the kinds of reasons given for *proposing* it. And proposing a hypothesis is just what abduction relates to. Thus, both Fann and Hanson emphasize the logical aspect of abduction. Reading Peirce from their viewpoint leaves the impression that abduction essentially concerns a kind of discovery process decomposable in logical terms.

I think Hanson's contributions to the logic of discovery have been misidentified with Peircian abduction. His explorations are interesting and have a direct and limited bearing on what Peirce did say, but they do not properly encapsulate the different things he said on the subject. It is therefore essential to discuss LOD to search for the source of the mis-impression, and to see what bearing LOD itself has on abduction.

There are mainly two senses to the phrase *the logic of discovery*. In fact, the phrase is, to quote Laudan "notoriously ambiguous,"[32] so that to be precise, there are at least two *clear* senses of LOD. In one sense, LOD is the technique of developing new theories, a "set of rules or principles according to which new discoveries can be *generated*."[33] Laudan incorrectly claims that Peirce and Hanson sought to find this technique through abduction;

30 Fann, *Peirce's Theory of Abduction*. p.3.
31 Hanson, "The Logic of Discovery." p.1074.
32 Laudan, *Science and Hypothesis*. p.181.
33 Ibid. p.182.

> ….both Peirce and Hanson construed the method of 'abduction' as a technique of scientific discovery. But it is nothing of the kind. As Wesley Salmon and others have pointed out, abduction does not tell us how to invent or discover an hypothesis. It leaves that (possibly creative) process unanalyzed and tells us instead when an idea is worthy of pursuit (namely, when it explains something we are curious about).[34]

But Peirce himself rarely ever used LOD to describe what he was doing, if he did at all. Hanson on the other hand, consistently did refer to what he himself was doing as LOD, but he rejected even the possibility of writing guidebooks to help make scientific discoveries, let alone think of himself as writing one.[35]

In the second sense, LOD is the analysis of the reasoning that goes into the early stages of science in the development and defense of one or another hypothesis. As mentioned, this is the sense in which Hanson used and developed the idea. The following is an outline of LOD understood as the logical analysis of the discovery process; Having pointed out that the reasons for proposing a hypothesis have a uniqueness about them, Hanson considers the question, whether an inquiry into this process belongs properly to logic, or to psychology or sociology. Rejecting Popper's intuition alluded to above, which would imply that proposing a hypothesis belongs to either psychology or sociology, he makes a case instead for a logical inquiry and presents its subject matter. Reasons for proposing hypothesis could take one or both of the two forms below;

(1) Does H look as if it might be that from which known phenomenon p_1 p_2...etc. can be shown to follow?
(2) Does H look as if it might explain p_1 p_2... etc. (for some values of H the answer at any time would be “No” - an answer for which good reasons could usually be marshaled.[36]

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34 Ibid.
35 To this order, he says “Neither Aristotle not Peirce imagined himself to be setting out manual to help scientists make discoveries. There could be no such manual.”Hanson, “The Logic of Discovery.” p.1073)
36 Ibid. pp.1074-75

For example, Kepler imagined the motion along an ellipse for planetary orbits as a result of observations of the velocities of the planet Mars along different axis of the eccentric. This is component (1). The hypothesis of elliptical motion looked as if it might explain the observation, but the shape and the color of planet did not look like it might do any explaining. This is component (2). Hanson later went on to stress that the reasons do not bear on this or that hypothesis in particular, but on the general *type* of some hypothesis.[37] Before the evidence is in, a scientist might say something like "whatever turns out to be the case eventually, the answer must look something like the following...," where the blank is filled by the general character of some hypothesis. Example;

> Before he had determined the exact value of the charge on the electron, Millikan could have advanced good reasons for the contention that (whatever the exact value might turn out to be) the fundamental charge would have some discrete value. It would not be subject to random fluctuations in intensity. Again, when Leverrier sought to explain aberrations in the perihelion of Mercury, he had the best argument for the plausibility of another "hidden planet" hypothesis.[38]

Or, to keep to commonplaces, on conceptual grounds, a detective could make the case that a human being was responsible for upset furniture in the house, and not God's wrath, or even the will of the furniture to disorder itself.[39] So, the part of the discovery process dealing with reasons for making a certain hypothesis should be taken "as designating the *type* of hypothesis more likely to meet with success than other types."[40]

Therefore, he cumulatively tries to establish the three points;

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37 Hanson, "More on 'The Logic of Discovery." p.183.

38 Ibid. p.186

39 Examples from the history of science have an advantage over ordinary day-to-day cases in that the legroom for misunderstanding is greatly narrowed. Thus, it is easy to be misled by this example, and to imagine, that a burglar is not the only possible explanation for a ransacked room. God is a conceivable reason, as is the decision of the table and bookshelf to fall into places of their choice. This is so, but the point is whether there are good reasons to prefer these hypotheses. Perhaps good reasons can be mustered for the act of God. In that case, the divine too has a place in our world. Unfortunately, in our times, philosophers have grown out of the habit of appeals to divinity in forging explanations.

40 Hanson, "More on 'The Logic of Discovery." p.183.

(3) LOD is a logical inquiry which concerns reasons for advancing an hypothesis, as opposed to reasons for accepting an hypothesis.

(4) Such reasons as there are for proposing a theory have a logical form different from the logical form of such reasons as there are for accepting theories.

(5) These are also reasons to show why hypothesis of a certain *kind* is to be preferred over hypothesis of another kind.

The points of emphasis of this account are designed to be a response mainly to theorization of the Hypothetico-Deductive[41] model of science. Hanson sought to show that the HD model leaves out a very important part of the scientific process, which has to do with the *proposal* of hypothesis. He argues that many philosophers of science, who place their faith in induction as the foundation of scientific discovery, present accounts which "reads less like a Logic of Discovery than the logic of the Finished Research Report."[42] But even putting (3), (4) and (5) together, it is easy to square with Laudan's charge, that the conception of abduction Hanson suggests with his LOD framework leaves the initial creative process of forming an hypothesis completely unanalyzed "and tells us instead when an idea is worthy of pursuit."[43] Moreover, as is clear, (3) is basically *justifying* the preference for an hypothesis. That is a different matter altogether. Although it still applies to the stage of discovery, where an hypotheses is proposed, it does not answer *how* they are proposed. To produce reasons to justify a theory choice, is to produce reasons to consider a theory worthy of further investigation. Once the entire psychological process takes its course, and the scientist settles onto an hypothesis, he presents reasons in the form Hanson talks about, to show why the hypothesis deserves closer inspection. Such reasons are sometimes not identifiable with reasons produced in inductive

41 The Hypothetico-Deduction (HD) model, is a *confirmation theory*, with a schema identical to that of the Deductive-Nomological (DN) of explanation, (See chapter 3 for details). The HD model employs a deductive schema consisting of a hypothesis as a premise, and an observational statement as a conclusion, to demonstrate the truth of the hypothesis. This is done by showing that a conclusion may be derived from it deductively. it has the form; P implies H; H therefore C, which will be recognized as the fallacy of *affirming the consequent.* Peirce's earlier formulations of abduction resembles the HD schema, with one big difference. Unlike the HD schema, abduction is not meant to show that a hypothesis is *confirmed* because it finds a natural place in the argument, but that it may be suggested as a *possible conjecture*.

42 Hanson, "The Logic of Discovery."

43 Laudan, *Science and Hypothesis*. p.182.

support of a theory. For example, Chomsky proposes that the human mind must have a rich cognitive structure to process language, because a child develops a language much too quickly to have learned it from impoverished structures the stimulus-response story assumes. That is an argument of a different order to that which states that human language exhibits the property of discrete infinity, requiring at least a minimal recursive structure somehow realized in the brain. The first marks conceptual reasons to propose a theory, the second is an empiric-inferential confirmation of the theory. But Hanson leaves out of focus a deeper inquiry, which asks, what the mechanisms behind abductive reasoning are? Thus, Laudan correctly accuses Hanson of having "obfuscated the real nature of the logic of discovery."[44] The Obfuscation, I think, is the result of misidentifying abduction with LOD.

While, Laudan's criticism, so long as it is confined to Hanson seems fair enough. He is properly cautious of advancing the charge with the same force onto Peirce. Great stress has been laid on the logical nature and structure of abduction in Hanson's reimagination of it, and not in Peirce's original proposals. I think Hanson overplays this aspect of the inquiry. For one thing, it is widely acknowledged that Peirce had many things to say about abduction which were in mutual tension. To point to anything in his account and identify that as representative of Peirce's entire view would surely be misleading.

Secondly, among the many things he did say regarding abduction, some of his proposals are far more interesting and useful from the modern point of view. With respect to references to LOD in Peirce's investigations on abduction, as much as he was interested in the nature of our reasoning capacity, he was interested in seeking a system to reduce effort and quicken the process of discovery. He emphasizes that much progress is made in science by adopting the method or model of a previous discovery for a new investigation. The logic underlying the previous findings can therefore be transposed to other parts of the world with fruitful results. It is this part of his work which concerned LOD. He writes;

> … what does it matter how the work of abduction is performed? It matters much, for the reason that it originates every proposition. It is true that, however carelessly the

44 Ibid.

> abduction is performed, the true hypothesis will get suggested at last. But the aid which a correct logic can afford to science consists in enabling that to be done at small expenditure of every kind which, at any rate, is bound to get done somehow. The whole service of logic to science, whatever the nature of its services to individuals may be, is of the nature of an economy. [7.220 Fn]

But this involves the scientific investigation itself, not the investigation of reasoning in science.[45] Whatever this other thesis may be, which relates to all the outward aspects of abduction, there must be a discovery process, whose principles we do not understand yet, that puts a limit on admissible hypothesis. Science could not begin without narrowing the search field. Most of these cognitive aspects of abduction are obscured as a result of a total preoccupation with LOD.

3. The Creative Capacity

In the previous chapter, we looked at the prerequisites to make knowledge of the world possible. In doing so we only cursorily defined the exact form abduction takes, and how it brings us new ideas. As we shall see, this matter is not as straightforward as it might seem, and has been a source of some controversy.

3.1 Objections against abduction

Harry Frankfurt objects to the two major ways Peirce spoke about abduction. These objections depend on mainly two ways in which Peirce used the term; first, as a source of new ideas, and second as an inference which latches onto an existing hypothesis. Frankfurt then goes on to reject the idea that an abduction can be understood as either of these two things.

Let us consider Frankfurt's first argument, that an abduction cannot be understood as a creative faculty. He builds his argument upon an intuition that the process of discovery is not amenable to logical analysis. There is no rule which can substitute for the creative act of inventing a

45 Or the process of landing upon truth with the fewest number of steps. Economy concerns for hastening scientific discoveries was of central importance to Peirce, and abduction had a prominent place in it. For a review of this aspect, see Rescher, “Peirce and the Economy of Research.”

theory. This is the intuition, which Popper explicates in the quote mentioned earlier.

Moreover, Peirce was himself emphatic about the limits of deliberative reasoning; "...self-control is the character which distinguishes reasonings from the processes by which perceptual judgments are formed, and self-control of any kind is purely *inhibitory*. It originates nothing." [5.194] He also often spoke of abduction as if it were an insight or an animal instinct. But recall he gives this form to abduction;

> **[Ạ]** The surprising fact, C, is observed
> But if A were true, C would be a matter of course
> Hence, there is reason to suspect that A is true. [5.189]

Which is clearly that of a logical inference. There is the first problem. We have two simultaneous claims that abduction is a creative insight and abduction is a form of logical inference. And these simultaneous claims run counter to the intuition Popper was expressing. But Frankfurt quickly overlooks this problem and takes abduction as an *insight,* to mean nothing else but the inference in [Ạ]. That leads us into new troubles. For what does the conclusion of [Ạ] tell us? It tells us there is a reason to suppose **A** true, provided it accounts for the observed fact **C**. This result cannot obtain unless the conclusion is already in the premises; "..if **A** were true, **C** would be a matter of course." Which is to say, contrary to what Peirce claims, new ideas could not be derived *as a result* of an abduction. They must be at hand before the inference had taken place. In Frankfurt's terms;

> The conclusion of the abduction is not the hypothesis itself – as we had been led to believe by Peirce's remark that a hypothesis "results from" abductive inference – but a statement that there is evidence for the hypothesis."[46]

We must then, it seems, either drop abduction as a source of creativity, or drop the idea that it is a logical operation. Alternatively we can dig a bit deeper into Peirce and see if we can find

46 Frankfurt, "Peirce's Notion of Abduction." p.594.

some resolution of the dilemma. Peirce indeed does not have a singular account, so there might be some hope in adopting a different track. Sometimes he writes as if abduction were a process that culminates in the *adoption* of hypothesis, as opposed to being the *source* of new hypothesis. The following statement is the strongest support to such a view;

> [*Hypothesis*] is where we find some very curious circumstance, which would be explained by the supposition that it was a case of a certain general rule, and thereupon adopt that supposition. [2.64]

The idea will be familiar from the review of Hanson's remarks on the logic of discovery. Abduction then leads one to adopt hypothesis for further consideration, as a “working hypothesis.” While this account does not permit understanding abduction as a creative capacity, it at least does not run counter to the inferential form of [Ạ]. But Frankfurt refuses to admit even this possibility. He shows that [Ạ] cannot even lead to adoption of hypothesis because its premises assert that our hypothesis accounts for some facts. Since there are innumerable theories which account for any given set of facts, the condition is too weak to warrant an adoption. If we are not to go through the entire list of possibilities to land on one true theory, we must assume “the human mind's having such a power of guessing right that before very many hypotheses shall have been tried, intelligent guessing may be expected to lead us to the one which will support all tests, leaving the vast majority of possible hypotheses unexamined”[6.530] Therefore, says Frankfurt, contrary to what we are led to believe, it is not abduction, but “intelligent guessing” that leads us to adopt hypothesis.

To reiterate, Frankfurt's first conclusion is that abduction could not result in new ideas, because the new ideas must be part of the premises of an abductive argument before they could be inferred. His second conclusion is that abduction could not lead to adoption of hypothesis because its premises only say that a theory accounts the facts, but that is not sufficient to warrant the adoption of that theory. It is not the inference in [Ạ], but “intelligent guessing,” which leads us to adopt an hypothesis.

3.2 Rejoinders

I think both of Frankfurt's arguments are valid, but both his conclusions with respect to Peirce are wrong. Let me explicate the view I want to defend.

(a) Abduction can be understood as the source of new ideas.
(b) We can make (a) intelligible when we understand abduction as *intelligent guessing* or *instinct*.
(c) (a) and (b) do not preclude the understanding of abduction as a kind of inference, provided, we understand an inference in a less strict way than is commonly supposed.

If Frankfurt's arguments are right, then he must have made a faulty assumption, to arrive at the faulty conclusion. In the first case, the fault lies in the idea that [Ạ] is meant to act as a schema for generating new ideas. Douglas Anderson has nevertheless tried – I think incorrectly – to defend [Ạ] as the source of creativity. He states that Frankfurt confuses "logical with temporal priority." Placing the conclusion of the argument in the premises is meant to show that explanatory power of **A** constrains what may be concluded. So **A** in the conclusion is logically prior but not temporally prior. **A** could after all have been simultaneously arrived at as a premise and as a conclusion, so it does not need to have been invented before drawing a conclusion from it.

The problem with this line of argument is that it runs counter to Peirce's own clarification of [Ạ], when he said "Thus, **A** cannot be abductively inferred, or if you prefer the expression, cannot be abductively conjectured until its entire content is already present in the premiss"[5.188]. He was therefore quite certainly not making different assumptions about logical priority and temporal priority. I think it is possible, consequently, that Frankfurt has correctly pointed out an inconsistency in Peirce's account, which Anderson misunderstood. But I am inclined to think otherwise. I do not think Peirce intended [Ạ] it to work as an argument for making new propositions. If we look at Peirce's own statement about the matter, it is quite clear that means for [Ạ] to work as an argument for the *adoption* of hypothesis;

> Long before I first classed abduction as an inference it was recognized by logicians that the operation of adopting an explanatory hypothesis – which is just what abduction is – was subject to certain conditions. [5.188]

Which corresponds with Frankfurt's second reading. However, this rendition has the unintended consequence of depriving abduction the role of creativity, which I am defending by claiming (a). I want to claim that this is only a local problem, and that Peirce was making careless use of the term in this passage. My argument is that [A̤] is supposed to function as a *container*, which captures the steps completed *after* an abduction has been made. I believe this is best supported in the following remarks Peirce made about reasoning;

> Practically, when a man endeavors to state what the process of his thought has been, after the process has come to an end, he first asks himself to what conclusion he has come. That result he formulates in an assertion, which, we will assume, has some sort of likeness--I am inclined to think only a conventionalized one--with the attitude of his thought at the cessation of the motion. That having been ascertained, he next asks himself how he is justified in being so confident of it; and he proceeds to cast about for a sentence expressed in words which shall strike him as resembling some previous attitude of his thought, and which at the same time shall be logically related to the sentence representing his conclusion, in such a way that if the premiss-proposition be true, the conclusion-proposition necessarily or naturally would be true. That argument is a representation of the last part of his thought, so far as its logic goes, that is, that the conclusion would be true supposing the premiss is so." [2.27]

It is quite clear from this passage, that Peirce has a very different conception of an inference from that of Frankfurt. The written or stated form of an inference (at least for the amplitative variety), for Peirce, is not meant to syntactically represent the order of thoughts from whence the conclusion was attained – as Frankfurt supposes – but only assert the verdict of thought as a conclusion, and a statement of justification for that conclusion, as one of the premise. So the conclusion of [A], "that we are to suppose **A** as true," is justified by the premise "if **A** were

true, **C** would be true." The abductive form is not an algorithm to bring about new ideas. It simply acts as a container representing partially what conclusion was arrived at and why. Thus, an abduction covers much more than is apparent from [A], and it would be a mistake to identify it entirely with that form. Frankfurt does this, resulting in a misunderstanding about the basic idea, and resulting in a rejection of abduction as the source of new ideas. He could not have properly shown that [A] does not help us invent, because [A] was not *meant* to be a rule for invention. It is only a "representation of the last part of....thought."

Peirce has on occasion mentioned expressly, that an abduction applies more widely than any one of its precise externalized form might indicate;

> Abduction, in the sense I give the word, is any reasoning of a large class of which the provisional adoption of an explanatory hypothesis is the type. But it includes processes of thought which lead only to the suggestion of questions to be considered, and includes much besides. [4.541 fn]

Frankfurt's second argument, we have seen, rejects even the possibility of adopting an hypothesis from the form Peirce attributes to abduction. The erroneous assumption here I think is the equivalence he draws between "**C** would be a matter of course" assuming **A** and **A** *accounts* for **C**. Frankfurt seems to have in mind here a relation of weak entailment between **A** and **C**, but this might not be the correct way to read it. Abduction, Peirce points out repeatedly, results into an explanatory hypothesis. Here the hypothesis is required to *explain* **C**, and not just *account* for it. Furthermore, the explanation must appeal to instinct. We can then read "If **A** were true, **C** would be a matter of course" as "**C** is suggested most naturally, when bringing **A** before the mind." Clearly, when searching for an explanation of some phenomenon, an infinite number of hypothesis do not spring to mind, but only those that seem plausible. The frame of reference presupposed in the [A] is not logical aptness, but psychological aptness. Thus, we can escape Frankfurt's objection from underdetermination.

3.3 Logical Validity as the Psychology of Relations

So far I have shown that Frankfurt's two arguments are faulty, because firstly, he misidentifies the form of an abductive argument with the entire abductive process and secondly, because he misreads the premise of an abductive argument [Ạ]. (a) and (b) have the sort of intuitive appeal which does not require further discussion. I have yet to show independently that (c) is true, that abduction may be thought of as an inference.

We saw earlier how Popper is skeptical that logic or rules can ever bring new knowledge. It was the same intuition that gave force to Frankfurt's arguments. This intuition has its source in the identification of logic, inference and like concepts with *deductive* argument.[47] Conclusions of a strict syllogistic argument do not take us much further than what was known before the inference was made. No new ideas are derived from syntactic reconstitution on the basis of valid rules. But logic for Peirce was not the same as either Aristotelian syllogisms or the rules of the *Principia Mathematica*. He understood logic in the broadest sense, as a kind of methodology, a method of methods.[48] Contra Frankfurt – and the received view – he did not see logic as a closed system of arriving at necessary conclusions. This broadest sense encompasses everything properly identifiable as scientific reasoning. To the extent an insight can be characterized in some definite form – so we can actually see that there is a connection between our start in observation and conceptualization of that observation – he speaks of abduction with respect to logical relations. In fact, he believes that the reason abduction was not earlier identified as a distinct form of reasoning,[49] was because logicians refused to loosen the grip over the necessary relation between the propositions of logical argument;

> ….logicians generally almost always confine what they have to say about reasoning to its "correctness," by which they mean its leaving an absolute inability to doubt the truth

47 Deductive systems – logic and mathematics – consist of self warranting formal arguments. Conclusions are necessary by virtue of the *form* and *order* of ideas. If logic is to be identified with such systems, even inductions would not belong to logic.

48 So at least KT Fann and Douglas Anderson, “The Evolution of Peirce’s Concept of Abduction.” etc. seem to think.

49 For example, he writes, “The general body of logicians had also at all times come very near recognizing the trichotomy [abduction, deduction, induction]. They only failed to do so by having so narrow and formalistic a conception of inference....”[8.228]

> of a conclusion so long as the premisses are assumed to be true. But that amounts to confining their study to deduction. [8.383]

Under the influence of such a conception of logic, abduction could not count as reasoning at all. In response to the charge, that abduction defies valid rules of logic, Peirce wrote;

> An argument is none the less logical for being weak, provided it does not pretend to a strength that it does not possess.... An argument is fallacious only so far as it is mistakenly, though not illogically, inferred to have professed what it did not perform....if a fallacy involves nothing in its conclusion which was not in its premisses, that is nothing that was not in any previous knowledge that aided in suggesting it, then the forms of logic will invariably and necessarily enable us logically to account for it as due to a mistake arising from the use of a logical but weak argumentation. In most cases it is due to an abduction. [5.192]

Therefore, Peirce asks us to loosen the criteria for what counts as *logical*. Still, how does one understand something as being both an insight and an act of reasoning?[50] Let us assume we have settled the issue, of abduction as insight. What about reasoning? I take it that there is less hesitancy in pronouncing something as being both a perceptual judgment and an insight, than there is in pronouncing something as both reasoning and insight. Peirce starts at the same place, comparing abduction with a perceptual judgment. It resembles perception in every respect but this; a perception cannot be refuted – at the pain of misunderstanding what perceptual judgments are – but an abductive suggestion can be. If someone upon the application of greatest care and attention were to pronounce a patch of red surface blue, we would fail to see how he could arrive at that judgment, supposing we knew very well that he understood the meaning of “red” and “blue.” But a judgment suggested by an abductive instinct can be alternatively seen, and thus would be open to challenges a perceptual judgment

50 I am of course arguing that to understand abduction, we must understand aspects relating to both. A literal interpretation has also been proposed, mainly by Douglas Anderson, “The Evolution of Peirce’s Concept of Abduction.”, who mentions Davis, Rescher and W.M. Brown in support of his own conclusion.

is immune to.[51] In fact, he often equivocates between defining abduction as an *inference* or a *perceptual judgment* – on the matter of *instinct* he is consistent – but is more comfortable with classing it as an inference.[52] This argument has often been neglected, but it is central to understanding the dual aspect of abduction.[53] I will illustrate this immediately in the next section.

4. The Dual Nature of Abduction

We can understand abduction to have a dual character, when we understand the process as something like, but not the same as *simulationism.* I term my variant *quasi-simulation.* I want to propose that the form Peirce attributes to an abductive inference in [Ạ], implies we perform a *quasi-simulation,* over a problem situation, and use our conceptual resources to bring the facts into deductive conformity. Strictly speaking, abduction does not require that facts be specifically deductively derivable. The conditions in [Ạ] are loose enough so that it simply demands logical consistency between what we assume, and what we observe, provided that which we observe follows in some way from that which we stipulate. But, I want to focus on deductive aspect because it is simpler to do so, and because Peirce was concerned mainly with reasoning in the sciences. There, an explanation requires than the *explanandum* be deducible from the *explanans*.

The mental act of simulation has an imaginative quality. Thus, under the present reconstruction of – perhaps one among several kinds of – abductive reasoning, I am thinking of abduction as a sort of creative imagination. When looked at this way, I think we can show that both what we understand as an inference, and what we understand as instinct have a special role to play in the suggestion of new ideas. To sketch the idea I have in mind, we might

51 This is the same sort of argument philosophers invoke to mark out first person authority in the context of self knowledge.

52 And this is his statement of it “Any novice in logic may well be surprised at my calling a guess an inference. It is equally easy to define inference so as to exclude or include abduction. But all the objects of logical study have to be classified; and it is found that there is no other good class in which to put abduction but that of inferences.” Quote in M. Bergman & S. Paavola (Eds.), The Commens Dictionary: Peirce's Terms in His Own Words. New Edition. Retrieved from http://www.commens.org/dictionary/entry/quote-proper-treatment-hypotheses-preliminary-chapter-toward-examination-humes.

53 See (5.186) and (5.187) for discussion.

examine what the simulationist model is used for, and what it tells us.

Simulationism is employed as a folk psychological theory to account for the knowledge we acquire about other people's minds. Either by virtue of sharing the relevant cognitive architecture with others, or because of the intimate acquaintance with our own conscious mind, we can discover what others think and feel, how they may behave, what they believe etc. by simulating their mental states. We do this in the act of adopting their beliefs, attitudes, preferences etc. I know for example, that a certain friend specially likes ornate prose style in literature, so I would simulate her state of mind to discover if she is more delighted by Jane Austen or Ernst Hemingway. Combining the knowledge of her literary preference and the fact that Austen's prose has a more richer quality, I learn of her delight in possessing the book, just by running through my state of mind as if it were hers.

The analogy seems to break down however, between knowledge of other minds, and knowledge of the world in the sense that the simulationist model is meant to handle. Causal structures, cognitive architecture and capacities are shared between two minds in a way that we do not share with a worldly target. This makes us capable of predicting other minds and their states. On the other hand, the sequence of thoughts of the simulator do not always follow causal sequences in the world, which operates on independent principles. Our predictions in this domain tend to falter. We are often wrong in our judgments about what will happen. After all, knowledge about the world has not come to us easily, which has taken painstaking efforts from a community of people working over centuries. Thus, as Sofia Ortiz-Hinojosa remarks "[t]he analogue between mind and world is less optimal than the analogue between mind and mind. We thus lose a powerful predictive link between simulator and target."[54]

Still, while it is true that "we thus lose a powerful predictive link between simulator and target," in adopting this model, I do not think it is a defect at all for the present purpose. We must retain the link if we believe we really learn anything about the mind independent world. And we *must* have a model which predicts a *weak* link between the simulator and the target,

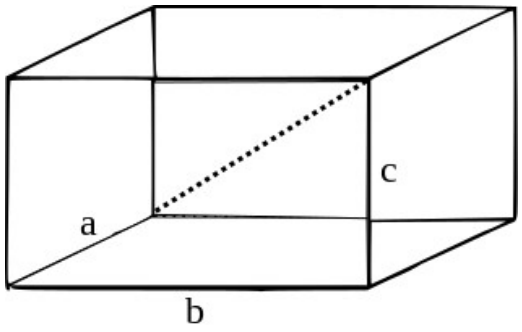
54 Sofia Ortiz-Hinojosa, "The Unique Utility of Imagination." p.11.

precisely because we know as a matter of fact that objective knowledge is not nearly as easily acquired as knowledge about other minds. Furthermore, it requires that the causal features of the objective world may be illuminated by the order of thoughts in our minds. Recall this was Peirce's point;

> It is certain that the only hope of retroductive reasoning ever reaching the truth is that there may be some natural tendency toward an agreement between the ideas which suggest themselves to the human mind and those which are concerned in the laws of nature. [1.80]

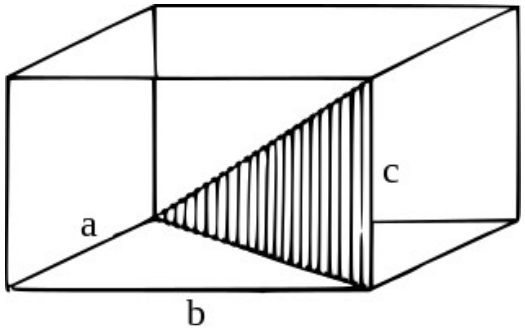
There are important limits to the model however. Simulationism is not about placing oneself in other people shoes. It is as if the target is itself within the mind, needing no special intervention on the part of the simulator to excite a thought. This condition is ill suited for mind independent target. We don't learn much without careful disciplined thinking. Science requires training and effort to master, unlike understanding another person. Moreover, we must consciously bring before our minds elements of a problem to extract explanations, which we expressly do not do on simulationist presuppositions.

What we do instead is what I am calling *quasi-simulation* which is summoned consciously, and employs cognitive structures developed in the course methodical investigation. In constructing a scientific hypothesis, we need from among conceptual resources, at least the knowledge of deductive validity. This is because it is only a deductive argument which strictly preserves truth in the inferential chain. Assuming for simplicities sake that we can make error free deductions at least up to a few steps, the following is what takes place; we encounter surprising facts (perhaps due to a failed inductive expectation). Then we run through the facts as if they had a place in a deductive chain, and search for the 'head' (clue which would fit the elements together) of the chain, from which the facts follow in some sequence. Thus, we *simulate* a deductive argument suggested from the data and the 'head' until we find a valid consequence.



Box 1

How do we find the length of the dotted line? We ask ourselves what would be needed to make the length of that line follow as a matter of conceptual necessity, assuming of course we are doing this rationally and methodically.. So we search out the 'head,' that brings all the different facts into accord. If we imagined the dotted line as the side of a triangle, we would bring the disparate facts into union, from which the sides could be deduced as in the figure below.



Box 2

Drawing a diagonal of the square with sides a,b enables us to “see” the sudden possibility of the application of the Pythagorean theorem. While the theorem itself may have been drawn from memory, it was from the probings of *instinct*, that its application to the problem was suggested. In other words, 'trace a line to see if a theorem applies' was a response from instinct or insight or guess making. It would be hard pressing to show that the insight followed as a result of some inference, because it is not clear how we could put together the elements of the figure to get “trace a line and check if such and such a system works” for a conclusion from

any known rule of inference. We thus account for the part instinct plays in an abduction. What about the inferential part? The answer is plain; we make an inference when we *judge* that a solution can be obtained by the application of the Pythagorean theorem. The inferential aspect of abduction also accounts for its defeasibility. We could make a mistake in the inference by incorrectly judging a relation between the figure and an idealization to explain the figure. This could be brought out by further inductive test.[55] Thus, to capture abduction in its entirety, we would have to recognize both the operation of an instinct and the operation of an inference. By the interaction of inferential representation and instinctive component, we can solve for problems like the above and account for new knowledge.

Another respect in which the quasi-simulationism exposes the workings of abduction is in the communication of an abductive conjecture. It consists in accounting how when lacking inductive support for a theory, we communicate the plausibility of a certain line of thinking. We do this by asking a person to bring themselves into our epistemic situation and then reconsider the situation in the light of a conjecture. For example, if someone were at a loss to solve the problem above (Box 1), we could 'hint' a solution by asking them draw a diagonal on the base of the box. The whole process would run through his mind once more – and assuming he has the prerequisite knowledge to solve the problem – he would see the line drawing as an "insight," to the solution.

Thus, performing an abduction is like performing a form of quasi-simulation, where the simulator runs through his mind a series of logical operations, and tries to alter the format of the knowledge already available to him, to derive something new. This process involves both the use inferences – or defeasible judgments about that is not immediately present to the senses – and flashes of insight – the realization that something new may be derived by assuming something about the problem. Furthermore, it is simulation also in the sense of communicating an abductive insight. When telling others how an idea was arrived at, or why some line of thinking is to be preferred, the communicator informs another of the requisite

55 There is also another respect in which abduction is defeasible, in that it ends up making false predictions about new facts. That however relates to the interface between mind and the world, as opposed being mind internal.

clues (knowledge which may be either present of absent to him) to rerun in his mind the same process from which an insight was arrived at.

Conclusion

The analysis Norwood Hanson brings to the logic of discovery, certainly does add depth to an aspect of the philosophy of science, but it misunderstands Peirce's original intent in proposing abduction. Hanson's account highlights the fact the there are sometimes conceptually compelling reasons in assenting to an hypothesis that depart from reasons we might have in their inductive support. But, his version of LOD does not touch upon the deeper theme concerning the architecture of the human mind, allowing us to comprehend the world – with abduction as the key capacity – let alone account for an aspect in the logic of scientific investigation Peirce did talk about. That aspect had to do with searching for ways to strike upon useful hypothesis faster than by random suggestions.

Harry Frankfurt's objections to abduction spring from the fact that Peirce used the term in many different ways. But in pressing the objections, Frankfurt overlooks a possible way to see abduction so that it can be understood as simultaneously an insight and an inference. I have tried to show this by understanding the process of imagining up an hypothesis as a kind of simulation over known inference rules, to search of a principle that would generate a valid argument.

Justification

Recent commentary mainly in epistemology and philosophy of science give central importance to explanatory considerations as an argumentative strategy. This is done by invoking an inference rule called *inference to the best explanation* (IBE). Commonly, *abduction* is used as a synonym for IBE.[56] If this were simply an instance of mislabeling, it would be trivial to compare them, as I propose to do here. There are overlaps between the sorts of issues abduction covers and the issues IBE is believed to resolve. For one thing, IBE is presumed to have a foundational role, just as abduction is. Both also concern inferences that are at once explanatory. The purpose of this chapter is to lay out the differences between the two notions, and reveal how IBE has shifted the original conception of abduction along a track that is inimical to its theoretical treatment at best and probably wrong at worst. I will also give my reasons for preferring Peirce's notion of abduction, against the sense in which that term is used today.

I want to propose broadly; (1) IBE belongs to what is called the context of justification, while abduction belongs to the context of discovery. As a result, the kind of justificatory burdens that falls on each of them are qualitatively different. (2) There is no notion of a “best explanation” in abduction, but it makes up the bedrock of IBE. The latter idea thereby exposes itself to a variety of attacks, which are difficult to fend off.

In the first section I lay down the general flavor of arguments on the justification of IBE to show that IBE has a very different role from abduction. Next, I sketch an account which makes it an essential principle of inference, and argue that it gives an unnatural picture of our inferential practices. Finally, I sketch a general argument against IBE that gives it a less fundamental role than is presumed and that also helps sharpen the role abduction is taken to play.

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56 Harman, “The Inference to the Best Explanation”; Ilkka Niiniluoto, “Truth-Seeking by Abduction”; Stathis Psillos, “Abduction: Between Conceptual Richness and Computational Complexity”; Lipton, *Inference to the Best Explanation*; Schurz, “Patterns of Abduction.” all of them use IBE in this way.

1. Inference to the Best Explanation

We seek explanations of surprising facts. There are often many different ways the same set of facts could be explained as we saw in chapter 1. In science and in common sense, when we have competing explanations, we try to eliminate each but one of them which seems true and satisfactory. One line of thinking characterizes this situation in terms of seeking for "best explanation" among a set of potential explanations. The inference we purportedly rely on to do this is called by its defenders *inference to the best explanation* (IBE), which is standardly spelled out as follows;

[IBE] Given hypothesis $H_1....H_n$ and evidence E, infer an H_i which best explains E.

While the preceding sketch implies that IBE functions as a rule to search for a true theory, not all philosophers employ the idea in this manner. Some philosophers advocate IBE as a rule for belief revision. On this view when faced with new facts that conflicts with ones prior set of beliefs, adjustments must be made on explanatory grounds. Or more simply, it asks us to update our beliefs based on what constitutes a better explanation. On other occasions, IBE is deployed to show that a given view of justification is truth bearing. Realists for example believe the success of sciences is best explained because realism is true.[57] Additionally, some philosophers think all amplitative inferences are nothing else but IBE's in disguise. Gilbert Harman and Stathis Psillos[58] for example argue in favor of such a foundational and ubiquitous role for IBE.

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57 Explanatory virtues of a theory are appealed to by the realists against anti-realists, in the philosophy of science literature. The camps are divided between philosophers who believe that knowledge of theory independent world is possible (realists) and those who believe that we can gain knowledge only of the empirical consequences of theoretical postulates but cannot be sure about the truth of the postulates themselves (anti-realists). For someone who maintains that science does not penetrate into the reality of such entities as subatomic particles, tectonic plates etc. but only to the consequences of assuming them, like emission of electromagnetic radiation and earthquakes respectively, IBE has little appeal, because he believes science strives for *empirical adequacy*, irrespectively of how much or how little a theory explains. Realists on the other hand regard the status of theoretical postulates to be on a par with their empirical consequences, a position they want to demonstrate by tying explanation with truth. On this view, when science succeeds in explaining the world, it succeeds in showing that all the assumptions beneath those explanations must be truth bearing. IBE is consequently, a realists defense.

58 Harman, "The Inference to the Best Explanation"; Stathis Psillos, "Abduction: Between Conceptual Richness and Computational Complexity."

Nevertheless such widespread optimism is not shared by everyone. Bas van Frassen[59] for example, is generally suspicious of IBE. He thinks IBE is defended by bad arguments and that a revision of ones belief on its basis conflicts with opinions we deem rational. Let us delve a bit deeper in to the nature of these debates, to get some idea of the kinds of justificatory problems IBE encounters.

The basic procedure is follows. Starting from an array of potential candidates H_1,…, H_n in [IBE], which are all of them equal on every parameter except explanatory power, we select that hypothesis which represents the greatest explanatory virtues.[60] Since explanatory power is deemed to have congruence with the truth, IBE is believed to be a reliable mechanism for the acquisition of knowledge. But the label presupposes that we are making not a comparative but an absolute judgment, as van Fraassen correctly points out. That is to say, IBE presupposes that our candidate explanations are the best explanations we have, and there is none that has not been conceived which could potentially trump them. But that assumption could be illicit. After all the best explanation may well be in the set we have not yet considered. So IBE can only be reliable on the additional assumption that we are somehow privileged in hitting upon the truth. To van Fraassen such a privilege seems *a priori* implausible. Some philosophers, who share van Fraassen's intuitions, concede the argument, and try to modify the rule by introducing notions such as approximate truth. Others, like Richard Boyd and Peter Lipton[61] do not think van Fraassen's arguments carry much force, and believe that a privilege of some sort must be in play.

Lipton argues for example that IBE is more accurately translated as an inference to the best of the available *potential* explanations, instead of the best *actual* explanation. The distinction marks the difference between an explanation that is not known to be true from an explanation we have reason to believe is true. It is supposed to rule out the kind of randomness in the selection of hypothesis. Also, Lipton thinks this account better incorporates our actual

59 van Fraassen, *Laws and Symmetry*. pp. 142-149.

60 Should the hypothesis differ in respects other than explanatory power, explanation would not be the sole criteria, and we could pick a hypothesis on other merits.

61 Boyd, “On the Current Status of the Issue of Scientific Realism”; Lipton, *Inference to the Best Explanation*.

inferential practices. After all we are not oracles, and often get our inferences the wrong way. But how could we arrive at such potential explanations, without assuming a certain kind of privilege? Lipton's solution is a kind of prefilter, which narrows the number of hypothesis we even consider, before making the final selection on the basis of IBE. The prefilter is a previous reliance on IBE itself to bring about our current state of knowledge. How does this come about? He constructs his picture on the analogy of Darwinian natural selection;

> The probability of a new complex organ, such as a wing, emerging all at once as a result of random mutation, is vanishingly small....How then does a complex organ evolve? The solution is an appeal to 'preadaptation.' Complex organs arose from simpler structures, and they were retained because they performed a useful though perhaps a different function....It later mutated into a more complex structure with a new function. In one sense, then, mutations are not random. Some complex structures have a much higher probability of occurring than others..."[62]

Likewise, the pool of potential explanations does not come to us by making inferences in the blank, but are determined by the prefiltering process that takes place through successive application of IBE over our background knowledge. The further we take this process, the closer we approximate toward the truth. These lines are reminiscent of Peirce's point about our natural propensity to arrive at the truth, guided by abduction. Yet, there are important differences. Peirce was not too concerned about privilege (he took it for granted), Lipton's alternative is meant to account for consequences of assuming such a privilege, without actually assuming them. But any account that draws upon background knowledge begs the question, as is easy to show; If successive application of IBE determines our present state of knowledge, there must have been a first in this chain. Either we then had a background store to draw from, or we did not. If we did, it must not have originated from an IBE, by assumption. We are then left stranded where we started. If we did not, we could not have made an inference to the best explanation in the first place. As a result, explaining the origin of potential explanations with IBE leaves more questions open than answers them.

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62 Lipton, *Inference to the Best Explanation*.

Other courses have been attempted to dodge the argument from the bad-lot, as the under-consideration argument is alternatively called, which I will not review here.[63] Irrespective of the success or failure of further attempts to fill holes in that argument, IBE faces more trouble from a different criticism by Van Fraassen. He asks to image a situation where we update our beliefs, in the face of new evidence, according to the probability calculus. Passing our hypothesis through test of empirical sufficiency, our IBE theorist would have us give extra points to those hypothesis which best explain. We would thus be led into imparting more weight to the most explanatory hypothesis in violation of the fundamental axioms of probability, and thereby commit a dutch book fallacy.[64] Since this is supposed a mark of irrationality, van Fraassen argues that dependence on IBE leads us into adopting incoherent beliefs.

In response, some IBE theorists look for a course that would make IBE compatible with Bayes rule,[65] while others try to curl out of van Fraassen's argument.[66] I wont labor the point here. The preceding account, though it be very selective, gives a general flavor of the nature of debate over IBE.

The crucial point in this debate is that it is possible to take sides on whether IBE should be adopted or rejected. This is due to the fact that IBE is conceived as a conscious and deliberative inference rule we have a choice to apply to a particular problem.[67] Moreover, IBE is applied when two or more theories all satisfy empirical criteria equally well; if they did not, we could make a choice on predictive rather than explanatory power. Abduction, on the other hand is not a process open to deliberative control to adopt or reject. It is not by choice, but by nature that we abduce. It could be argued however, that to point out an inference is beyond

63 See Douven, “Testing Inference to the Best Explanation.” for other defenses.

64 The fallacy of the *Dutch Book* shows “that if an agent's belief function violates the probability axioms, his betting rates make him susceptible to forming collections of bets on which he will lose no matter what happens.” Teller, “Conditionalization and Observation.”

65 Lipton, *Inference to the Best Explanation*; Weisberg, “Locating IBE in the Bayesian Framework.”

66 Douven, “Inference to the Best Explanation Made Coherent.”

67 Perhaps an exception to this characterization is Harman, who presumably does not think it is up to us to overrule the use of IBE, because, he contends, that all amplitative inferences are nothing else but IBE in various forms.

ones voluntary control does not show we are thereby justified in relying on it. The reliability of an inference of this type must be argued for independently. But, it is not hard to show that this argument is misplaced. Since abduction is conceived as – for brevities sake – the suggestion of an hypothesis by instinct, the demand for justification is really a demand to know whether we could be warranted in the acceptance of hypothesis, and not if we could be justified in advancing an hypothesis in the first place – whatever that could mean. But that sort of demand is no different from the usual demands on any amplitative inference, like the agreement of the hypothesis with the facts, cogency, explanatory value etc. So the recourse against demands for justification would be much like Hume's answer on the justification of induction; “It is true that non-demonstrative inferences are fallible, but we can't help but make them.” In any case, if knowledge is to be possible, it must be on that basis. Likewise, Peirce maintains, that the only justification for abduction is “that if we are ever to understand things at all, it must be in that way.” (5.145) This is one respect in which the two conceptions differ (excluding Harman for the time being), making the kinds of justificatory issues that have a bearing on abduction different in character from the those that have a bearing on IBE.

2. The alleged Ubiquity of IBE

I have mentioned that some philosophers think IBE has a more fundamental role than the somewhat limited accounts we have discussed above. Harman believes for example, that IBE's are foundational and ubiquitous.[68] A generalization, he says, where successful, really is an explanatory inference, hence “superfluous” under the label of “enumerative induction.” We can be warranted to make an enumerative induction, which is an inference of the form 'All A's are B's,' only when, given all the evidence, it is “simpler” and “more plausible” than the alternatives. We could imagine for example, that “someone is biasing the observed sample in order to make us think that all A's are B's.”[69] So “All A's are B's” happens to best explain instances of having observed A's which correspond with instances of having observed B's. Harman's point is that enumerative induction understood under that label is a less fancy way of saying “I saw this, and concluded that, because that explains this best.” Once this is

68 Harman, “The Inference to the Best Explanation.”

69 Gilbert H. Harman, “The Inference to the Best Explanation,” The Philosophical Review 74, no. 1 (January 1965). p. 94.

understood, the label falls apart, describing only poorly the process of explanation.

Similarly, Stathis Psillos takes “abduction” – by which he really means IBE – to be the basic form of amplitative reasoning.[70] His view, like Harman is essentially, that the application of an induction involves a comparison of hypotheses. And like Harman, Psillos thinks we accept inferences of the form “All A's are B's” because we believe it provides a better explanation of observed frequencies when contrasted with a slightly different inference which states “someone is biasing the sample to make me think all A's are B's.” (cite)

Let us call this view *Inductively Veiled Explanation* (IVE).[71] The view has two important facets;

(a) It does not recognize induction as an analytically separate process from explanation seeking.
(b) It demands that an inductive statement be adopted (and is in fact adopted) because it meets some criteria of an explanation more satisfactorily *in contrast* to its competitors. This is the condition for the *best explanation.*

Let us go over each of these points in turn.

2.1 Induction as an explanation

Under Peircian scheme of scientific reasoning, we have seen, induction clearly has a separate role from an explanatory inference. Thus either (a) or Peirce or both are on the wrong track. Again, I prefer Peirce's classification, and here I will sketch my reasons for the preference. Rather than analyze (a) as a basic viewpoint and try to locate the faults within it, we might state how Harman arrives at that conclusion. I think we find when we do this, that Harman's reasoning, though it be correct, applies to issues somewhat different from the ones he takes

70 Stathis Psillos, “Abduction: Between Conceptual Richness and Computational Complexity.”
71 An IVE theorist precisely speaking, is someone who takes all cases of what is called “enumerative induction” to be nothing else but explanations in disguise. IVE is to be understood here as a special version of IBE, and not as a different conception from IBE.

them to apply to.

Harman's skepticism with respect to enumerative induction stems from the fact that the theories such as that of “subatomic particles certainly does not seem to be describable as an instance of enumerative induction,”[72] to take one example. The reason we believe such events really occurred is because we think the data we have, explain their existence. Likewise, where we do apply enumerative induction, such as when we say “All Ravens are Black,” we do so on the belief that that induction constitutes the best explanation for having observed Black Ravens so far. So enumerative induction happens to be a class of statements that sometimes constitute the best explanation of certain phenomena. It may well be possible that they are really “complicated cases of enumerative induction,”[73] but he thinks the burden of proof shifts onto the shoulders of those who claim as much.

Now I have argued earlier (chapter 1) that induction alone could not enable the knowledge of certain features of the world; events like the Big Bang, entities like subatomic particles etc. Hence I concur entirely with at least part of Harman's point. It is not apparent how we can deduce events and entities which modern sciences inform us exist, on the basis of induction alone. But it does not follow from that argument that induction has no unique role of its own, and that it belongs to a more fundamental explanatory sort of inference. Keeping that aside, what does the IVE account really entail? It tells us that there are no basic conceptual distinctions between a description and an explanation. For every descriptive story we make, like “All Ravens are Black,” we can find an underlying explanation which tells us why the story seemed that way to us. If we said “All Ravens are Black” merely happens to be a way of re-describing what we observed and will have occasion to observe when we look at Ravens, we would be obscuring its true basis in an explanation, and only be making a surface statement.

But this is too artificial. We certainly do make conceptual distinctions of the sort Harman's

72 Harman, “The Inference to the Best Explanation.”
73 Ibid.

account erases. Not everything which fits observations explains. The Ptolemaic system of planetary motion fit observations of his time rather well, but even by the standards of science then, could not be called an explanation, as the Copernican theory could be[74] One could give several equivalent descriptions of the shape of a snowflake, but that description becomes an explanation *only* when we find that the flake must assume its shape as a consequence of some fundamental (physical) principles, such as the structure of the H_2O molecule.

This argument need not be a direct challenge to Harman because we mean something very specific by an explanation in these examples. Perhaps Harman does not mean to deny that the distinction is possible on *one* conception of explanation. His point may be that presupposing no particular account, we employ amplitative inferences to explain in the broadest possible meaning of explanation. But even this interpretation has serious defects, which I show below (section 3.2).

In fact, Peirce conceived of a description as the conclusion arrived at by means of an inductive argument and an explanation as something arrived at by means of an abduction. Sometimes they converge, but the neat separation is for a sharpened classification. We thereby not only emphasize the gap that exists between regular inductive generalization and explanatory statements (like Napoleons existence), but the significance of knowledge arrived at by means of an abduction as opposed to an induction. In science more than in any other enterprise, we search for regularities. But that is not enough. We also seek to answer why those regularities obtain. The former search does “not take any high rank among scientific discoveries” (2.637) in Peirce's words. It is in answering the *why* problems that the greatest of scientific contributions consist. By incorporating Kepler's laws of motion into his system, Newton showed not only that planets *do* revolve in ellipses but they do so because that outcome is necessary as a result of the specific forces acting between two masses. And we take Newtons law's to be more fundamental than Keplers. Moreover, it certainly seems to be the case that the observation of a certain regularity and a theory explaining it are processes quite different from one another.

74 Heilbron, *Physics*.

2.2 IVE and the best explanation

A caveat with respect to (b) is in order. A contrastive explanation *in itself* does not delimit some version of an explanation. That is, when one says that E_1 is better explanation of H than E_2, we don't learn how it is better. To answer how it is better, we must have some independent account specifying what it means to explain. Ergo, an IVE theorist rests no special adherence to any particular account of an explanation. His point is to emphasize the grounds for the choice of a theory, which he takes to be the overall explanatory value.

The fundamental role attributed to explanatory value by an IVE theorist leads to rather awkward consequences. I will review a counter argument to foundational role of explanation in the next section. Here I want to show that the contrastive cases of the IVE advocate shifts the focus from explanations that are of interest to a domain (sciences) to some other domain. As a result, it gives an unnatural view of the scientific practice.[75]

Let's first examine the basic claim, that concerns generalizations of the form “All A's are B's.” That inference is adopted, says the IVE advocate, because we believe it to be a better explanation than some other generalization. If this account is correct, then all expressions of regularities are nothing else but a restatement of the explanation schema “these facts apply, because their rivals for explanatory reasons, don't.” For example, I say, all objects fall near the earths surface at approximately 9.8 meters per second, because that explains my observing the fall of this ball at that rate better than that a daemon is deceiving me into thinking the rate of fall is 9.8 meters per second.

Now these are without doubt potential explanations. However, the point I want to stress is that just because some explanation may be pulled out for those observations, does not mean we admit those observations on its basis. By explaining that the rate of fall at 9.8 meters per second represents an actual state of affairs, rather than some kind of a deception, we answer the question “Am I observing the rate of fall for what it really is?,” but not the question “How can I explain the falling of the object at 9.8 meters per second?” The last question is far more

75 There remarks need not apply to non-scientific contexts, where theoretical goals are not very strict.

interesting from the point of view of a physics than the first. This is because – and it was Peirce's point as well – that specific rate of fall is a surprising fact demanding an explanation. Whether we are really observing the fall for what it is does not call for an explanation, but for a test. And even then, we do not test for evidence of a daemon lurking somewhere, but whether the measurements are accurate and so forth. The force of IVE, thus depends upon desensitizing context specificity of explanation on the one hand, and imposing an explanatory story to the admittance of an observation. We could have admitted the observation simply because it is what we expect, from habit, or from an induction, reiterating the point above.

What IVE does show is that we can can construct a contrast to evoke a seeming explanation of a fact from whatever foil we deem favorite. Thus, to answer why it rained today as opposed to yesterday, we could point to the unusually high humidity in the atmosphere today, which was absent yesterday.[76] It could have rained due to other causes, like reduced atmospheric pressure in nearby region, or because of the arrival of monsoon. Yet somehow, the first does not look out of order as an instance of a genuine explanation. Peter Lipton remarks that such contrastive explanations are sometimes interest relative.[77] Our choice of foil will be determined by what we are interested in explaining. So the IVE accounts shows at most that when we get interested in explaining if “All A's are in fact B's,” we can do so by planting the relevant foil “or are they C's”?

2.3 Justification and discovery

Psillos' makes an additional claim on top of the IVE, that an enumerative induction exhibits a Deductive Nomological (DN) structure.[78] Suppose we have the following argument;

76 A famous variant of the example often discussed in the literature is the explanation of the fact that Jones contracted Syphilis but not Smith by invocation of observation that only Jones had Paresis but not Smith. Here, like above, the difficulty is that very rarely does someone with Paresis ever contract Syphilis, but still Jones' Paresis is used to explain why he but not Smith got Syphilis. This illustration is used as a problem case to the account that takes an explanation to be the citing of causes. The trouble is, in spite of the causal confusion in both examples (Jones probably did contract Syphilis for reasons other than his Paresis, it probably did rain for reasons other than the changes in humidity) the explanation seems natural.

77 Lipton, *Inference to the Best Explanation*.

78 The DN model is an account of explanation which answers a question such as, why did X happen? To do this, it describes a set of conditions in the premises, where one of those conditions is a law, and deduces an observation from them. By thus relating the assumed conditions to the final observation, it

H {a is B; All A's are B; therefore a is A}

If we translate **H** as, “This bird is black; All Ravens are black; therefore this bird is a Raven.” Can we say **H** explains? Psillos argues that it does; He gives the following example in support of his claim;

> Suppose that we observe a black bird (a is B) and that, by instantiating schema H : {a is B; All A's are B; therefore a is A}, we infer that, given that All ravens are black, this bird is a raven (a is A). We have thereby answered the explanation-seeking question "Why is individual a B?" by hypothesising that a is A and by appealing to some sort of nomological connection between being A and being B.[79]

Thus, the fact that “this bird is a raven” is a potential explanation because we can expose the nomological connection between it and the fact that “this bird is black,” on the basis of the law, “All Ravens are Black.”

Either the account above concerns explanations, once they are had, or it concerns the search for explanations. If it is the former, we already have laws and observations at hand, so it leaves out the analysis of how we arrived at those particular laws at all. If it is the latter, then the story is told backwards. Psillos' assumes a law is available before an inductive generalization is made on its basis. For instance, we knew that “All Ravens are Black” which laid the basis for the nomic-expectability of the fact that “this bird is a Raven.” But if this is true, we were never required to make a search in the first place. Alternatively the contention is that the above is a search schema. Indeed this is what the phrase “potential explanation” seems to indicate. When we observe a black raven, we idealize the schema H, and ask which law fills the gap appropriately. The appropriate law then provides a complete explanation. But the DN model is

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shows us how the phenomenon was to be expected. The connection between the premises of a DN argument and the conclusion is described a nomological (lawful) connections, and the expectations based on those connections is called *nomic-expectability.*

79 Stathis Psillos, “Abduction: Between Conceptual Richness and Computational Complexity.”

not *sufficient* to account for that sort of explanation.[80] As Wesley Salmon[81] points out, not everything which corresponds to the DN model is a genuine explanation. We could have some law in our premises that is of no scientific interest, exactly like the statement "All Ravens are black." We could have a phony law for the DN structure, to derive an accurate conclusion which does no explaining whatsoever, like;

> $\mathbf{H_b}$ {The man takes contraceptives; All men taking contraceptives don't get pregnant; The man is not pregnant}[82]

The DN explanation is neutral to the directionality of deduction. It gives us no sense of what should count as a law, and what its consequence. For instance, using Psillos' example;

> ….we offer a potential explanation of the fact that the beer keg exploded in the basement by citing the law which connects the pressure of a liquid with its temperature and by appealing to a certain antecedent condition, viz., that the temperature of the beer in the keg rose rapidly. We therefore explain the explanandum by subsuming it under a law. (Stathis Psillos 2013, p.64)

While the example may offer an explanation, its converse would not. We cannot explain the temperature increase in the beer keg by deriving it from the fact that the keg exploded, even if all the conditions of a DN explanation are satisfied. Thus, the fact that we can explain any generalization does not show the generalization is a relevant explanation. And as $\mathbf{H_b}$ shows, not everything which conforms to the structure of an explanation in a DN sense is really an

80 We already depart here from garden variety explanations when we begin to talk about the DN model. Ordinary explanations cover a much larger range of conceptions than the DN structure was modeled to handle. Sometimes we understand explanations as asking about a *purpose*. For example, "Why doesn't Jane want to get married?" or "Why does the universe exist?" Answers to these questions are really demands for *purposes;* "because Jane believes she is non-committal," to answer the first question, or because "existence signifies a greater perfection than non-existence" to answer the second question. A different kind of explanation searches for causes; "What made the cask fall over?" Which could be answered by a causal explanation such as, "the waitress tipped it over." Neither of these kinds of explanations fit the DN model since neither require a law to draw upon in order to work.

81 Wesley Salmon, "Four Decades of Scientific Explanation."

82 Example due to Ibid. p.102.

explanation.

It is here that the core differences between abduction and IBE become more apparent. Nomic-expectibility derives from already having a theory at hand which relates the law and observation. Thus, Psillos' is really talking about how a scientific explanation works, once it is fully formed. Abduction on the other hand is that which initiates a scientific investigation. It is not an after story, but the beginnings of constructing a story. Abduction and IBE are both thought to govern theory selection, but in completely different contexts. When we talk about abduction "selecting" a theory, we are really referring to restrictions allowing the suggestion of a few hypothesis from infinitely many possible ones (chapter 1). IBE applies to *justifying* a theory selection at the final stage of scientific inquiry. For that to be possible, we must already have rival hypothesis, which were themselves obtained from an abductive inference. Once we do, IBE demands we select one of them on explanatory grounds. Imagine selecting one among (1), (2) and (3) in the Bongard puzzle in the last chapter, because that one serves to explain the evidence better. Thus, abduction belongs to discovery, while IBE belongs to justificatory context.

3. General Argument Against IBE

There are more general reasons to be suspicious of the foundational role attributed to IBE by Harman and Psillos. We might recall the form of an IBE;

[IBE] Given hypothesis $H_1....H_n$ and evidence E, infer an H_i which best explains E.

It is clear, that to lend substance to **[IBE]** some independent account of an "explanation" is needed. Those that are available, are notoriously unspecific or underdeveloped. Broadly then, two courses could be pursued. One course retains the ambiguity, proposing nothing definitive on what exactly an explanation amounts to, in which case IBE becomes only "a slogan" to borrow[83] terminology. To say we infer to the best explanation then would be like saying by

83 Lipton, *Inference to the Best Explanation*.

analogy, “[e]xplanation in physics is what physicists have done when they say Aha!”[84] Continuing with Weinberg, we can conclude that such a priori definitions are not very useful. IBE ends up becoming only a redescription of the problem of understanding the acquisition of knowledge.

A second course is to provide some framework of explanation. Day and Kincaid[85] identify broadly two categories into which suggested frameworks fall. Those that emphasize explanations as *unification* and those which appeal to *causal* factors. But this course does not hold much promise either. As the authors point out;

> ….if we understand explanation as unification, then either (1) IBE collapses into nothing more than coherence with the totality of belief and evidence, thus making IBE redundant and uninformative; or (2) IBE is a quite defeasible, limited inference strategy....On the other hand, if we understand explanation as the citing of causes, then IBE is likewise a defeasible, limited argument strategy: for explanatory power is a virtue that can be and often is trumped by other empirical virtues. Thus, on both main accounts of explanation, IBE is not a special, foundational inference strategy.[86]

I believe Day and Kincaid's conclusions not only attenuate the case for IBE, but actually bolster the case for abduction. This can be be argued for, I think, as follows. Absent some account of explanation, IBE is empty. But when we do put a definition couched in terms of an explanation, it collapses into whatever happens to be the sense of that definition. To continue Day and Kincaid's argument, suppose we define explanation as unification. Most philosophers understand unification as bringing about of coherence within our belief system.[87] To say that one theory is more unifying than another would then amount to saying the theory coheres with our prior beliefs better than others did. In the application of IBE, we then select that theory which is the best explanation, or one which brings about the greatest coherence. But then IBE

84 Weinberg, “Can Science Explain Everything?”
85 Day and Kincaid, “Putting Inference to the Best Explanation in Its Place.”
86 Ibid.
87 See (Day and Kincaid 1994, pp.274-275) for further comments.

faces new worries;

> ….if we equate explanation with unification, and unification with overall coherence, then IBE is really nothing other than belief revision based on overall coherence. Rather than fleshing out the idea of 'total evidence', IBE turns out to be just another name for the common practice of evaluating any particular belief in terms of its fit with what else one believes. IBE adds nothing of its own to the epistemic situation.[88]

Similarly, when we understand explanation as making causal claims, IBE reduces to a defeasible inference strategy. After all, we cite causes by scanning through the relevant background information, which may or may not rule in favor of a causal explanation given the *totality* of the information in the background. And we must make inferences based on total evidence, because rationality requires we revise our beliefs on the basis of everything we know about a case. Thus, IBE as inference to a cause could make for a bad argument and therefore become defeasible.

Other accounts don't fare much better, such as the one advanced by Paul Thagard.[89] He suggests that explanations are a measure of *concilience,* or the scope of explanation. The more comprehensive a theory, the greater its concilience. One theory explains “more,” than another, when it applies to high number of *classes of facts* than the other. As examples of distinguishable classes of facts, Thagard mentions reflection and refraction, that make up “more than one application of the wave theory of light.” Conversely “the distribution of species of finches and the distribution of tortoises on the Galapagos islands are not facts of different classes.”[90] But this analysis is not without its faults. Without the subtle notion of classes of facts, the argument is plainly wrong. Kepler's laws of planetary motions is applicable to the motion of an electron within an atom. Newton's laws cannot be similarly applied in this region where gravitational forces are minuscule. Do Kepler's laws thereby

88 Day and Kincaid, “Putting Inference to the Best Explanation in Its Place.”
89 Thagard, “The Best Explanation.”
90 Ibid.

explain more than Newton's laws?[91] Similarly, Aristotle's physics is meant to describe all motions in nature. Does Aristotle's physics explain more than classical mechanics, which is not applicable to the quantum world? No one really thinks so. The measure is not much better when we describe concilience in terms of the notion of classes of facts. This is because the precision we hoped to gain in defining an explanation is lost to the ambiguity of the very idea of classes of facts. For as Thagard points out, its application is "....distinguished by means of background knowledge and historical precedents shared by competing theories...,"[92] Two scientists working in the same scientific context would have a mutual understanding of what constitutes one class and what constitutes another, but there is no independent way of marking out the boundaries on classes. A different set of scientists might have different understanding of classes, with respect to the background information available to them, and a different history of precedents on competing theories. Thus, the argument against defining IBE in terms of concilience assumes the same general character of the argument against understanding IBE in terms of unification and the citing of causes.

What all of them show in common, continuing with Day and Kincaid, is the application of IBE becomes a contextual affair. This can happen in two respects; (1) When an IBE is warranted may vary, (2) each context may pose a different requirement of what constitutes a good (best) explanation. So IBE is much more limited than its theorist suppose it to be. When we work out an independent notion of explanation (assuming we can), couched in terms of simplicity, causality, concilience etc. IBE is delimited to those contexts where one or another of these concepts is applicable. When we don't, the label carries no information. Thus, IBE understood as a foundational rule is quite inimical to theoretical treatment.

The preceding considerations, I have already mentioned, is supportive to the conception of abduction. Like IBE, abduction is supposed as a foundational inference rule. But, unlike IBE, it does not demand a theory be selected solely on account of its explanatory virtues. The emphasis of the problem of abduction is not in some unique quality of explanation we might

91 Weinberg, "Can Science Explain Everything?"
92 Thagard, "The Best Explanation."

derive, but only that we find some account which will explain the surprising facts at hand. So, it seeks to account for the fact that sciences do develop successful theories. But this presupposes that the judgments about a the best explanation, or some other empirical criteria (say predictive power) are made by people, even if they cannot say exactly how they make them. We know that this process involves some sort of a combined influence of background knowledge and the available data. The exact mechanisms are unknown, but scientists do have guiding intuitions, otherwise it is impossible to account how the relevant judgments are made. Abduction essentially is another term for the inferences made based on those intuitions. These inferences could not be identified solely in terms of induction, we have seen from considerations in first chapter, nor could they be solely identified in terms of an IBE, as we have seen here.

4. Conclusion

Most of this chapter has been an attempt to present the qualitative differences in the nature of inferences that are mistakenly used synonymously. I have pointed them out first, by highlighting the disputes between the advocates and detractors of IBE, and showing how the burdens of justification which apply to IBE do not apply to abduction. This is mainly because the two concepts delimit very different processes. IBE is a rule one applies to an epistemic context as a justification for adopting a view, while abduction marks out no rule whatsoever, only to describe a suggestion one may accept or reject on subsequent consideration.

I have argued independently against the characterization of all inferences as essentially IBE, because that view gives an implausible picture of our inferential practices, and fails to mark out a distinction between descriptions and explanations which are fundamental to the sciences. I have also tried to sharpen the issues relating to abduction using Day and Kincaid's general arguments against the foundational role of IBE, simultaneously indicating why they do not affect the general conception of abduction.

Conclusion

The understanding into the nature of abduction, or explanatory reasoning, from its inception a century earlier, is approximately as advanced today, as it was when Peirce first proposed it. This work did not aim to take that limited understanding on the issues surrounding abduction much further, but was directed at clarifying them, and separating them from closely associated notions, with which abduction has been mistakenly identified. The hope has been at least to reorient the fashions in philosophical debates on reasoning, so as to bring a greater focus on the problems and themes which provoked the recognition of an abductive type of inference in the first place.

My contention is that the recognition of abduction would not be possible, if amplitative inferences were to be solely identified with inductions, and our philosophical preoccupations in scientific reasoning were to be concerned mainly with the *justification* of an induction. To this order, we examined a few use cases of amplitative inferences, and demonstrated the difficulty of capturing those inferences under any known conception of an induction. Consequently we were led to admit a different source for the origination of knowledge than an induction, which, borrowing from Peirce, we called an *abduction*, whatever its exact nature may be. From considerations of the impossibility of reducing an abduction into an induction, we rejected a view which proposed enumerative induction (or any of the many varieties of inductions) to account for how we formulate an hypothesis, or gain knowledge about the world. We then saw that an abduction properly speaking belongs to the conceptual or the early phase in development of ideas; to the stage of discovery. Assuming as little as we could about the context in which new ideas are proposed, we saw that Peirce defined abduction as nothing else but a *guess*. Induction, on the other hand, in Peirce's classification, was identified as the last step in the life of an hypothesis, the role of which is mainly to test whether our hypothetical guess holds universally.

Once the questions of classification of reasoning were settled, we then went on to the more

mainstream philosophical issues surrounding inductions, and considered the problem of the justification of induction in a new light. This involved reimagining the implications of Nelson Goodman's inductive paradoxes. The proposal was that the human mind is predisposed to strike upon certain kind of hypothesis, due mainly to what I called *canalization* effects, borrowing the term from P.F Waddington. That is to say, the human mind *prefers* organization of knowledge in a certain way, which do not cause for scientists the puzzling outcomes which Nelson Goodman brought out through his inductive paradoxes. Thus from the perspective of the mind, Goodman's paradoxes indicate a pre-ordering of hypothesis, posing no inductive problem, even if his remarks may be puzzling and of some interest from the logical point of view. The phenomenon of canalization was acknowledged by Peirce, and considered a prerequisite for knowledge, when he remarked about the prejudice of the human mind toward the truth. I have suggested that canalization as such need not imply we get anywhere near to truth. Consequently, the focus need not shift away from the fact about optimization of hypothesis, to justificatory questions about truth. The idea about canalization of theories is motivated by the relative quickness with which sciences have developed, and the modalities of this fact, regardless of ones epistemic assumptions about sciences in general.

In spite of having a more or less reasonable understanding of the place of abduction in very general terms, specific proposals of its nature are rather hard to come by. Peirce himself was agonizingly ambiguous in this respect. He sometimes spoke of abduction as if it were a logical reasoning, and sometimes as if it were a creative insight, and mostly as an animal instinct. To say that abduction is instinctive to man may be read as being compatible with either reasoning or insight. But when understood as reasoning – the preferred interpretations of Harry Frankfurt, Larry Laudan and Norwood Hanson – either we get a rather limited framework or an intractable problem. The limited framework was, as we have seen, proposed by Hanson. He understood Peirce to be engaging in a kind of logical investigation, where the proposal of a hypothesis is a purely conceptual affair, to be separated from the eventual acceptance of hypothesis, which might be done from empirical considerations. This analysis leaves little room to understand abduction as a principle which captures the very process of the formulation of hypothesis itself.

When we depart from Hanson's limited framework, we find that it is hard to make the idea of logical inference as a creative inference work. This is because, as Frankfurt points out, the form of an abductive argument already requires new ideas in the premises from which a conclusion may be obtained. So it could not be the inference which is doing the work of producing ideas, but some other source. I have proposed that on the matter of interpreting Peirce himself, Frankfurt's objections could be resolved by making a distinction between the process of discovery, and a logical analysis of that process. Hence, when Peirce used abduction to characterize the invention of hypothesis, abduction was there regarded as an *insight*, which may be taken to have, as an *ex post-facto* result of the insight, a logical form. That is to say, an abduction is nothing but an insight or a guess, which may be communicated as a logical inference when pressed for how the guess was arrived at.

I have proposed that this interpretation can be supported from two sides; from Peirce's (1) understanding of logic, and (2) his understanding of a perceptual judgment. A logical argument is consciously performed, and as a result, is prone to errors, either from false premises or mistakes of reasoning. But we do not exercise control over our perceptions, and thus it is hard to make sense of the idea of an error of perception. Moreover, controlled acts do not originate new ideas. But controlled action is the feature of logical reasoning, and involuntariness a feature of perceptual judgment. Thus, Peirce combines the elements of these distinct notions in conceptualizing an abduction, where it resembles a logical inference in its defeasibility and perceptual judgment in its creativity. It is hard to tell if any of this was really Peirce's intent, but at least the proposal satisfies a broader range of his comments, than either of the interpretations of Laudan, Hanson, or Frankfurt. I then tried to show how abduction can be understood as a kind of quasi-simulationism which demonstrates the place of both a logical type of argument and a creative insight in the proposal of a hypothesis.

The idea of an explanatory inference has found a resurgence in contemporary philosophical discussion under the guise of Inference to the Best Explanation. While this notion is alternatively referred to as abduction, it is quite different from the way Peirce understood the term. In one way, IBE is simply a reformulation of quest for the search for the best hypothesis.

In that sense, I have argued, the idea is uninformative. In a different way, IBE is grounds for selecting a hypothesis. On this reading, IBE is either unreliable as Bas van Fraassen argues, or defeasible – contrary to the claims of ubiquity of its advocates – as Day and Kincaid argue. The main theme of discussion in the IBE debates is whether the rule is a good or a bad proxy to approximate toward the truth. But that is decidedly a matter Peirce's abduction was not conceived to handle, and a problem which was of little interest to its original purpose. Peirce's abduction had for its purpose to capture how knowledge is acquired. IBE on the other hand concerns how that knowledge can be justified. In this respect, an IBE is more like an induction, in Peirce's classification of reasoning, than an abduction. Thus, I have argued that the two are quite distinct, and are not reducible or derivable from one another. From the point of view of the study into the nature of human cognition, Peirce's abduction is far more relevant and interesting.

But the most striking aspect of Peirce's abduction is his identification of it with animal instinct. In every other use he makes of the term, it is possible to situate abduction in the logical analysis of scientific reasoning. When understanding abduction as an instinct however, the emphasis shifts straight into the study of the human mind and cognitive capacities. It is within this region that the most significant issues as to the mysteries of abduction belong. My own analysis has only cursorily touched upon that theme, for the important reason, that a coherent picture of the mind which accommodates abductive process has been particularly hard to come by.[93] Also, because I am unsure how the program itself could be undertaken. Nevertheless, the framework has been adopted fruitfully in other sciences of the mind, notably linguistics.[94] But abduction in the context of acquiring scientific knowledge still remains a problem, elusive and obscure.

93 See for example, Fodor, *The Mind Doesn't Work That Way*. For an insightful discussion of computational theory of mind, and the difficulty of making abduction like inferences intelligible in its terms.

94 Chomsky, *Language and Mind*. pp.79-82.

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