AMPLIATIVE AND ANTICIPATIVE INFERENCES IN SCIENTIFIC DISCOVERY: INDUCTION, ABDUCTION AND PREDUCTION

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1. Introduction

IN THIS PAPER I am concerned with three kinds of inference in the context of scientific discovery. Two of them, induction and abduction, are ampliative inferences. The third one, preduction, is an anticipative inference, in the sense of *anticipation* that I am going to make clear in Section 4.

Since the establishment of the philosophy of science as an academic discipline, philosophers of science have to a great extent neglected the issues related to the context of scientific discovery. It is true nonetheless that since the 1980s a renewed interest in abduction and in the question of the existence of any logic of discovery has attracted some attention, mainly in the domains of artificial intelligence, knowledge acquisition, logic programming and related matters. It is also true that since the identification by Gilbert Harman of abduction with inference to the best explanation, abduction has become a fundamental tool of scientific realism, i. e. at the meta-scientific level.

As to induction, Popper's anathema towards all philosophical analysis of the ways in which new ideas come to science, and his negative solution to the logical and methodological problem of induction, which was followed by an anti-inductivist campaign that lasted his whole life, led to the failure of all attempts to rescue induction from oblivion. I instead propose to view induction as a strategy of discovery or as a practice of scientific creativity leading to the postulation of fallible hypotheses about the world.

As to abduction, I agree with Peirce about its importance for the context of discovery. Indeed, abduction is currently used in observational sciences and in the empirical or phenomenological areas of the theoretical sciences. As a matter of fact, many scientific hypotheses are the result of abductive inferences. In my contribution I am going to show that Bayesian inference is of no help to abduction. I will argue, by means of a fictitious historical interpretation of the empirical basis of Newtonian mechanics, that Bayesianism cannot be used to support abductive inferences. Abduction shares with induction that it can only support the postulation of fallible hypotheses about the world.

Finally I am going to rejoin Charles Peirce's view that abduction is the only way by means of which new ideas come to science. In particular, I will contradict Peirce's claim that deduction can never originate any idea whatsoever. I claim that deductive reasoning can be extended to the context of discovery in the methodology of theoretical sciences such as mathematical physics. And I call *preduction* the way of reasoning which consists in the implementation of deductive reasoning in the context of scientific discovery. Preduction is the way of reasoning that is grounded in the available theoretical background as a whole and it permits the deduction of new results, provided that the combination and mathematical manipulation of the theoretical products which have been taken as premises of the preductive reasoning are compatible with dimensional analysis. I affirm that this is the way in which many factual hypotheses and theoretical laws and models are obtained in physics.

Since preduction is an implementation of the hypothetic-deductive method, it is not an ampliative inference. It is *anticipative*, as it furnishes theoreticians with several as yet unavailable new ideas, on the understanding that the results of preductive inferences are fallible as well. Indeed the available background on which preductive inference is based can be not true.

My conclusions are twofold: first, that ampliative inferences, like induction and abduction, and also anticipative ones, such as preduction, are merely fallible practices or strategies in scientific discovery, and second, that they furnish us with excellent arguments against the *myth* of the scientific method.

2. Induction

FROM ITS FIRST formulation by Aristotle in the fourth century B. C. until the present, induction has taken a wide variety of forms. Let us assume, to begin with, *induction* to be a kind of ampliative and truth conservative inference. Then *inductive reasoning* would provide a method of scientific discovery. Indeed, in the *Prior Analytics*, Book II, 68b15-29: Aristotle proceeds in the following way (cf. Rivadulla 2007):

Let *A* stands for long-liver,
B for bileless,
C for particular long-lived animals like man, horse, mule.

Aristotle's *inductive syllogism* looks like this:

All *C* are *A*	(*A* belongs to the whole of *C*)
All *C* are *B*	(*B* also belongs to every *C*)
Hence: All *B* are *A*	(*A* belongs to *B*)

If this form of reasoning were legitimate, then *inductive generalizations* would be a current method of inference in science. Indeed, by *simple enumeration* of a finite amount of empirical data it would be possible to arrive at true, until now unknown, natural laws. In the case presented by Aristotle, it would suffice to know that men, horse and mule are long-lived bileless animals, in order to infer that all bileless animals are long-lived. But two problems arise with this alleged form of inductive way of reasoning:

The first is that Aristotle's inductive syllogism is not a syllogism of the first figure (it is not like *Barbara*).

The second is that Aristotle's inductive syllogism is not a valid syllogism of the third figure, i.e. it is not of the kind: *Darapti, Felapton, Disamis, Datisi, Bocardo, Ferison.*

In conclusion: Aristotle's inductive syllogism is not a syllogism at all. Nonetheless, Aristotle himself provided a tentative solution:

> "If then ***C*** is convertible with ***B***, and the middle term is not wider in extension, it is necessary that ***A*** should belong to ***B***. For ... if two things belong to the same thing, and the extreme is convertible with one of them, then the other predicate will belong to one that is converted."

Thus Aristotle's solution consists in permuting *B* and *C*, and Aristotle's inductive syllogism would look like a syllogism in *modus Barbara*, so it would be formally correct:

All *C* are *A*	(man, horse and mule are long-lived animals)
All *B* are *C*	(All bileless animals are man, horse and mule, i.e. the only bileless animals are man, horse and mule)
Hence: All *B* are *A*	(All bileless animals are long-liver)

The price Aristotle is prepared to pay for this transformation of an invalid syllogism into a logically valid argument is to assume that induction proceeds by complete enumeration of all cases:

> "[W]e must apprehend ***C*** as made up of all the particulars. For induction proceeds through an enumeration of all the cases."

Induction by complete enumeration of all the cases is indeed a form of deduction. Therefore it did not capture the attention of scientific-minded philosophers. Instead, philosophers concentrated on induction by simple enumeration as a way of reasoning leading to the discovery of laws. In this sense:

1.- Induction was criticized by Duns Scotus and 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. 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 extension of David Hume's argument of the logical illegitimacy of induction consisted precisely in his claim of the impossibility of inductive probability. I have rejoined myself to Popper's arguments in Rivadulla (1987, 1991 and 1994). Nonetheless, the anti-inductivist approach had already been 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.

In spite of this anti-inductivist attitude, Hans Reichenbach was convinced of the relevance of induction to 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 would 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."

A third position on 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." (1974, 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." (1974, 5.170)

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

The last approach to induction I am taking here into account is Bertrand Russell's. Russell (1949, pp. 69-70) claims that all scientific laws rest upon induction. Nonetheless he maintains, similarly to Francis Bacon, that induction by simple enumeration is a highly dangerous form of argument, that it is incapable of providing certainty. 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 reorder its premises. And this provides me with the occasion, in the next section, to examine the second kind of inference I am concerned with in this paper.

3. Abduction

John R. Josephson (1994, p. 5) conceives Abduction in the usual Peircean way as a

> "[F]orm of inference that goes from data describing something to a hypothesis that best explains or accounts for the data."

And abduction belongs for him to the kind of *ampliative inferences*, like inductive generalizations.

Moreover, abduction or *inference to the best explanation*, is considered by scientific realists, for instance by Psillos (1996, p. 31):

> "[T]he mode of reasoning that scientists follow in order to form their theoretical beliefs, and ... it can reliably produce and sustain (approximately) true beliefs about the world."

I agree that abduction is fundamental for hypothesis formation, i.e., in the context of discovery, both in observational natural sciences such as palaeoanthropology and Earth sciences and in theoretical sciences, such as mathematical physics (Rivadulla 2008). Abduction is the way of reasoning by means of which hypotheses and/or theoretical models are proposed as the most reasonable tentative explanations of the available empirical data.

Moreover there is a *meta-scientific* application of abduction. In this use, "scientific realism has been defended by an abductive nomiracle argument." (Niiniluoto 1999, p. 436). An exception to this was scientific realist Karl Popper. Indeed, the word *abduction* is conspicuous by its absence in his whole work. How can this be explained? Besides the fact that Popper learned about Peirce relatively late, in the early 1950s, the most important reason may have been that Popper was not interested in the *context of scientific discovery*. Popper's rejection of inductive generalization as the way of proposing theories from observations may have led him to ignore abduction. And this was Popper's mistake, since the Popperian view of *deductive testing* as the method of science excluded from the realm of science nearly any *observational natural sciences* such as Darwin's evolution theory, palaeontology and geophysics.

Let me illustrate this point by resorting to the personal confessions of several of the protagonists in the development of modern earth sciences. John G. Sclater (2001, p. 137) describes himself as a former graduate "imbued by the hypothesis-testing method of doing science, which I had picked up in the experimental physics courses that I had taken. I started and finished my graduate career at Cambridge with the same basic approach." In spite of his Popperian education, he finally

(Sclater 2001, pp. 137 f.) became convinced that the creators of plate tectonics merely

> "were creating new concepts out of the synthesis of poorly-constrained observational information. They thought their concepts had validity because they explained the patterns they recognized in so many different sets of data.

And that

> "[m]ajor progress occurs by constructing simple physical models that describe the patterns that earth scientists have selected out of the background noise."

In a similar way Dan McKenzie (2001, p. 185) affirms:

> "In many parts of the earth sciences, including tectonics, experiments are impossible: nothing human beings can do can affect the processes involved, and these cannot be scaled properly to conduct laboratory experiments."

> "I spend my time trying to construct models that can describe what I and others have observed, using ideas from mathematics and other physical sciences. I judge I have succeeded when I find that my model can account for some well-known observation that was not understood, and which it had not occurred to me to connect with the observations that I was trying to model."

And Jack Oliver (2001, p. 164) emphasizes what he names the *inductive style of science*:

> "Science is the 'organization of observations'. Furthermore, that's all it is. Science is basically empirical. Observations are the only truth, or facts, of science. Hypotheses, theories, laws, or whatever, mathematical or not, are merely ways we

> have devised to organize those facts so that we can comprehend and interrelate them. Any part of the theoretical side of science is subject to revision if observations so dictate. The scope of science is hence determined and delimited by its observations. Therefore one good way, probably the best way, to add to the scope of science is to make reliable new observations of the unknown."

Oliver's own confession is that (pp. 165-166; my emphasis):

> "[I]t was this kind of thinking that led me to attempt to explore and obtain new observations of a poorly known part of the earth, the deep continental crust and uppermost mantle, through deep seismic reflection profiling, that is, echo sounding, of that part of the earth. [...]
> I remain convinced of the great potential for major advance in earth science through acquisition of this kind of observational data. For those whose goal is to make a great advance in earth science, perhaps on the scale of plate tectonics, my recommendation is to make a major effort, *in the inductive style*, to explore and understand that region of earth, the deep crust and uppermost mantle, comprehensively, through application of the seismic reflection profiling technique that we know will work and that we are fully capable of carrying out. The envelope of our world of science that is determined by our observational database will be pushed back. ...
> The kind of reasoning that I have just been using for the last few paragraphs is designed to reveal effective strategy and tactics from study of the history of previous successes in science, so that such strategies and tactics can be applied to new topics. Such reasoning, in my opinion, is sound and very likely to produce major discoveries."

Of course these personal confessions prove nothing. But if we were prepared to accept, as I do, that geology constitutes a typical case of an abductive science, then it would support my view that the methodology of science cannot be reduced to mere hypothesis testing.

Charles S. Peirce's very well known scheme of the abductive argument

— 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

was transformed by Josephson into the following

— *D* is a collection of data (facts, observations, givens)
— *H* explains *D* (would, if true, explain *D*)
— No other hypothesis can explain *D* as well as *H* does
— Therefore, *H* is probably true.

Thus the question arises: Who are right, those philosophers like Russell and Josephson, who claim that ampliative inferences are probabilistic inferences, or those like Popper, who maintain that the belief in a theory being probably true cannot be rationally justified?

In order to answer this question I will analyse whether the Bayesian model of inference provides a reasonable way of assessing the viability of probable inferences.

Let e_i, e_j, e_k, e_l be different items of evidence favourable to a given hypothesis h. Let us assume that each of these items of evidence satisfies the following conditions:

1) $h \rightarrow e_i$ and 2) $0 < p(h) < p(e_i) < 1$.

Under the verification of these conditions, Bayes' Theorem allows us to conclude that

$$..> p(h|\, e_j \wedge e_i \wedge e_k) > p(h|\, e_j \wedge e_i) > p(h|\, e_j) > p(h),$$

i.e., that any growing amount of evidence favourable to a given hypothesis contributes to increasing the probability of the hypothesis being true.

But in spite of the intuitiveness of this result, two theoretical problems arise here:

1.- If new evidence contradicts the hypothesis, or if a new prediction of the hypothesis fails, then a highly probably true hypothesis becomes false.

2.- Imagine the history of Newtonian mechanics had been as follows:

    2.1.- Let us assume that Newton's *Query I* of his *Optics*, 1704: "Do not bodies act upon light at distance, and by their action bend its rays; and is not this action (*coeteris paribus*) strongest at the least distance?" was the first prediction of Newton's theory, and that it had been answered by him in the following terms: the bending angle amounts to 0.87 seconds of an arc.

    2.2.- Let us assume that Newton himself had indicated the conditions for the confirmation of this prediction, and that astrophysical and technical conditions were given for the empirical proof of the hypothesis.

    2.3.- Then, due to the discrepancy of the *predicted value*: 0.87 seconds of arc with the *observed value*: 1.75 seconds of an arc, *Newton himself would have acknowledged that his gravitational hypothesis was simply wrong*. And the history of physics would have been completely different.

    2.4.- Finally, since no further application of Bayes' Theorem could contribute to overcome the first *a posteriori* zero probability of Newtonian mechanics, we must conclude that Bayes' rule, which is a theorem of the probability calculus, is not suitable for the probabilistic evaluation of scientific theories.

4. Theoretical preduction. The extension of deductive reasoning to the context of discovery

Neglecting the importance of the context of discovery for the methodology of science may have been a major shortcoming of Popper's philosophy of science. As I have indicated above, it is not true that the scientific method consists exclusively in the deductive testing of hypotheses. And the example of natural observational sciences shows that these disciplines are not primarily concerned with hypothesis testing. Thus to exclude them as non-scientific would be an extraordinary exercise in irresponsibility. Moreover, Popper missed the opportunity to recognize that hypothetic deductivism can also be applied to the introduction of new ideas in science. I call *preduction* the way of reasoning consisting in the extension of the hypothetic-deductive method to the context of discovery.

The dispute for many decades in the methodology of science around the possibility of induction, inductive logic and inductive probability hindered the recognition of the importance in the history of science of abduction and of ways of conceiving new ideas. Nowadays, when Peircean abduction is being widely accepted as a *logic of discovery*, and the context of discovery receives at least as much attention as the context of justification or validation, it is time to ask whether or not abduction is the only way of reasoning by which all ideas come to science, i. e. the only logical operation which introduces new ideas, as Charles Peirce (1974, 5.171) triumphantly claimed.

Indeed I wonder that philosophers of science —who have systematically applied the hypothetic-deductive method to the contexts of justification and of explanation— have disregarded its applicability to the context of discovery, although its implementation as a way of conceiving new ideas constitutes a tried and tested form of reasoning in theoretical physics. This is what preduction consists in, and this is how many factual hypotheses, theoretical laws and theoretical models are introduced in theoretical physics.

For the sake of the argument I resort to Charles Peirce (1974, 5.145), according to whom: "Induction can never originate any idea whatever. No more can deduction". I disagree with him that "All the ideas of science come to it by the way of abduction." (1974, 5.145). Thus my main question is: *can deductive reasoning be used in the context of scientific discovery?* My answer is: Yes, it can. This overcomes the alleged 'weakness' of the hypothetic-deductive method, which Peter Medawar (1974, p. 289) points to: "The weakness of the hypothetic-deductive system... lies in its disclaiming any power to explain how hypotheses come into being."

I maintain that in the methodology of theoretical physics we can extend deductive reasoning to the context of discovery, beyond its ordinary use as axiomatic deductivism, or in the context of hypotheses testing, or in the context of theoretical explanation.

Theoretical physics makes use of mathematics as an indispensable tool. The theoretical physicist applies at will Leibniz's principle of *substitutio salva veritate*. Dimensional analysis guarantees the truth, or rather the legitimacy of the undertaken substitutions. My thesis is that a new form of reasoning in scientific methodology becomes recognizable. I call it *theoretical preduction* or simply *preduction*. *Preduction* is the extension of the deductive way of reasoning to the context of scientific discovery. It consists in resorting to the available results of theoretical physics as a whole, in order to make it possible to obtain, or to *anticipate*, new results by mathematical combination and manipulation, compatible with dimensional analysis, of the available results.

Both abduction and preduction are forms of reasoning by means of which new ideas come to science. They belong to the context of discovery. But there is a fundamental difference between them: Whereas in abduction, novel conjectures are suggested by the empirical available data, in preduction new ideas result deductively from the available theoretical background of the whole of physics by combination and manipulation, compatible with dimensional analysis, of previously accepted results —on the understanding that 'accepted' does not mean 'accepted as true'.

Since elsewhere (Rivadulla 2008 and 2009) I have illustrated this notion with many examples, let me here show as a case study how preduction can help to scrutinize the 'internal constitution' of white dwarf stars.

White dwarfs are stars which have nearly the Sun's mass and the size of the Earth. These circumstances pose questions about how white dwarfs can stand such pressure, or whether there is a limit to their mass. There is no single theory capable of answering these questions. But combining by several theories —namely classical physics, quantum mechanics, atomic physics and relativity theory— it becomes possible to *preduce* a theoretical model for white dwarfs that allows us to deal predictably with these objects.

In fact, in order to have an idea about the pressure in the interior of white dwarfs it suffices to assume spherical symmetry in order to apply the *equation of hydrostatic equilibrium* of classical physics: $dP/dr = -\rho g$ which expresses the *pressure gradient* in function of the gas density ρ and the *gravity acceleration* $g = G_N M/r^2$ at distance r from the star centre. Since $dP/dr = -(4\pi G_N \rho^2 r)/3$ integrating over all pressures on the left and over all distances on the right, we obtain that $P = (2\pi G_N \rho^2 R^2)/3$. This pressure counteracts the gravity force and makes it possible for the star to remain static. Under these assumptions, the pressure at the centre of the white dwarf is about 1.5 times higher than at the Sun's centre. (Ostlie & Carroll 1996, p. 580)

Now, in order to answer the question of how a white dwarf can maintain the equilibrium of the different forces of pressure and gravity acting in opposite directions, it is necessary to combine atomic physics, and in particular Pauli's exclusion principle —which prohibits two or more electrons (fermions) from having the same quantum numbers in a given state— Fermi's energy value for completely degenerate electron gases, and Heisenberg's indeterminacy relations. We obtain the value for the pressure due to the electron degeneracy, which "*is responsible for maintaining hydrostatic equilibrium in a white dwarf*." (Ostlie & Carroll 1996, p. 588)

The question now is how much matter can stand this pressure. Taking into account that in the case of extreme velocities, the electron speed must be c at most, equating the value of the electron degeneracy pressure $P \approx (\hbar c/3)(Z\rho^2/Am_H)^{4/3}$ with $P = (2\pi G_N \rho^2 R^2)/3$, we obtain $M_{Ch} \approx (\hbar c/G_N)(Z/Am_H)^2$, an expression known as the *Chandrasekhar mass limit for white dwarfs*. Ostlie & Carroll (1996, p. 590) claim:

> "This formula is truly remarkable. It contains three fundamental constants —*ħ*, *c*, and *G*— representing the combined effects of quantum mechanics, relativity, and Newtonian gravitation on the structure of a white dwarf."

Thus Ostlie and Carroll acknowledge the need to combine (principles or results of) different theories in theoretical physics in order to obtain interesting hypotheses, a fact that I call *theoretical preduction*, or simply *preduction*.

(By the way in the case study presented above the combination, compatible with dimensional analysis, of three 'incommensurable theories'—in Kuhn's sense— is necessary, in order to know, or to guess about, the *internal constitution* of white dwarfs.)

Preduction is a way of reasoning that starts with previously accepted results of the available theoretical background. These results are postulated *methodologically* as premises of an inferential procedure. These premises can proceed from different theories. Any accepted result can serve as a premise, on the understanding that *accepted* does not imply *accepted as true.* This evokes the notion of hypothetic-deductive method. Indeed, *preduction* is an implementation of the deductive way of reasoning. The specificity of preduction is that it is an extension of deductive reasoning to the context of scientific discovery.

5. Main theses on the relationships between ampliative and anticipative inferences

1.- *PREDUCTION* IS A WAY of reasoning that starts with previously accepted results, not necessarily assumed as true and which are *methodologically* postulated as premises of further scientific inferences.
2.- These premises can proceed from different theories.
3.- Preductive reasoning differs from abduction and from any ampliative inference that the preduced hypotheses do not proceed from empirical data, but that they are the result of

deductive reasoning starting from the available theoretical background.

4.- Preduction is extension of deductive reasoning to the context of scientific discovery.
5.- Preduction consists in the combination, compatible with dimensional analysis, of previously accepted results of the available theoretical background. This is the way in which many factual hypotheses and theoretical laws or theoretical models are introduced or anticipated in physics.
6.- Preduction is an *anticipative* inference as it furnishes theoreticians with several as yet unavailable new ideas
7.- All preduced products are intrinsically fallible.
8.- Whereas abduction, and in general *ampliative* inferences, are the preferred way of reasoning in the methodology of *observational natural sciences*, preduction is preferred —although not exclusively— in the context of discovery of *theoretical natural sciences*, such as mathematical physics.
9.- Both *ampliative* and *anticipative* inferences widely encompass —but do not necessarily exhaust— the creative spectrum, the *ars inveniendi*, in natural sciences.

6. Conclusion: The intrinsic fallibility of inductive, abductive and preductive inferences and the myth of the uniqueness of the scientific method

In the case of abduction, and in general of *ampliative inferences,* new data can recommend the revision of the accepted hypotheses and their replacement by new ones, which should be compatible with both the new and old data. Indeed, as Peirce (1974, 2.777) himself recognizes: "The hypothesis which it [abduction, A.R.] problematically concludes is frequently utterly wrong itself, and even the method needs not ever lead to the truth."

In the case of preduction, the preduced hypotheses and models depend on the totality of the available theoretical background, parts of which can be not true.

Thus induction, abduction and preduction are merely *fallible reasoning strategies, or reasoning practices, that allow us to deal scientifically with Nature.* There is no unique scientific method, but instead several scientific methodologies suited to the different concrete disciplines. It might be better to say that *there are different practices or strategies guiding us to scientific creativity,* and that Induction, Abduction, Preduction (and also Serendipity and Analogy) are some of them.[1]

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