Discovery practices in natural sciences: from analogy to preduction[1]

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Abstract

In this paper I face up to Charles Peirce's view that deduction can never originate any idea whatsoever. According to him new ideas come to science only by means of abduction. I affirm on my side that deductive reasoning can be extended to the context of discovery in mathematical physics. Thus a new form of discovery becomes recognizable. I call it *preduction* and I claim that it is the way by which many factual hypotheses, theoretical laws and theoretical models are anticipated in physics.

I point also to the fact that both, abduction and preduction, provide us with intrinsically fallible hypotheses. The fallibility of abductive inferences was already recognized by Peirce himself. The fallibility of preductive inferences follows directly from the fact that the available background, on which the preductive way of reasoning grounds, can be not true.

Since moreover many other creativity practices, like induction, analogy and serendipity, are worthy to be taken into consideration in the methodology of natural sciences, it becomes reasonable to assume the existence of a plurality of fallible strategies in scientific discovery.

Keywords: scientific discovery, induction, analogy, abduction, preduction

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1. Introduction

In this article I assume that, as far as the natural sciences are concerned, a clear-cut differentiation has to be done between *observational* and *theoretical* natural sciences. I take geophysics and palaeontology as typical examples of observational natural sciences, and mathematical physics as a paradigmatic example of theoretical natural science. The difference between both of them lies firstly in the fact that the observational sciences do not apply the methodology of hypothesis-testing in the same way as physical sciences do. Moreover I claim that observational sciences are mainly *abductive* sciences, although abduction is applied also in the domain of theoretical sciences.

Although the distinction between the *context of discovery* and the *context of justification* is a controversial issue in the contemporary philosophy of science, it is suitable for the purpose of this paper. The introduction of this distinction is usually attributed to Hans Reichenbach in his 1938 book, *Experience and Prediction*. Nevertheless it had already been anticipated by Karl Popper in §2, *Elimination of Psychologism*, of his *Logik der Forschung*, 1935. Indeed, Popper (1959, p. 31. My emphasis) affirms:

> *I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically*. As to the task of the logic of knowledge -in contradistinction to the psychology of knowledge- I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained.

Nonetheless Popper was not interested in the context of discovery. Indeed, in *op. cit., ibid.*, he claims that

> 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 scientific theory- may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned ... only with questions of *justification or validity*.

And in (1960, §29) he insists on that

> The question 'How did you first *find* your theory?' relates, as it were, to an entirely private matter, as opposed to the question 'How did you *test* your theory?' which alone is scientifically relevant.

No wonder that Larry Laudan (1980, p. 173) ironically affirmed that "Karl Popper wrote a book called the *Logic of Scientific Discovery*, which denies the existence of any referent for its title."

It is undeniable that it was Hans Reichenbach who introduced the terms of *context of discovery* and *context of justification*, the last one not being exclusively concerned with the investigation of the methods employed in testing hypotheses or theories[2]. Moreover, as a matter of fact Reichenbach was convinced of the relevance of induction for the context of discovery. Indeed, in (1936, p. 4) he claimed that, if the aim of scientific research, i.e. the establishment of laws of the natural events, is to be reached, it will be by means of the inductive method. And in (1938, p. 383) he maintained that "The methods of induction ... always will remain the genuine methods of scientific discovery." This shows that for Reichenbach the issues related to the context of discovery were very significant for the methodology of science. This is one of the reasons why in the next section I dwell upon the relevance of induction for scientific discovery.

2. The debate about the methodological role of induction in natural sciences

Historically there have been different positions on the problem of induction. Traditionally Aristotelian induction has been conceived as a *truth conservative and content ampliative inference*. It allowed to conceiving *inductive generalization* as a way of reasoning leading to the discovery of laws and principles of science. As such

1. Induction was criticized by Duns Scotus and by Thomas Aquinas in the 13th century, by Francis Bacon in the 17th century, and by David Hume in the 18th century.

2. Induction was declared inexistent by Karl Popper in the 20th century. Complementing Hume, Popper himself proclaimed that even inductive probability is impossible. The reasonableness of this anti-inductivism lies in the logical invalidity of induction as a method for the discovery of general truths. Popper's (1959, *Addendum*, 1972) negative solution of the logical and methodological problem of induction was: "*We can never rationally justify a theory*, that is to say, our belief in the truth of a theory, or in its being probably true."

Popper's (negative) solution of the logical-methodological problem of induction is shared, among others, by John Wisdom (1952, p. 49): "There is no rational machinery for passing from observational premises to an inductive generalization."

[2] What he precisely did mean is a question that is not my concern here. Thomas Nickles (1980, p. 11 ff.) discusses to a large extent the *true* meaning of Reichenbach's phrase *context of justification*.

And the logical positivist Victor Kraft (1950, p. 145) maintained also that there is no logically legitimate specific inductive inference from the particular to the general. Nonetheless it must be reminded that the anti-inductivist approach had been already anticipated both by Pierre Duhem (1906, pp. 327 and 334), according to whom it is impossible to build a theory by purely making use of the inductive method, and by Albert Einstein (1936, p. 307) who claimed that there is no inductive method which could lead to the fundamental concepts of physics.

A third position in the debate about induction is the inductive approach of Charles Peirce and Rudolf Carnap, who, contrary to Reichenbach, placed induction in the context of justification. Indeed, according to Peirce

> Induction is the experimental testing of a theory. (…) It sets out with a theory and it measures the degree of concordance of that theory with fact. (CP, 5.145)

> Induction consists in starting from a theory, deducing from it predictions of phenomena, and observing those phenomena in order to see *how nearly* they agree with the theory. (CP, 5.170)

Peirce's approach to induction anticipates by no means Carnap's inductive logic.

Fortunately the debate on induction has not taken only extreme positions. In the history of science there have been also pragmatic approaches to the use of induction in scientific discovery. For instance Isaac Newton (1687; 1952), whose *Rule* IV *of Reasoning in Philosophy*, affirms that

> *In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions.*

And in his *Optics*, 1706, Book III, Part I, p. 543, Newton claims that

> As in mathematics, so in natural philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions but such as are taken from experiments, or other certain truths. For hypotheses are not to be regarded in experimental philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions, yet it is the best way of arguing which the nature of things admits of, and may be looked upon as so much the stronger, by how much the induction is more general. And if no exception occur from phenomena, the conclusion may be pronounced generally. But if at any time afterwards any exception shall occur from experiments, it may then begin to be pro-

> nounced with such exceptions as occur. By this way of analysis we may proceed from compounds to ingredients, and from motions to the forces producing them; and, in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general. This is the method of analysis; and the synthesis consists in assuming the causes discovered, and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanations.

From these quotations it becomes clear that Newton maintains a pragmatic position on induction. Indeed inductive generalizations can admit of exceptions, but this is the best way of reasoning in the context of scientific discovery.

We find again this pragmatic view on induction among the members of the primitive Vienna Circle. In the programmatic document of the Vienna Circle, written by Carnap, Hahn and Neurath (1929, p. 24), we read that the method of induction can be applied, with sufficient reliability or not, there where it provides fruitful results. But that it is not any guarantee of certainty.

In a similar way Bertrand Russell (1949, pp. 69-70), despite his claiming that all scientific laws rest upon induction, he maintains, very closely to Francis Bacon, that induction by simple enumeration is a very dangerous form of argument, that it is not capable of giving certainty. Therefore Russell's inductive argument

- If a certain hypothesis *H* is true, then such and such facts *P* will be observed
- These facts *P* are observable
- Therefore *H* is probably true,

looks like an abductive argument, if we simply permute its premises, as we can see if we compare Russell's inductive argument with Peirce's abductive argument given in Section 3 below.

In spite of the existence of radical anti-inductivist positions or less radical ones, we find also in the history of the methodology of science many examples on behalf of the view that induction plays a fundamental role in scientific discovery. This is the case of Grossteste's method of agreement and difference, Duns Scotus method of agreement, William Ockham's method of difference, in the middle age, and John Stuart Mill's methods of agreement, of difference, joint method of agreement and difference, method of concomitant variations, and method of residues, in the 19th century.

In conclusion, although induction is logically illegitimate as a discovery procedure, when conceived of as a truth conservative and content ampliative inference, nevertheless it can be accepted as a practice or as a strategy leading to fallible scientific discoveries.

3. Abduction and its reception in the philosophy of science

Charles S. Peirce (CP, 5.145) is the first philosopher who, besides the two characteristic ways of reasoning in science, deduction and induction, proposed a new form: *abduction*. According to him, deduction "is the reasoning of mathematics, it starts from a hypothesis, the truth or falsity of which has nothing to do with the reasoning." On his side, "Induction is the experimental testing of a theory. (…) It sets out with a theory and it measures the degree of concordance of that theory with fact." The relationship between both ways of reasoning being the following: "deduction can draw a prediction which can be tested by induction" (CP, 5.171). Moreover induction shares with deduction that "It never can originate any idea whatever. No more can deduction".

Contrary to both ways of reasoning, induction and deduction, Peirce (CP, 2.777) affirms that "Presumption [Peirce's old word for *abduction*, A. R.] is the only kind of reasoning which supplies new ideas, the only kind which is, in this sense, synthetic." Insisting on the idea that only abduction has a creative and an explanatory character, Peirce (CP, 5.171) claims that "Abduction is the process of forming an explanatory hypothesis. It is the only logical operation which introduces any new idea; for induction does nothing but determine a value, and deduction merely evolves the necessary consequences of a pure hypothesis." Thus, "if we are ever to learn anything or to understand phenomena at all, it must be by abduction that this is to be brought about."

It is undoubtedly true that abduction provided for Peirce the logical form of scientific discovery, i.e. that abduction was for him the *ars inveniendi* in science. Indeed, as it is very well known, the logical scheme of any abductive argument is for Peirce (CP, 5.189) the following:

> 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.

In any case, in spite of the emphasis Peirce puts on abduction as a vehicle of scientific explanation and discovery, he (C.P. 2.777) was perfectly conscious of the fallibility of abductive inferences, i. e. of the fact that they do not necessarily lead to the truth: "The hypothesis which it problematically concludes is frequently utterly wrong itself, and even the method needs not ever lead to the truth; […] Its only justification is that its method is the only way in which there can be any hope of attaining a rational explanation." This last view reminds us of Newton's position on induction. In short, abduction was conceived by Peirce as the way of reasoning that allows scientists to learn fallibly from experience.

Despite the fact that abduction has been thought of by Peirce as an inferential form in scientific discovery, and that particularly Popper (1959, Preface) claimed that the central problem of epistemology is that of the growth of knowledge, the philosophy of science in the last century has been concerned to a large extent, and until very recently, with the discussion of induction, thus widely ignoring abduction, with the exception of Hanson (1958).

The word *abduction* is conspicuous by its absence in Popper's work. No wonder that whereas Vol. II of *Karl Popper. A Centenary Assessment*, edited by I. Jarvie, K. Milford and D. Miller, 2006, includes several critical discussions of different aspects of the induction problem it does not contain any single reference to abduction.

Even Peirce's name appears scarcely in Popper's work. Popper read for the first time about Peirce in 1952. In his "A Third Note on Degree of Corroboration or Confirmation", Popper (1959, Appendix *IX) simply mentions that Peirce has raised the question of the weight of evidence. And in *Conjectures and Refutations*, 1963, p. 226; p. 231 n. 17; p. 240, Peirce is briefly mentioned in contexts related to "the idea of truth as a *regulative principle*", to the "idea of approaching to truth", etc. In the *Postscript to L.S.D.*, volume 2, but only in the Preface of 1982, Popper mentions Peirce parenthetically in relation to the question of determinism. And also exclusively in the context of the determinism/indeterminism issue did Popper refer to Peirce in *Objective Knowledge*, 1972, Chapters VI and VIII. Peirce's name is also conspicuous by its absence in Popper's *Open Society and Its Enemies* and in *The Poverty of Historicism.*

Astonishingly Peirce counts, together with William Whewell and Claude Bernard (Medawar 1974) as an anticipator of Popper's view that science always begins with problems and hypotheses. And in the same line, Eugene Freeman and Henrik Skomilowski (1974) remind us that Peirce (CP, 1.68, 1.120) anticipated even Popper's falsification notion.

Not better fortune enjoyed Peirce among neo-positivists and logical empiricists. Indeed Carl Hempel ignores Peirce both in his popular *Philosophy of Natural Science*, 1966, and in his influential book *Aspects of Scientific Explanation*, 1965. Rudolf Carnap only mentions Peirce once in *Philosophical Foundations of Physics*, 1966, chapter 26, together with Dewey as members of the pragmatist stream. Finally, Ernest Nagel does, on the contrary, refer to Peirce several times in his *The Structure of Science*, 1961, but always in contexts like instrumentalism, statistics and operationalism.

Abduction was ignored as well in the post-positivist philosophy of science. Toulmin, Kuhn, Lakatos or Feyerabend do not take abduction into account, and both Peirce and abduction are ignored as well by Wolfgang Stegmüller in his *Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie*.

And the same applies to Helmut Seiffert and Gerard Radnitzky in *Handlexikon zur Wissenschaftstheorie*, etc.

Nonetheless, the idea of something like abductive reasoning had been accepted long before Peirce explicitly formulated it. Indeed, John Herschel in his 1830 *Preliminary Discourse on Natural Philosophy* distinguished between a context of justification and a context of discovery (John Losee 1980, pp. 115-118): By way of *hypotheses* pointing to some relationships between facts or previously unconnected laws it is possible to postulate new generalizations in science. More clearly an anticipation of the concept of abduction is William Whewell's conception of induction as a colligation of facts. Indeed, as Losee (1980, p. 124) affirms "Whewell's examination of the history of science convinced him that the colligation of facts is achieved through the creative insight, and not by means of the application of specific inductive rules." Since a preferred example of Whewell's triumph of induction is Kepler's abductive way of reasoning, it seems reasonable to claim that Whewell's induction is Peircean abduction.

Finally, without mentioning explicitly the word *abduction*, in contemporary philosophy Victor Kraft (1950, p. 145) referred to it, as he claimed that on the basis of observations we make some suppositions, which do not derive logically from the observations, but which we creatively add to them: we establish *hypotheses*. They have provisional validity, since they can in principle be refuted by new observations.

Only at the end of the seventies and with the beginning of the eighties of the last century abduction became to be known among the philosophers of science. Indeed, Gilbert Harman (1965, pp. 88-89) proclaimed for the first time abduction as inference to the best explanation, as opposed to enumerative induction: "'The inference to the best explanation' corresponds approximately to what others have called 'abduction', 'the method of hypothesis', 'hypothetic inference', 'the method of elimination', 'eliminative induction', and 'theoretical inference'. […] In making this inference one infers, from the fact that a certain hypothesis would explain the evidence, to the truth of that hypothesis … Thus one infers, from the premise that a given hypothesis would provide a 'better' explanation for the evidence than would any other hypothesis, to the conclusion that the given hypothesis is true." In general all inductive inferences are abductions, i.e. inferences to the best explanation according to Harman (1968, p. 165): "Since the reasoner must infer that one explanation is better than competing explanations, I say he makes an *inference to the best explanation*. In my view, all inductive inference takes this form. Even when a person infers a generalization of the evidence, his inference is good only to the extent that the generalization offers (or is entailed by) a better explanation of the evidence than competing hypotheses."

Ten years later the identification of abduction with inference to the best explanation had been already accepted in the philosophy of science, as following text by Paul R. Thagard (1978, p. 77) shows:

> The phrase 'inference to the best explanation' is relatively new, but the idea is old. Inference to scientific hypotheses on the basis of what they explain was discussed by such nineteenth-century thinkers as William Whewell and C. S. Peirce, ... To put it briefly, inference to the best explanation consists in accepting a hypothesis on the grounds that it provides a better explanation of the evidence than is provided by alternative hypotheses. We *argue* for a hypothesis or theory by arguing that it is the best explanation of the evidence.

Thomas Nickles (1980) discusses *abduction* in Section IV: “Hanson and his Critics” of his “Introductory Essay: Scientific Discovery and the Future of Philosophy of Science”. And abduction is mentioned in passing, in relation to Harman’s inference to the best explanation by Peter Achinstein (1980, pp. 126-127). Also Martin V. Curd (1980, section III) analyses briefly the approach to the logic of discovery in terms of abductive inference, and Gary Gutting (1980, pp. 225-227) claims that the logic of discovery is Peirce’s abduction. Only Larry Laudan (1980, p. 174) shows a critical position as he claims that “the abductive method belongs most naturally to neither discovery nor justification, but rather to pursuit.”

Finally, Hilary Putnam (1981, pp. 197-198) defended the scientific character of Darwinism due to the use of abduction or inference to the best explanation: “we accept the Darwinian theory of evolution by natural selection as what Peirce called an ‘abduction’, or what has recently been called an inference to the best explanation.” i.e. “because it provides a *plausible explanation* of an enormous amount of data.”

With the increasing meta-scientific use of inference to the best explanation on behalf of scientific realism, abduction became more and more accepted as an argument in epistemology, associated with Boyd-Putnam’s no-miracle argument.

With the turn of the century, abduction has been given a prominent role in new fields like artificial intelligence, logical programming, knowledge acquisition and related matters, where abduction is being widely applied. This is the case of Josephson, Magnani, Flach and Kakas, Aliseda, etc.

Astonishingly, philosophers of science do not seem to have realized yet to what extent natural sciences use abduction both as a means of discovery and of explanation.

In sections 5. and 6. I am going to discuss the relevance of abduction and preduction as reasoning strategies for the context of scientific discovery in the natural sciences. But before doing it I want to dwell upon a current practice of scientific reasoning that frequently leads to promising results. I mean *analogy*.

4. The heuristic role of analogy in science

Analogy is one of the most fruitful forms of reasoning in scientific creativity. Very frequently analogy has been conceived as a form of inductive inference. Nevertheless I want to emphasize here that the use of analogy in science is by far much more promising than the application of induction. First of all the recognition of analogy between the so-called *source* and *target* subjects is the basis for the establishment of metaphors in science. But I am not interested to discuss here the very interesting question of whether or not scientific metaphors have cognitive value, an issue that is the subject nowadays of very serious epistemological discussions. Since I have faced this question elsewhere (Rivadulla 2006*a*), I want to analyse in the following how analogy can become a guide in scientific creativity, an issue that I did not enter to in the above mentioned paper.

By means of some examples taken from theoretical physics I will show the heuristic fertility and the variety of forms of analogical reasoning in scientific discovery: From the interpretation of novel phenomena and the suggestion of new explanatory hypotheses to its application to the meta-scientific level.

4.1. The proposal of the nuclear shell model on the analogy of the shell model in atomic physics

The first example I am going to present here is one of many cases in science where the deep relationships between theoretical models, analogies and metaphors become evident. But I will only insist on the heuristic fertility of analogical reasoning for the proposal of new theoretical models, i. e. on the role of analogy in scientific creativity.

In the forties of the last century nuclear physicists were confronted with the issue of accounting for the extraordinary stability of the atom nuclei of those chemical elements whose total nucleon numbers are 2, 8, 20, 28, 50, 82, 126. On analogy of the atomic numbers of noble gases they were called *nuclear magic numbers*.

The existence of the *atomic magic numbers*: 2, 10, 18, 36, 54, 86 was given a theoretical explanation by assuming that electrons are disposed in shells (electrons with identical principal quantum number *n*) and sub-shells (electrons with the same quantum numbers *n* and *l*) around the nucleus. This was possible because the explanation grounded on the hypothesis that each electron moves *independently from the others* in a central field. The corresponding theoretical atomic model was called *atomic central field shell model* (Rivadulla 2006*b*, § 3.2).

In nuclear physics there was available also since the thirties a theoretical model, the so-called *liquid drop model* of the nucleus, a metaphorical name suggested by the analogies of the properties of the nucleus model proposed by Bohr and Frenkel with macroscopic drops of incompressible liquids. The problem with the *liquid drop model* was that it was incapable of providing a theoretical explanation of the existence of *nuclear magic numbers*.

According to the *nuclear liquid drop model* it was impossible for the nucleons inside the nuclei to move independently from each other. Nonetheless some physicists, in spite of the theoretical difficulties they faced, conjectured that if the shell model of atomic physics, where electrons move independently from each other in a central field, had proven very successful in providing a theoretical explanation for the *atomic magic numbers*, a shell model of the nucleus might also be successful in providing a theoretical explanation for the existence of *nuclear magic numbers*. Maria Goeppert-Mayer and Hans Jensen succeeded to propose the *shell model of the nucleus* in analogy of the *atomic shell model* (Rivadulla 2006*b*, § 3.2).

This use of analogy reasoning in theoretical physics resembles inference to the best explanation, i. e. abduction. Indeed, this example shows that analogy reasoning, besides the possibility of suggesting scientific metaphors, also guides the scientific creativity in the proposal of new theoretical models capable of providing theoretical explanations of open problems.

4.2. The use of analogy in the interpretation of the wave model for electrons

One of the most decisive experiments in the development of quantum mechanics is the *double slit experiment*. This experiment amazed physicists because of the astonishing similarities of the behaviour of light and electron beams.

Contradicting the expectations derived from a corpuscle model of light, light arrays passing through a double slit show a characteristic interference pattern formed by brilliant and dark lines, when projected on a screen. Reasoning by abduction, the best explanation of this phenomenon was that light behaves like waves. Indeed it is a very common experience that waves in water reproduce exactly an interference pattern. This allows to assuming that the best explanation of the behaviour of light arrays is a wave model of light. This is a first example of analogy reasoning in this experiment.

But if we now repeat the experiment with electrons, which are material particles, it is also evident, to our astonishment, that the impact of the electrons that pass through a double slit shows an interference pattern as well, i. e. *electrons must also behave as waves*.

On the analogy of the behaviour of light arrays we conclude that the best explanation of the electrons behaviour is that they are like waves. Any abductive inference grounding on analogy is the basis of the postulation of the wave model for electrons, and, in general, for material quantum particles.

As this experiment shows, analogy serves to provide a theoretical interpretation of a surprising experimental outcome, and it is the basis of an abductive inference to the best explanation of the experimental result.

4.3. The classical limit of quantum mechanics on analogy of the Newtonian limit of relativity theory

Albert Einstein was the first scientist who pointed to the fact that Newtonian mechanics constitutes a limiting case of relativity theory. The impact of this viewpoint in the philosophy of science provoked a very intensive dispute between those philosophers, like Karl Popper, who agreed with Einstein, and others, like Kuhn and Feyerabend, who, because of the alleged existence of incommensurability between both theories, disagreed with Einstein. Since this philosophical dispute does not concern us here (Rivadulla 2004*a*), we are simply going to assume as a matter of fact, that in cases of weak gravitation fields and low velocities Newton's gravitational theory obtains as a limiting case of relativity theory, both special and general.

To the most relevant physicists the Newtonian limit of relativity theory should not be a unique case in theoretical physics. Niels Bohr, Paul Wigner, Hermann Weyl, Albert Messiah or Willis Lamb are examples of quantum physicists concerned with the explanation of the circumstances under which classical physics could be shown to constitute a limiting case of quantum mechanics as well. Quantum physicists succeeded to show the wanted classical limit of quantum physics either by the explicit formulation of a *correspondence principle*, according to which quantum physics becomes classical when the values of quantum number n are big, or by mathematical derivation from Schrödinger's equation, when $\hbar \rightarrow 0$, of the Hamilton-Jacobi equation of classical mechanics.

The meta-scientific relevance of this example shows that analogy reasoning works not only at the object level, as the two preceding cases show, but also at the meta-scientific level. And the conclusion is that analogy reasoning constitutes an important vehicle of scientific creativity.

5. Abductive reasoning in the observational-minded approach to scientific discovery

Abduction is the way of reasoning by means of which some hypotheses or models are proposed as the most reasonable tentative explanations of the available empirical data. Abduction has to sides: It is a vehicle of discovery or creativity, and it is the way by which 'explanation' is achieved in observational sciences. Of course, since novel facts may appear in detriment of the conjectured explanations, it is reasonable to revise or even to replace them by new ones that should be compatible both with the old and the new data. Surprising facts, abduction by hypotheses elimination and selection of the most reasonable one, revision of abduced hypotheses by novel facts, etc., are common facts currently happening in the methodology of observational natural sciences. Let me illustrate this point with a brief reference in palaeoanthropology to the recent discovery by Manuel Domínguez Rodrigo of an archaic *Homo sapiens* fossil, the frontal bone *EH06*, in Lake Eyasi, Tanzania. According to Domínguez Rodrigo (2008, p. 903), "EH06 shows an interesting continuation of the primitive features shown in Eyasi 1 [three hominid skulls discovered in the early 20th century in Lake Eyasi as well, A.R.] in a period for which other areas have yielded, from a morphological standpoint, substantially more modern-looking specimens". Indeed, these modern-looking features, affirms Domínguez Rodrigo, *op. cit., ibid.*, "can be observed on the Omo and Herto crania (Ethiopia) and the Ngaloba (Tanzania), Jebel Irhoud (Morocco) and Florisbad (South Africa) crania, dated between 265,000 and 120,000 years ago." Thus the discovery of *EH06* contributes to the revision of the hypothesis of the basic morphological homogeneity of our own species.

As an example of abductive inference, also in palaeoanthropology, let me present with some extension following *inference to the best explanation*:

1. The discovery in 1995 at Sierra de Atapuerca, Spain, of a piece of the facial skeleton of a young man provided a big surprise. It corresponded to a hominid fossil, known since then as the *Gran Dolina Boy*. It did not show either the primitive aspect of *Homo ergaster* or the derived features of *Homo heidelbergensis*, which were inherited by *Homo neanderthalensis*. Surprisingly, his face was identical to that of *Homo sapiens* (Bermúdez de Castro 2002, p. 29). Previous discoveries from 1994 had dated the fossil register associated to the *Gran Dolina Boy* with an antiquity of nearly 800 ky. According to the then accepted hypothesis the oldest hominid in Europe, *H. heidelbergensis*, an alleged common ancestor of *H. neanderthalensis* and *H. sapiens*, was attributed an antiquity of nearly 500 ky only. Thus the *Gran Dolina Boy* was considerably older than *H. heidelbergensis.*

2. Taking into account that hominid brains stop growing in the eleventh year of age, and that the skull capacity of the *Gran Dolina Boy* of about 1000cc was considerably bigger than that of the three best preserved known skulls of *H. Ergaster* (Arsuaga 2006, pp. 300-301), this provided an excellent argument to exclude him from being *H. Ergaster*.

3. Since it was also excluded that the *Gran Dolina Boy* were *Homo erectus* -an evolutionary line derived from *H. Ergaster*- which besides in the actual Israel (with

an antiquity of 1,4 My.) and in Dmanisi, Georgia, (1,8 My.), is mainly distributed in Asia, the final conclusion could only be the following (Bermúdez de Castro 2002, pp. 33-35):

4. The *Gran Dolina Boy* belonged to a new species, called *Homo antecessor*, "a common predecessor of both the evolutionary line that in Europe leaded to *H. neanderthalensis* and of the evolutionary line that in Africa leaded to the modern populations (*H. sapiens*)."

5. The hypothesis of the existence of *H. antecessor* has been supported by the discovery in 2007 of the mandible ATE9-1 at the Sima del Elefante, Atapuerca, Spain (Carbonell 2008). This mandible has been dated to the Early Pleistocene (circa 1.2 My.), and it proves that the human occupation in Europe is much older than it was assumed.

Palaeoanthropology is a paradigmatic example of an abductive science. Since the recognition in 1861 of *Homo neanderthalensis* as a different species of *Homo sapiens* until the discovery in 2001 of *Sahelanthropus tchadensis*, all hominid genera have been postulated by successive abductions as the best explanations of the corresponding available fossil registers.

The situation is similar in Earth sciences. A chain of abductions leads from Eduard Suess's first abductive explanation of the distribution of the glossopteris fossil register in the austral hemisphere to the modern plate tectonics model, the convective hypotheses, etc., passing through Lothar Wegener's continental drift hypothesis. To illustrate the explanatory strength of abduced hypotheses, let me quote Naomi Oreskes's evaluation (2001, p. 3. My emphasis) of Lothar Wegener's continental drift hypothesis: "The paleontological patterns and jigsaw-puzzle fit *could be explained* if the continents had migrated across the earth's surface, sometimes joining together, sometimes breaking apart. (…) Continental drift *would also explain* paleoclimatic change, as continents drifted through different climate zones and ocean circulation was altered by the changing distribution of land and sea, while the interaction of rifting land masses provided a mechanism for the origins of mountains, volcanoes, and earthquakes." Wegener's continental drift hypothesis presented in his seminal work *The Origin of the Continents and Oceans*, 1915, 1929, was abduced on the grounds of a lot of geodetic, geophysical, geological, palaeontological and palaeoclimatical data. (Rivadulla 2008)

As to other sciences the relevance of abductive reasoning is overwhelming. Indeed, in ancient astronomy all geometrical models since Eudoxus were abductive hypotheses proposed as the best explanations of the planetary movements. Johannes Kepler contribution had been considered by Peirce, and later by Hanson, as a paradigmatic example of abductive reasoning. The same did Whewell. Abductive was as well the procedure by which such entities like phlogiston, caloric and ether were excluded from the theoretical vocabulary of natural sciences. And abductive is the way of reasoning by which the principles of classical thermodynamics, and many others as well, have been postulated in physics (Rivadulla 2009).

It looks strange that philosophers of science have been so reluctant, until very recently, to accept abduction. Undoubtedly Popper's anathema against the context of discovery has been so grave, that it has prevented contemporary methodology of science from recognizing the relevance of abduction for scientific creativity, a discovery and explanatory practice that natural scientists have been applying for centuries in an extensive and systematic natural way.

6. Preductive reasoning in the theoretical-minded approach to scientific discovery

When thinking about the implementation of the hypothetic-deductive method philosophers of science and philosophically minded scientists usually mean either the process by which scientific hypotheses are tested empirically, or the process by which scientific explanation is achieved. Thus the application domain of the hypothetic-deductive method is restricted either to the context of justification or to the context of explanation. The first one allegedly refers to the *method of science*, whereas the second one usually means Popper-Hempel's deductive-nomological explanation model (Rivadulla 2004, p. 70).

As to the first one, many representatives of observational natural sciences, like geology, have rejoined that the deductive testing of hypotheses does not necessarily characterize their disciplines. The reason is that these sciences are more observational than experimental. Indeed, in the observational natural sciences, like evolutionary biology, palaeontology and earth sciences the context of discovery is preeminent over the context of justification. This does not mean that in these sciences hypotheses are not tested. They are, of course. But hypothesis testing here does not resemble to the characteristic Popperian test. When a test is possible it usually amounts to extending the observational basis of the hypothesis, i. e. novel facts contribute to the acceptance or to the rejection of the current hypothesis, as show the discoveries in 2007 of the mandible of *H. antecessor* and the frontal bone EH06 of *H. sapiens*. An extended and almost continuously changing empirical basis guarantees the reasonable acceptance of previously abduced hypotheses or it recommends the replacement of already accepted hypotheses.

Contemporary philosophers of science have committed two important mistakes in the methodology of science. The first one, already discussed, was the disregard of the important role abduction plays as a vehicle of discovery or creativity in science. The other one was that they have not taken into account the relevance of deductive reasoning in the context of scientific discovery. Indeed, the "disclaiming any power to explain how hypotheses come into being" had been considered at least by Peter Medawar (1974, p. 289) as a sign of an alleged weakness of the hypothetic-deductive method. It will be my duty in the following to show that this is not true, although I myself wonder that philosophers of science have not yet realized the relevance of the deductive way of reasoning for the context of scientific discovery.

I conceive *preduction* as the way of reasoning that consists in the mathematical combination and manipulation of previously accepted results *of the whole of physics*, in order to deduce new hypotheses, provided the undertaken substitutions and combinations are compatible with *dimensional analysis.*

The typical and most simple example I usually take to illustrate the implementation of preductive reasoning is Einstein's preduction of the dual character of photons by *combination of two previously available results*: $E=cp$ from special relativity, and $E=hv$ from Planck's quantum physics.

Since in Rivadulla (2008 and 2009) I illustrate the notion of preduction with examples like the preduction of the critical density of the Universe, Schrödinger's equation, the temperature in the Sun's surface, Compton's wavelength, and the application of the Doppler effect to electromagnetic radiation, two more examples will be enough to better understanding the preductive way of reasoning in mathematical physics.

6.1. The theoretical preduction of Planck's radiation law

The description of the preduction process of Planck's *radiation law* is tantamount as the telling the history of its postulation. Very briefly I summarize Planck's preduction as follows:

In the first step *thermodynamics* played a decisive role. Wilhelm Wien had published in 1894, in an article entitled "Temperatur und Entropie der Strahlung", a first law according to which the energy density of black bodies depends on an unknown function of its frequency and temperature. When later on Wien succeeded in finding the mathematical form of this function and that the whole expression of the energy density depended on the third power of the frequency, Wien became convinced that thermodynamics had exhausted all its explanatory possibilities.

In the second step Max Planck was the protagonist. He combined *electrodynamics* and *thermodynamics* in order to obtain an expression of the energy density of black bodies, which he presented before the German Society of Physics in Berlin on October 1900. His paper was entitled "An improvement of Wien's spectral equation". Planck's radiation law was immediately compared with previously available empirical data and was recognized as very successful.

The third step consisted in a refinement of this law. Planck was not satisfied with the way he reached it. It had been the result of a *lucky conjecture*. In order to give to it a theoretical justification Planck decided to combine *statistical mechanics* with *thermodynamics.* The commitment to make his expression compatible with Wien's law provided finally the current formula of Planck's black body radiation law.

6.2. The theoretical preduction of Bohr's atomic planetary model

In order to model the physical condition for an electron to move around the atom nucleus in a circular orbit in a stationary state, Bohr combined in a first step *electrodynamics* with *classical mechanics*. The result was the *mechanical equilibrium equation* $(Ze^2 / 4\pi\varepsilon_0 r^2) = (mv^2 / r)$, expressing that the Coulombian force between electron and atom nucleus is equilibrated by the centrifugal force due to the orbital movement of the electron.

In a second step Bohr resorted to *quantum physics,* Planck's theory of the quantization of energy, according to which an electron moving from an orbit of higher energy to an orbit of less energy emits an energy quantum in form of electromagnetic radiation of frequency $v = (E_i - E_f) / h$. E_i and E_f denote respectively the energies of the initial and final orbit.

Finally he assumed the quantization of the electron *angular momentum* $\vec{L} = \vec{r} \times \vec{p}$ in integer multiples of $\hbar$.

Bohr's atomic model could show some success, like the *theoretical explanation of Balmer's phenomenological formula.* Nonetheless it left behind some other questions unexplained, like the computation of the energy in multi-electronic atoms.

In sum, the main features of preduction are the following:

1. Preduction is an extension of the deductive method to the context of scientific discovery in the methodology of physics.
2. To preduce a theoretical model or a hypothesis amounts to generating deductive-mathematically an equation or a set of related equations, whose consequences should fit in well with observations.
3. Preduction is the way by which factual hypotheses and theoretical laws and models can be anticipated in physics by combination, in a form compatible with dimensional analysis, of previously accepted results.
4. Preduction grounds on previously accepted results of the whole theoretical background, which are *methodologically* postulated as premises of the inferential procedure. On the understanding that *accepted* does not mean *accepted as true.*

5. Preduction shares with the other discovery practices that preduced results are intrinsically fallible.

7. Conclusion

The main aim of this paper is to vindicate the importance of the context of discovery in the methodology of natural sciences. In this sense I have shown that there are different ways by means of which new ideas come to science. Following Peirce I accept indeed that *abduction* is one of them. But there are still more.

Resorting to palaeoanthropology and geology as paradigmatic observational natural sciences I have shown that they apply the abductive way of reasoning both as a means of discovery and of explanation. Therefore it looks strange that traditionally the academic philosophy of science has been so reluctant, until very recently, to accept abduction as an important tool of scientific methodology. I guess that both Popper's condemnation of the context of discovery in philosophy of science and the intensive discussion on induction and inductive probability have prevented philosophers of science from recognizing the role played by abduction in the history of natural sciences. Indeed it is unquestionable that scientists have been using the abductive way of reasoning both for the introduction of new hypotheses and for the explanation of available empirical data.

The disregard of the importance of abduction by philosophers of science has been a major shortcoming in the history of modern philosophy. Another shortcoming in contemporary philosophy of science was also the neglect of the relevance of the deductive way of reasoning in scientific discovery.

Indeed, whereas abduction is a sort of ampliative inference, *preduction*, i.e. the application of deductive reasoning in scientific discovery, is a kind of anticipative inference. Since preduction consists in the deduction of new hypotheses through the combination, compatible with dimensional analysis, of previously accepted results of physical theories, it supplies with new ideas, whose theoretical relevance can only be assessed after their submission to empirical control. Preduction thus extends the number of procedures capable of introducing new ideas in science.

Nonetheless abduction and preduction are not the only ways of reasoning in scientific discovery. A pragmatic approach to induction and the vindication of analogy as a guide to scientific creativity show that the number of ways of reasoning capable of supplying with new ideas in science is considerably larger that Peirce did once think.

All these forms of scientific discovery share a common feature: They are ways of fallible reasoning. Therefore I claim that the methodology of science provides us with a plurality of discovery practices or strategies that allow us to deal fallibly with Nature.

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