Archaeological Researches on Popper's Philosophy of Science: Lights and Shadows

Investigaciones arqueológicas sobre la filosofía de la ciencia de Popper: luces y sombras

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Abstract: This article dives into the origins of the philosophy of science of Karl Popper. Where does Popper's epistemological position take root around the two fundamental problems of the theory of science? This is the main question. This research brings to light really amazing results, some of which may lead to cast doubt on the soundness of the basic Popperian approaches. But it is no less surprising, if at all, that with so little Popper could get so much. Popper's case is an excellent example of how some ideas that some might consider pinned up, can penetrate deeply if, besides being interesting, they are defended with conviction and intelligence and at least a part of the intellectual environment contributes to their discussion, assimilation and development.

Keywords: inductivism, falsificationism, scientific explanation, realism, instrumentalism, Popperianism.

Resumen: En este artículo buceo en los orígenes de la filosofía de la ciencia de Popper. ¿Dónde se enraíza su posición epistemológica en torno a los dos problemas fundamentales de la teoría de la ciencia? Ésta es la pregunta principal. Esta investigación saca a la luz resultados realmente sorprendentes, algunos de los cuales pueden llevar a hacer dudar de la solidez de los planteamientos popperianos básicos. Pero no es menos sorprendente, llegado el caso, que con tan poco Popper pudiera llegar a conseguir tanto. El caso de Popper constituye un excelente ejemplo de cómo unas ideas, que alguno podría considerar prendidas con alfileres, pueden calar profundamente si, además de ser interesantes, se defienden con convicción e inteligencia, y al menos una parte del entorno intelectual contribuye a su discusión, asimilación y desarrollo.

Palabras clave: inductivismo, falsacionismo, explicación científica, realismo, instrumentalismo, popperianismo.

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1. **Introduction**

In the XXI century the philosophy of science is an academic discipline fully consolidated in universities all over the world, with specialized professors, many of whom have dedicated to it their lives. We cannot therefore judge the contributions of the predecessors who inaugurated the discipline with the same criteria of rigor with which we evaluate almost daily to each other in our days. But tracking down their origins is a fascinating task. This always requires exquisite care, prudence, respect and, of course, professionalism. Surely this search can sometimes lead us to demystify, at least in part, the characters investigated; but in part, too, to recognize their achievements. Let me pick up here some lines from the French epistemologist Claude Bernard (1813-1878). For Bernard (1957:41) the non-submission to authority "is by no means out of harmony with the respect and admiration which we bear to the great men preceding us, to whom we owe the discoveries at the base of the sciences of to-day."

The character I'm going to tackle in this article is the Viennese Karl Popper (1902-1994). Popper began stomping on the philosophical arena. In 1933 he had already concluded, and was close to publishing, a book entitled *Die beiden Grundprobleme der Erkenntnistheorie* (*The Two Fundamental Problems of the Theory of Knowledge*). Both fundamental problems of the philosophy of science were the problem of induction and the problem of demarcation. The choice of the title of the book was not casual, since both problems were the two central questions that were already being debated in the philosophical environment of that time, an environment that casually, but fortunately, was also mainly Viennese. The Vienna Circle and its philosophical surroundings were particularly interested in establishing a criterion of demarcation between science and metaphysics. And as Popper, by that time –late twenties and early thirties of the last century– already had a solution to both problems, he found a tilled ground, though certainly not propitious, to the proposal of their own approaches on the matter.

To investigate the roots of Popper's philosophical thought is to first investigate his mentioned book, for his *Logik der Forschung*, hereafter *LF,* 1935, is an abbreviated version of it, not even written by Popper himself. This is an anecdote that still amazes many, but that Popper did not hide. He revealed it forty years later in his *Intellectual Autobiography* (Popper 1974*a*: 67). Nonetheless *The Logic of Scientific Discovery*, hereafter *LSD*, is Popper's work, even if Popper was not who wrote it.

The Logic of Scientific Discovery, and Rudolf Carnap's *The Logical Structure of the World* (*Der logische Aufbau der Welt*, 1928) are the books that inaugurate the contemporary philosophy of science. Both are seminal, but in the present paper, I focus myself on the first one, which the history of scientific philosophical thought of the West has placed in the privileged place it deserves. Here I tackle the following issues:

1. What did Popper know about the philosophers around him?
2. What did Popper know about other European philosophers of science?
3. Where did Popper's anti-inductivism come from?
4. Where did Popperian falsificationism come from?
5. What role did Popper play in the development of the theory of scientific explanation?
6. Was Popper in his early days a realist philosopher of science?
7. What is the origin of instrumentalism in the current philosophy of science?

To be completely honest, it is absolutely imperative to remember that it is not possible to make a point-by-point comparison between *The Logic of Scientific Discovery* and *The Two Fundamental Problems*. The reason is that this last book, edited by Troels Eggers Hansen, and published in 1979, is based on the available manuscripts of the years 1930-1933, and that, as Hansen himself says in his *Editor's PostScript*, p.485 (German[1]: *Nachwort*, p. 441), of the two volumes of which the work was composed "Volume I: *The Problem of Induction* appears to have been preserved in its entirety, whereas almost the whole manuscript of *Volume* II: *The problem of Demarcation* must be presumed lost. Of this volume, there exist a few fragments only and the drastically

[1] Hereafter, 'German' refers to *Die beiden Grundprobleme der Erkenntnistheorie*, 1979.

abridged version published in 1934 under the title *Logik der Forschung*." This would explain, among other things, why in *Logik der Forschung* there is a chapter, entitled "Observations on Quantum Mechanics", number IX, about which there is practically nothing in the modern edition of *The Two Fundamental Problems*. But this circumstance, that half of the source work from which *LF* (*LSD*) comes is lost, forces us to be very prudent in our judgments on Popper's *LSD*, and therefore on the origins of his philosophy of science. Now prudence is not at odds with rigor.

2. What did Popper know about the European philosophers of science?

2.1. From the contemporary German-speaking philosophers and scientists we would nowadays consider the most relevant, Popper mentions Einstein, as we shall see later. From Mach and Schlick the original sources are naturally in German.

But **neither** Richard Avenarius (1843-1896), a German positivist of great influence in Carnap, and author, among others, of *Kritik der reinen Erfahrung*, Leipzig 1888-1890, **nor** Wilhelm Ostwald (1853-1932), author among other books of *Grundriss der Naturphilosophie*, 1908, are mentioned by Popper.

Hugo Dingler (1881-1954), a German philosopher of physics, is referred to in page 232 (German: 215), regarding his critic to the theory of the relativity. The most complete reference is given in p. 414 (German, p. 375) in which he raises the point of view of Dingler that "we address reality with our theoretical questions, and 'exhaust' it with the help of our theories". But it 's the editor, and not Popper himself, who relates both references respectively to Dingler's works *Physik und Hypothese*, 1921, and *Grundlinien einer Kritik und exakten Theorie der Wissenschaften insbesondere der Mathematischen*, 1907. On p. 436 (German, p. 394) Popper mentions Dingler's conventionalism as a resource for emerging from the collapse of the science of the turn of the century. The editor relates this reference to Dingler's works *Die Grundlagen der Physik: Synthetische Prinzipien der mathematischen Naturphilosophie*, 1923, and *Der Zusammenbruch der Wissenschaft und der Primat der Philosophie*, 1926.

2.2. French philosophers

In his "Reply to Medawar on Hypothesis and Imagination" Popper (1974*b*: 1030-1031) claims: "Medawar's contribution made me read Claude Bernard's *Introduction to the Study of Experimental Medicine* and I am immensely grateful for this. … [Bernard] constantly faced the ever-repeated overthrow of theories on all levels of universality and importance." And Popper recognizes: "Medawar's contribution has made me see, to my surprise, how many of my ideas have been anticipated by others; for those ideas not only came to me without my having read or heard about them, but in some cases were developed in conscious opposition to my elders and betters." Scientific realism, falsificationism and critical attitude, the anticipatory role of hypotheses and theories, questions that would become defining characteristics of the Popperian philosophy of science, were advanced by Bernard, many decades before Popper had developed his own ideas. But even the role of the context of scientific discovery, linked to a non-naive conception of induction and the view that scientific activity always begins with problems, were also anticipated by Bernard.

Of course, when Bernard published in 1865 his *An Introduction to the Study of Experimental Medicine* (*Introduction à l'étude de la medicine experimentale*) he was in top form: in the intellectual maturity of a man of science also turned to philosophical reflection. His philosophical ideas are not therefore pinned they are not a happy finding, let alone the result of ingenious occurrences. They are the ripe fruit of serene critical reflection based on scientific practice.

Henri Poincaré (1854-1912) is mentioned indeed by Popper, but without referring to a single work of this relevant scientist.

First reference, p. 193 (German, p.179): "But conventionalism (Poincaré) asserts than in defining a space-time system of measurement, we only *apparently* proceed in this fashion."

Second reference, p. 195 (German, p.181): "This sums up the view of conventionalism. It was originally developed (in a less radical fashion) by Poincaré. In Duhem's work in particular, it becomes evident that conventionalism is actually purely *deductivist*." And several times on Section 30: *Conventionalist and empiricist interpretations, illustrated by the example of applied geometry,* p. 223

In a *further reference*, on p. 397 (German, p.338) Popper insists again, but this time without mentioning Poincaré: "The conventionalist view might be characterized by the statement that a scientific *theorist can never be put right by 'experience'*; only an experimenter can be put right by experience –even he, however, not about the truth of scientific statements but only about the practical-experimental success of theories."

Also Pierre Duhem is mentioned on p. 195, as indicated above. And on p. 196 (German, p.182): "Duhem and Kraft are probably the most important defenders of the deductivist modes of thought in the modern theory of knowledge." And on p. 282 too (German, 260), from the German translation of his *Physical Theory* of 1906, in relation to the problem of scientific holism. He never mentions him again. But it certainly is interesting what Popper says about him, p. 260: "From considerations of this sort, it has been concluded (for example, by Duhem[2]) that there exists no actual falsification of natural laws. Only a *theory taken as a whole* can be rejected, which by no means implies that all contentions of the theory have been rejected. On the contrary, we should always be prepared for individual propositions or parts of a falsified theory to re-emerge later (or in a different context). They cannot, that is to say, be regarded as *definitely* falsified." It is striking that Popper assumes that the falsification procedure was a pre-existent methodological tool in the practice of science.

2.3. British philosophers and scientists

The mathematician William Kingdon Clifford (1845-1879) and the theoretical statistician Karl Pearson (1857-1936) –both of them very interested in the philosophy of science– are ignored by Popper. **Nor** is William Whewell (1794-1866) **ever mentioned** by Popper. Only on p. XVIII, of the Introduction, 1978!, he qualifies him as a great opponent of John Stuart Mill (1806-1873), in the evaluation of the inductive sciences.

John Herschel (1792-1871) is only mentioned once, at p. 22 (German, p.20), as the origin, together with Bacon, of Mill's ideas. William Stanley Jevons (1835-1882) is only mentioned once, in p. 8, where Popper rejects Jevons's alleged view that the scientific method would be a link of induction and deduction. And Arthur Stanley Eddington (1882-1944) is also mentioned only once by a German translation (p. 414, note 1 (German, p. 375, note 2)). Did any other more honourable reference appear in the missing volume of *The Two Fundamental Problems* to this British astrophysicist, who in 1919 contributed to the confirmation of the general theory of relativity? Of course, Eddington would have deserved it. But that, it seems, we will never know.

The great name of the theoretical statistics of the early twentieth century, Sir Ronald Aylmer Fisher (1890-1962), for whom the theoretical statistics was the inductive logic par excellence, is **completely ignored** by Popper. Only years later, as recorded in the new appendix * IV. *The Formal Theory of Probability* of *LSD* makes use Popper (1959: 330) of Fisher's *Likelihood* concept (*L.F.*, p. 272), but erroneously, since for Fisher precisely likelihood is **not** a probability, and also in * IX. *Corroboration, the Weight of Evidence, and Statistical Tests*, from 1954, 1957 and 1958 published in *B.J.P.S.*, where he again refers to Fisher's concept in p. 387, as well as in notes * 1 of p. 398 and * 5 of p 414 (p. 340 and notes *1 of p. 350 and *5 of p. 367 respectively of *LF*) The mistaken interpretation of the Fisherian concept of likelihood is also observed in footnote 8 at p. 243, as well as in p. 252 of the *PostScript* Vol. 1.

But the most astounding thing about this whole affair is that Walter Schiff, the carnal uncle of Popper, who wrote the final version of *LSD*, was professor of statistics[3] at the University of Vienna! How did he not bring his nephew abreast of the developments in Ronald Fisher's inductive statistical theory? Fisher's first major contribution to theoretical statistics: "On the mathematical foundations of theoretical statistics" goes

[2] [Pierre Duhem, *Ziel und Struktur der physikalischen Theorien* (German von Friedrich Adler, 1908), pp. 243 ff., 266 f.]
[3] Cf. Popper (1974*a*: 65)

back to 1922. And between this date and 1935 Fisher published a significant number of articles on statistical estimation, inverse probability, fiducial probability and mathematical likelihood.

The *inductive* character of theoretical statistics was fundamental for Fisher (1922: 366), who indeed maintained that "the purpose of the statistical reduction of data is to obtain statistics[4] which shall contain, as much as possible, ideally the whole, of the relevant information contained in the sample." More clearly Fisher (1932: 257) recognized that "in inductive reasoning we attempt to argue from the particular, which is typically a body of observational material, to the general, which is typically a theory applicable to future experience. In statistical language we attempt to argue from the sample to the population from which it was drawn." And in (1935:39) Fisher assumed that in inductive processes we attempt "to draw inferences from the particular to the general; or, as we more usually say in statistics, from the sample to the population. Such inferences we recognize to be *uncertain* inferences, but it does not follow from this that they are not mathematically rigorous inferences." And whereas there is no guarantee for Fisher (1935: 40) of the adequacy of probability "for reasoning of a genuinely inductive kind", "a mathematical quantity of a different kind, which I have termed *mathematical likelihood*, appears to take its place as a measure of rational belief when we are reasoning from the sample to the population."[5] Being Popper a convinced and confessed anti-inductivist theorist it is amazing that he never paid attention to the anti-Bayesian theoretical statistics, and in particular to his contemporary Sir Ronald Aylmer Fisher.

3. Where did Popper's anti-inductivism come from?

In *The Two Fundamental Problems*, p. 316, (German: 288), Popper puts forward what he calls the *fundamental thesis of inductivism*: "All legitimate statements of science must be reducible to elementary empirical statements. In other words: the truth of all legitimate statements must depend on the truth values of some elementary empirical statements." Of course, for Popper it immediately follows that "As long as *induction*, or the inference of universal statements from singular experiences, is accepted as justified, the 'fundamental thesis of inductivism' proves to be an exceedingly useful *demarcation criterion* with the help of which natural laws can also be demonstrated to be 'legitimate'."

Nonetheless, according to him (*The Two Fundamental Problems*, p. 357. German, p. 325)): "The problem of induction, the question of the truth of universal empirical statements, is answered as follows: universal empirical statements can never have a [definitive] positive truth value, but only a [definitive] negative truth value.". And in *LSD* §6: *Falsifiability as a Criterion of Demarcation*, p. 40, Popper claims: "Now in my view there is no such thing as induction. Thus inference to theories, from singular statements which are 'verified by experience' (whatever that may mean), is logically inadmissible. Theories are, therefore, *never* empirically verifiable."[6]

Considerably more evolved is Claude Bernard's position, seventy years earlier, who devotes Section V of Chapter II of his 1957 book to induction and deduction in experimental reasoning. Understanding induction "as the process of moving from the particular to the general, while deduction is the reverse process moving from the general to the particular", Bernard (1957: 45) claims that "Both forms of reasoning, investigating (inductive) and demonstrating (deductive), pertain to all possible sciences, because in all the sciences there are things that we do not know and other things that we known or think to know." "The principles or theories which serve as foundations for a science, whatever it may be, have not fallen from the sky; they were necessarily reached by investigation, inductive or interrogative reasoning, as we may choose to call it." (45)

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[4] For instance, the calculation of the sample statistics from the data of the population's unknown mean values and the standard deviation. These statistics have to fulfil the requirements of consistency, efficiency and sufficiency.

[5] In Rivadulla (1991*a*, Chap. IV and V and in 1991*b*) I have tackled extensively the problem of induction from the point of view of theoretical statistics, both historically and systematically.

[6] And in the Addendum, 1972, Popper (*LSD* 1959, 2002: 281), affirms: "*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." A position that he ratifies in his *Intellectual Autobiography*, where Popper (1974a: 118) maintains that: "induction is a myth. No 'inductive logic' exists. And although there exists a 'logical' interpretation of the probability calculus, there is no good reason to assume that this 'generalized logic' (as it *may* be called) is a system of 'inductive logic'."

According to Bernard (47): "induction and deduction belong to all the sciences. I do not believe that induction and deduction are really two forms of reasoning essentially distinct." The reason, as Bernard (1957: 46) points out, is that "the general proposition which he [the naturalist] has reached, or the principle on which he relies, is relative and provisional, because it embodies complex relations which he is never sure that he can know. Hence, his principle is uncertain,…and so he must necessarily appeal to experiment to verify the conclusion of this deductive reasoning." Or as he says later (page 48): "our principle must always remain provisional, because we are never certain that it includes only the facts and conditions of which we are aware. In short, our deductions are always hypothetical until verified experimentally." That is, inductive inferences have to pass empirical control.

Being an inductivist scientist does not necessarily entail being naive or radical: "When any sort of phenomenon strikes us in nature –claims Bernard (1957:48) in a way that reminds us of Peirce– we work out our idea of the cause determining it." Bernard's theory of induction maintains indeed suggestive relations with the Peircean idea of abduction, and thus provides a claim to the context of scientific discovery, unhappily rejected both by Popper and Reichenbach, which produced a considerable delay in the further development of the philosophy of science.

Bernard also anticipates to Popper in the idea that the empirical science should not be merely observational, but must be guided by theory: "It is impossible to devise and experiment without a preconceived idea; devising an experiment,…, is putting a question; we never conceive a question without an idea which invites an answer." (p. 23) This idea he had already presented a few lines before: "An experimenter,…, is a man inspired by a more or less probable but anticipated interpretation of observed phenomena, to devise experiments which, in the logical order of his anticipation, shall bring results serving as controls for his hypothesis or preconceived idea." (22) Being anticipations of observations, hypotheses, conjectures or scientific theories are, for Bernard, subject to experimental control.

In relation to the view of the scientist's anticipatory work we can see in Bernard an advance of the idea of abduction as a way of reasoning for the proposal and explanation of new hypotheses: "[Scientists] formulate more or less ingenious and more or less probable hypotheses based on these observations" (p. 25), or: "Men who experiment, despite all their dexterity, cannot solve problems unless they are inspired by a fortunate hypothesis based on accurate and well-made observations." (25-26) The hypotheses suggested as explanations of facts have to be submitted, however, to subsequent experimental control: "The true scientist is one whose work includes both experimental theory and experimental practice. (1) He notes a fact; (2) *à propos* of this fact, an idea is born in his mind; (3) in the light of this idea, he reasons, devises an experiment, imagines and brings to pass its material conditions; (4) from this experiment, new phenomena result which must be observed, and so on and so forth." (p. 24) Or: the experimenter: "states an idea as a question, as an interpretative, more or less probable anticipation of nature, from which he logically deduces consequences which, moment by moment, he confronts with reality by means of experiment. He advances, thus, from partial to more general truths, but without ever daring to assert that he has grasped the absolute truth." (p. 27) It is not, therefore, by means of a simple and naive enumerative induction that the scientist advances, anticipates or postulates new ideas.

There is no choice but to consider that Bernard is anticipating this way of reasoning which Peirce later called *abduction*. And as Bernard (1957: 33) goes deeper ahead: "*À propos* of a given observation, no rules can be given for bringing to birth in the brain a correct and fertile idea that may be a sort of intuitive anticipation of successful research. …its appearance is wholly spontaneous, and its nature is wholly individual." Unlike Popper, the context of discovery is indeed relevant to the methodology of the science of the French physician. Clearer cannot express it Bernard in the following sentence: "Discovery, then, is a new idea emerging in connection with a fact found by chance or otherwise. Consequently, there can be no method for making discoveries." (p. 35) "when one calls a new fact a discovery, the fact itself is not the discovery, but rather the new idea derived from it." (53) But, as we said at the beginning, Bernard was a complete stranger to Popper.

In relation to the problem of induction we are still more astonished that Popper only refers to Bacon and Mill. Moreover, these references are not only indirect –Popper does not cite them directly– but they are also completely erroneous, for instance as he reproaches Mill that he makes no concession to the deductive method, for in Mill the deductive method is precisely the key to understanding the role of the inductive method and at some point Mill even suggests the methodological importance of the *modus tollens*, although without explicitly mentioning it.

It is very strange indeed that Popper never mentions any work of Bacon, in particular, he **never mentions** the *Novum Organum*. And the same thing happens with Mill, of whom Popper **never mentions** his *System of Logic*. In his *Intellectual Autobiography* Popper attributes to Bacon a naive inductivism and reproaches him to adhere to an inductivism of collection of observations (Popper 1974*a*: 62). Amazingly, Bacon (1620: First Book, 105) himself claims precisely that induction by simple enumeration of cases is a childish affair: "The induction which proceeds by simple enumeration is puerile, leads to uncertain conclusions, and is exposed to danger from one contradictory instance, deciding generally from too small a number of facts, and those only the most obvious."

Popper attacks Francis Bacon in *The Two Fundamental Problems*, pp. 45-46 (German, p. 42): "*Naive inductivism* –prior to Hume– readily affirms the existente of universal empirical statements. Bacon believes in *inductio vera*, a scientific method that in principle is capable of establishing, through systematic generalization, true and universally valid laws (…) This is the position against which Hume's arguments are actually directed. It seems to me that this position has been finally overcome by Hume (in spite of Mill) and will not, therefore, be treated any further in this analysis." The reproach of naive inductivism, attributed to Bacon, reappears again in *The Two Fundamental Problems* p. 286 (German, p. 264): "The interpretation attempted in the present section (the '*naive*' *interpretation of pseudo-statements*) corresponds to naive inductivism (Bacon): this attempt is also defeated by an infinite regression. And the attempt to avoid this regression leads (…) to an *apriorist* solution."

Popper's position is unwise. He fails to consider the differences between the inductivist approaches of Aristotle and Bacon. Incidentally, the proper name *Aristotle* appears mentioned in *The Two Fundamental Problems* only on a single occasion, throughout the book. Indeed in p. 22 (German, page 20) we read: "classical logic is purely deductivist; inductivist reasoning has placed a very minor part (in spite of various attempts going back to Aristotle and perhaps to the Socratic method)."

Bacon and Mill are mentioned together in Chapter XII, *Conclusion*, of *The Two Fundamental Problems*, where on p. 355 (German, page 323), we read: "*The theory of induction* of Bacon (and Mill): Induction in an epistemological sense is rejected: there is no inductive rational method." By the way, in *The Two Fundamental Problems*, Table II, p. 366 (German, p. 332), in which he presents the relationships between different positions in the theory of knowledge, Sir Karl again places Bacon and Mill together as representatives of a *naive inductivism*, whose thesis would be: "There is empirical verification of natural laws."

However, in *The Two Fundamental Problems*, p. 315 (German, p. 288) Popper claims that "Bacon confused theory formation with metaphysics: by appealing to the evidence of the senses, he refused to give up his geocentric convictions." Indeed, as I point out in Rivadulla (2003: 40-41), Bacon (1620, Second Book II, p. 178) refuses to recognize the Earth's rotating movement: "[Galileo] has, however, imagined this data which can not be conceded (namely, the earth's motion)", and this raises the suspicion that Popper would know more about Bacon than what he hints.

As far as Stuart Mill is concerned, the following quotation gives us an idea of Popper's image of Mill: "Theories of Knowledge may have either a *deductivist* or an *inductivist* orientation, depending on how they assess the significance of deduction (logical derivation) and of induction (generalisation) (…) Radical inductivist positions (such as Mill's) *deny that deduction has any significance at all*; for, it is argued, what can be deduced is only that which induction has originally placed in the major premises. But even intermediate positions (such as that of Jevons), which seeks to characterise the empirical-scientific method as a synthesis of induction and deduction, will be rejected here as 'inductivist'. The deductivist view advocated here denies that induction has any significance." (Popper, *The Two Fundamental Problems*: 8. German, 7-8. My emphasis, A.R.) (He practically literally returns to these ideas on p. 469 (German, 425)). Popper perseveres in this position on Mill in *The Two Fundamental Problems*, p. 22 (German, p. 20): "Notwithstanding Mill, who further developed Bacon's and Herschel's approaches, attempts to develop a *logic of induction* have not succeeded in dislodging the theory of deduction from its dominant position in logic."

Popper mentions Mill also in the 1958 *Preface* of the English edition of *LSD*, and in note 3, p. 36, in an inconsequential reference. He also treats Mill in * IX, 1958, *A Third Note on Degree of Corroboration or Confirmation*, where he also refers to Whewell, twenty-five years after the publication of *L.F.*

4. Where did the Popperian falsificationism come from?

Clearly it comes from Einstein, as Popper (1974*a*: 29) points out in his *Intellectual Autobiography*: "what impressed me most was Einstein's own clear statement that he would regard his theory as untenable if it should fail in certain tests. Thus he wrote, for example: 'If the redshift of spectral lines due to the gravitational potential should not exist, then the general theory of relativity will be untenable.'"[7] And Popper (1974*a*: ibid.) continues: "Einstein was looking for crucial experiments whose agreement with his predictions would by no means establish his theory; while a disagreement, as he was the first to stress, would show his theory to be untenable.

This, I felt, was the true scientific attitude. It was utterly different from the dogmatic attitude which constantly claimed to find 'verifications' for its favourite theories."

The origins of Popperian fallibilism are linked to this recognition: "At that time I read through Einstein's writings ... What I did find was his paper *Geometrie und Erfahrung*, in which he wrote[8]:

> 'In so far as the statements of mathematics speak about reality, they are not certain, and in so far as they are certain, they do not speak about reality.'
> At first, I generalized from mathematics to science in general[9]:
> 'In so far as scientific statements speak about reality, they are not certain, and in so far as they are certain, they do not speak about reality.'
> (…)
> This idea of uncertainty or of the fallibility of all human theories, even of the best-corroborated ones, I later called 'fallibilism'. (To the best of my knowledge, Charles Sanders Peirce was the first who used this term.)" (Popper, *The Two Fundamental Problems,* XXV. German: XX-XXI)

Indeed in *The Two Fundamental Problems*, p.10 (German, p. 10) Popper already affirmed: "Einstein's statement that 'In so far as the statements of mathematics speak about reality, they are not certain, and in so far as they are certain, they do not speak about reality' may be generalized (if we replace 'not certain' by 'falsifiable' or 'refutable') into the following definition of empirical science: in so far as scientific statements speak about reality, they must be falsifiable, and in so far as they are not falsifiable, they do not speak about reality." This lead Popper (1959: 86) to propose following definition in §21: *Logical Investigation of Falsifiability*: "A theory is to be called 'empirical' or 'falsifiable' if it divides the class of all possible basic statements unambiguously into the following two non-empty subclasses. First, the class of all those basic statements with which it is inconsistent (or which it rules out, or prohibits): we call this the class of the *potential falsifiers* of the theory; and secondly, the class of those basic statements which it does not contradict (or which it 'permits'). We can put this more briefly by saying: a theory is falsifiable if the class of its potential falsifiers is not empty." With this basic conception Popper (1966: 260) could maintain that: "the method of science is rather to look out for facts which may refute the theory. This is what we call testing a theory –to see whether we cannot find a flaw in it."

But if Popper had read John Herschel, he would have found that although Herschel argued that knowledge of the near causes of phenomena takes place inductively, he (1830: 172) also held that "our next business is to examine deliberately and *seriatim* all the cases we have collected of its occurrence, in order to satisfy ourselves that they are explicable by our cause." A methodological compromise that Herschel reiterates in (1830: 176. My emphasis): "our next step in the verification of an induction must therefore consist in *extending* its application to cases not originally contemplated: in studiously varying the circumstances under which our causes act, with a view to ascertain whether their effect is general; *and in pushing the application of our laws to extreme cases*." Herschel's inductivism is not, of course, naive, radical or dogmatic: "it is very important to observe, that the successful process of scientific inquiry demands continually the alternate use of both the *inductive* and *deductive* method." (Herschel 1830: 184) In fact, Herschel insists (1830: 189) that "the inductive

[7] See Albert Einstein (1920, p. 132). German first edition: *Über die spezielle und die allgemeine Relativitätsheorie*. Brauchschweig: Vieweg, 1917. But also Einstein (1920: 43, 75 and 104) gives indications of the need to subject theory to the control of experience.
[8] [Albert Einstein, "Geometrie und Erfahrung" 1921, pp. 3 f.]
[9] [Karl Popper, "Ein Kriterium des empirischen Charakters theoretischer Systeme", *Erkenntnis* **3** (1933), p. 427.]

and deductive methods of enquiry may be said to go hand in hand, the one verifying the conclusions deduced by the other; and the combination of experiment and theory, which may thus be brought to bear in such cases, forms and engine of discovery infinitely more powerful than either taken separately."

Herschel (1830: 187) makes also use of the term *empirical law*. From these empirical laws he states that they are "*unverified inductions*, and are to be received and reasoned on with the outmost reserve …; and frequently, when afterwards verified theoretically by a deductive process, turn out to be rigorous laws of nature."

But Herschel (1830: 218.My emphasis, A.R.) even advances the notion of a *crucial experiment*[10] in situations of contrasting theories. In fact, he maintains that "When two theories run parallel to each other, and each explains a great many facts in common with the other, *any experiment which affords a crucial instance to decide between them, or by which one or other must fall, is of great importance*." That is, a hundred years before Popper, the foundations of the critical methodology of science were already being taken up in the Western philosophy of science.

However, the demand for falsifiability or refutability in the methodology of science is present in the Western philosophy still long before Herschel. In the Middle Ages the founder of the Franciscan School of Oxford, Robert Grosseteste (1168-1253), already advocated the falsification, via *modus tollens*, of hypotheses involving false consequences, in order to eliminate the maximum possible competing hypotheses. Following A. C. Crombie, John Losee (1980: 36-37) presents an application of the modus tollens argument which led Grosseteste to falsify the hypothesis that the sun generates heat by conduction. In the contemporary historiography there is a debate about whether experimentalism in the modern sense was already introduced by Grosseteste, but what is indubitable is that already in the thirteenth century Robert Grosseteste was in favour of the methodological advantages of falsificationism, and in general of the emergency of experimental science. For instance, McEvoy (1982: 207) asserts that "[Grosseteste] applied his theoretical principles of methodology to geometrical optics, using experiment and observation to verify or falsify his hypotheses." Grosseteste was certainly an inductivist: "the universal can only be arrived at by way of induction." But, as a good Aristotelian, he argued that the role of demonstrative reasoning is essential to science as well: "The absence of the universal in the understanding removes the possibility of demonstration, which can only begin from universals; and if there can be no demonstration, it follows that there can be no scientific knowledge."[11]

Centuries later, in the eighteenth century, Immanuel Kant (1781: 669) also rejected the possibility of inferring or demonstrating the truth of a hypothesis from the knowledge of its consequences; on the contrary by means of *modus tollens* "if even only a single false consequence can be derived from a proposition, then this proposition is false."

But if Popper had also known Claude Bernard he would have had to resign himself to the evidence that the nineteenth-century French epistemologist clearly anticipated his falsificationist methodology of science. Indeed Bernard (1957: 23.My emphasis, A. R.) claimed: "[the experimenter] must submit his idea to nature and *be ready to abandon*, to alter or to supplant *it*, in accordance with what he learns from observing the phenomena which he has induced." And on p. 32 he reiterates this same idea: "[the experimenter] makes suppositions about the cause of actions taking place before his eyes; and to learn whether the hypothesis which serves as groundwork for his interpretation is correct, he takes measures to make facts appear which in the realm of logic may be *either the confirmation or the negation of the idea which he has conceived*."

Bernard anticipates what Popper would say many decades later, especially he advances the idea, which will also advance Einstein, that we must be willing to rule out our theories, if nature denies them. That is, our theories must be falsifiable, and the scientific method must be that of conjectures and refutations. Confidence in the principles of science does not prevent scientists "from testing and verifying them by direct observation." (Bernard 1957: 36) In fact: "it is better to know nothing than to keep in mind fixed ideas based on theories whose confirmation we constantly seek, neglecting meanwhile everything that fails to agree with them." (p. 37) "we must never make experiments to confirm our ideas, but simply to control them; which means, in

[10] Which, as we shall see, also advances Claude Bernard.
[11] Grosseteste quoted by McEvoy (1982: 330) from Gilson.

other terms, that one must accept the results of experiments as they come, with all their unexpectedness and irregularity." (p. 38) "The theories which embody our scientific ideas as a whole are, of course, indispensable as representations of science. … But as these theories and ideas are by no means immutable truth, *one must always be ready to abandon them*, to alter them or to exchange them as soon as they cease to represent the truth." (p. 39. My emphasis, A. R.) "when we have put forward an idea or a theory in science, our object must not be to preserve it by seeking everything that may support it and setting aside everything that may weaken it. On the contrary, *we ought to examine with the greatest care the facts which apparently would overthrow it*, because real progress always consists in exchanging an old theory which includes fewer facts for a new one which includes more." (Bernard 1957: 40-41. My emphasis, A.R.)

The reason for Bernard's fallibilist and falsificationist methodology of science lies in the conviction that "All the theories which serve as starting points … are true only until facts are discovered which they do not include, or which contradict them. When these contradictory facts are shown to be firmly established, …, experimenters … hasten … to modify their theory, because they know that this is the only way to go forward and to make progress in science." (49-50) For this reason, "*When experiment disproves his preconceived idea, the experimenter must discard* or modify *it*. But even when experiment fully proves his preconceived idea, the experimenter must still doubt; for since he is dealing with an unconscious truth, his reason still demands a counterproof." (52. My emphasis, A.R.)[12] And Bernard focuses on counterproof in section VIII of ch. II: "Counterproof decides whether the relation of cause to effect, which we seek in phenomena, has been found. To do this, it removes the accepted cause, to see if the effect persists, relying on that old and absolutely true adage: *sublata causa, tollitur effectus*. This is what we still call the *experimentum crucis*." (55-56) Even the expression *experimentum crucis* is advanced by Bernard! "Indeed, proof, in science, never establishes certainty without counterproof. …experimental counterproof constitutes the scientific feeling *par excellence*." (p. 56)

Popper is naturally forgiven of his ignorance of this epistemological precedent, since he was unaware of the existence of Claude Bernard. But what is truly astonishing is that Popper was unaware that Stuart Mill himself –of whom Popper (1974*a*: 93) recognizes in his *Intellectual Autobiography* that he had already anticipated the concept of causal or deductive explanation, a concept Popper himself would also advance, more than twenty years before Hempel, in his *Logik der Forschung*, 1934, § 12– had also been ahead of him in the requirement of the deductive hypothesis testing. In his reflections on the Deductive Method in Book III, Chapter XI, *Of the Deductive Method*, Mill (1843: 304) claims that "To the Deductive Method, thus characterised in its three constituent parts, Induction, Ratiocination, and Verification, the human mind is indebted for its most conspicuous triumphs in the investigation of nature."

For Mill (1843:303-304) induction is subordinated to deduction: "If direct observation and collation of instances have furnished us with any empirical laws of the effect,… the most effectual verification of which the theory could be susceptible would be, that it led deductively to those empirical laws." And: "To warrant reliance on the general conclusions arrived at by deduction, these conclusions must be found, on careful comparison, to accord with the results or direct observation wherever it can be had. If, when we have experience to compare with them, this experience confirms them, we may safely trust to them in other cases of which our specific experience is yet to come. But if our deductions have led to the conclusion that from a particular combination of causes a given effect would result, then in all known cases where that combination can be shown to have existed, and *where the effect has not followed, we must be able to show* (…) *what frustrated it: if we cannot, the theory is imperfect, and not yet to be relied upon*." (Op.cit: 303-304. My emphasis, A.R.). This clearly shows the commitment of Mill with the scientific methodology of the deductive test of hypotheses, ahead of Popper in about a hundred years.

But even William Whewell (1794-1866) was himself committed almost hundred years before Popper with the idea of falsifiability of the proposed hypotheses: "What is requisite is, that the hypothesis should be close to the facts, and not connected with them by other arbitrary and untried facts; and that *the philosopher should be ready to resign it as soon as the facts refuse to confirm it*." (Part Two, Book XII, Chapter XIII, pp. 276-

[12] González Recio (2004) also acknowledges the anticipation by Claude Bernard of falsificationism in the methodology of science.

277) [13] This idea is justified later, when Whewell (Part Two, Book XII, Chapter XIII, p. 287.My emphasis, A.R.) states that "when the validity of the opinion adopted by us has been repeatedly confirmed by its sufficiency in unforeseen cases, so that all doubt is removed and forgotten, the theoretical cause takes its place among the realities of the world, and becomes a *true cause*." And immediately adds Whewell: "Newton's Rule then, to avoid mistakes, might be thus expressed; that 'we may, provisorily, assume such hypothetical cause as will account for any given class of natural phenomena; but that when two different classes of facts lead us to the same hypothesis, we may hold it to be a *true cause*.' And this Rule will rarely or never mislead us. There are no instances, in which a doctrine recommended in this manner has afterwards been discovered to be false." (Part Two, Book XII, Chapter XIII, p. 2876)[14]

Continuing with the British tradition, which he obviously belongs to, the logician, economist and philosopher of science Stanley Jevons (1835-1882) also considers that the deductive test of hypotheses is essentially part of the inductive methodology of science. This is firmly expressed by Jevons (1873: 525): "Inductive investigation … consists in the union of hypothesis and experiment, deductive reasoning being the link by which experimental results are made to confirm or confute the hypotheses."

But if there is a philosopher of science who, like those already mentioned, not only anticipates Popper in the idea of the testability of hypotheses but also in the modernity of the problematic around their empirical falsification, that is the French physicist, historian and philosopher of science Pierre Duhem (1861-1916). Now, Duhem (1954: 168. French[15], 1906: 274) maintains a holistic position on physics: "A physical law is a symbolic relation whose application to concrete reality requires that a whole group of laws be known and accepted." That is why, in Part II, Chapter VI, 'Physical theory and Experiment', Duhem labels §II as follows: An experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group. The falsificationist methodology is more than evident in Duhem (1954: 184. French, 1906: 302) when trying to test a theory: "A physicist disputes a certain law; he calls into doubt a certain theoretical point. How will he justify these doubts? How will he demonstrate the inaccuracy of the law? From the proposition under indictment he will derive the prediction of an experimental fact; he will bring into existence the conditions under which this fact should be produced; if the predicted fact is not produced, the proposition which served as the basis of the prediction will be irremediably condemned." Now, the procedure is much more complex than it may seem in the light of these words, for as Duhem immediately points out (1954:185. French, 1906:303-304), "A physicist decides to demonstrate the inaccuracy of a proposition; in order to deduce from this proposition the prediction of a phenomenon, …, he does not confine himself to making use of the proposition in question; he makes use also of a whole group of theories accepted by him and beyond dispute. The prediction of the phenomenon, whose nonproduction is to cut off the debate, does note derive from the proposition challenged if taken by itself, but from the proposition at issue joined to that whole group of theories; if the predicted phenomenon is not produced, not only is the proposition questioned at fault, but so is the whole theoretical scaffolding used by the physicist. The only thing the experiment teaches us is that among the proposition used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where this error lies is just what it does not tell us. The physicist may declare that this error is contained in exactly the proposition he wishes to *refute*, but is he sure it is not in another proposition?"[16] And Duhem concludes (1954: 187. French, 1906: 307): "In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his prediction, what

[13] In fact, for Whewell the hypothesis testing is already included in the induction itself, for example, when he asserts that "in the task of induction, we employ clear ideas, rigorous reasoning, and close and fair comparison of the results of the hypothesis with the facts. These are, no doubt, important and fundamental conditions of a just induction." (Part Two, Book XII, Chapter XIII, p. 288)

[14] Now, there are many examples against. For example, as I say in Rivadulla (2004: 95), sometimes, from a new theory, questions are raised, that the old theory should respond satisfactorily, if it pretends to be true, but it can not solve; in these situations the old theory is empirically refuted. This is the case of tests that refute MN –the advance of Mercury perihelion and the deviation of light by the Sun– that nevertheless confirm *TGR*, and new tests: gravitational redshift and gravitational waves, also surpassed by *TGR*, but absolutely unsuspected from the point of view of *MN*.

[15] Hereafter 'French' means the original edition of Duhem's book.

[16] The word *refute* is not an interpretation of the English translator. It appears in the French original: "Le physician declare-t-il que cetter erreur est précisément contenue dans la proposition qu'il voulait réfuter et non pas ailleurs?"

he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed." It is not surprising, then, that in Section 3 of Chapter VI Duhem states that "*A 'Crucial Experiment' is impossible in Physics*", precisely against the author of the *Novum Organum*.

5. Popper's role in the development of the contemporary theory of scientific explanation

For two thousand and four hundred years, since Plato and Aristotle, the scientific philosophical thought of the West has largely focused on the concept of causation: what does it mean to say that something is the cause of something, and, above all, whether the causes of observed phenomena are capable of being known. The great contribution of the twentieth century philosophy of science was to make explicit the form of the argument of scientific explanations. Popper's contribution to this work was decisive. Popper (1959: §12) claimed indeed that "To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal *laws*, together with certain singular statements, the *initial conditions*." And he adds (1959: 61) that "The 'principle of causality' is the assertion that any event whatsoever *can* be causally explained –that it *can* be deductively predicted."[17] Since, according to Popper, "It is from universal statements in conjunction with initial conditions that we *deduce* the singular statement", Popper (1979 § 11, p. 86, note *2) obviously anticipated Hempel's D-N model of scientific explanation and Hempel (1965: 337, note 2) himself acknowledged it reluctantly.

However, that scientific explanation takes the form of a deductive argument has a long tradition in the Western philosophy of science. We can go back, of course, to Aristotle. But it is John Stuart Mill, who, in the nineteenth century, truly intuited the modern form of the argument of any scientific explanation. Indeed, as he stated in Chapter XVI, § 1 "Of Empirical Laws," p. 339: "The ascertainment of the empirical laws of phenomena often precedes by a long interval the explanation of those laws by the Deductive Method; and the verification of a deduction usually consists in the comparison of its results with empirical laws previously ascertained." Or as he claimed two pages later: "all results obtained by the Method of Agreement (and therefore almost all truths obtained by simple observation without experiment) must be considered, until either confirmed by the Method of Difference or explained deductively, …, accounted for *à priori*." It is interesting to note, first, the importance Mill grants to the deductive method, and secondly that Popper himself, in his *Intellectual Autobiography*, acknowledges that Mill had already anticipated his model of scientific explanation: "In section 12 of *Logik der Forschung* I discussed what I called 'causal explanation', or deductive explanation, a discussion which had been anticipated, without my being aware of it, by J. S. Mill, though perhaps a bit vaguely (because of his lack of distinction between an initial condition and a universal law)" (Popper 1974*a*: 93)

John Stuart Mill agrees with John Herschel both in the existence in science of *empirical laws* and in the necessity that such empirical laws receive a scientific explanation. Indeed, in Chapter XVI. Of empirical Laws, §1, Mill manifests himself in this respect as follows: "Scientific inquirers give the name of Empirical Laws to those uniformities which observation or experiment has shown to exist, but on which they hesitate to rely in cases varying much from those which have been actually observed, for want of seeing any reason *why* such a law should exist. It is implied, therefore, in the notion of an empirical law, that it is not an ultimate law; that if true at all, its truth is capable of being, and requires to be accounted for. It is a derivative law, the derivation of which is not yet known. To state the explanation, the *why,* of the empirical law, would be to state the laws from which it is derived; the ultimate causes on which it is contingent…

[17] And in note 1* of this section Popper (1959: 61) clarifies years later: "I feel that I should say here more explicitly that the decision to search for causal explanation is that by which the theoretician adopts his aim –or the aim of theoretical science. His aim is to find *explanatory theories* (if possible, *true* explanatory theories); that is to say, theories which describe certain structural properties of the world, and which permit us to deduce, with the help of initial conditions, the effects to be explained. (…) I therefore wish to make it quite clear that I consider the theorist's interest in *explanation* –that is, in discovering explanatory theories– as irreducible to the practical technological interest in the deduction of predictions."

The periodical return of eclipses, as originally ascertained by the persevering observation of the early Eastern astronomers, was an empirical law until the general law of the celestial motions had accounted for it." (p. 338) Obviously: "it is the very meaning of an empirical law that we do not know the ultimate laws of causation on which it is dependent." (p. 341)

And in a form that seems to anticipate the idea of initial conditions, Mill explains: "To deduce the laws of the heavenly motions, we require not only to know the law of a rectilineal and that of a gravitative force, but the existence of both theses forces in the celestial regions, and even their relative amount. The complex laws of causation are thus resolved into two distinct kinds of elements: the one, simpler laws of causation, the other (in the aptly selected expression of Dr. Chalmers) collocations; the collocations consisting in the existence of certain agents or powers, in certain circumstances of place and time." (p. 306)

Wesley C. Salmon (1984: 21, note 4) thinks about Popper's anticipation of the DN model that "Although Popper's *Logik der Forschung* (1935) contains an important anticipation of the D-N model, it does not provide as precise an analysis as was embodied in (Hempel and Oppenheim, 1948). Moreover, Popper's views on scientific explanation were not widely influential until the English translation (Popper, 1959) of his 1935 book appeared. It is for these reasons that I chose 1948, rather than 1935, as the critical point of division between the history and the prehistory of the subject." This seems reasonable. But what is indisputable is that Popper has already advanced very clearly to the Hempel-Openheimer model of scientific explanation and that even Popper had the decency to recognize that this model was already in germ at Mill. (This naturally has a second reading not so favourable for Popper: his criticisms of the supposed naive Millian inductivism are not supported if Popper himself has to acknowledge the importance of deductive reasoning in Mill's methodology of science.)

6. Was Popper in his early days a realist philosopher of science?

The term *realism* –in a form we might call *ontological*– appears in opposition to idealism in *Beiden Grundprobleme*, p. 73 and again on pp. 437-438 (*Zusammenfassender Auszug*, IX), where Popper presents six theses of a so-called *methodological realism*. Popper concludes this section IX with the statement that the thesis that *there are laws of Nature* is *epistemologically equivalent to realism*. These laws of Nature or regularities is what we should look for if we want to know, maintains Popper in *Two Fundamental Problems* §10. This makes one suspect that scientific realism has certainly been present in Popper's thinking ever since.

However the term *realism* never appears in *LSD*: neither in the main text nor in the appendices. Nonetheless we may find serious glimpses of an underlying realistic position, e.g. in Chapter III: *Theories*, where literally Popper (1959: 59) states that "Theories are nets cast to catch what we call 'the world': to rationalize, to explain, and to master it. We endeavor to make the mesh ever finer and finer." An of course in *LSD* § 85: The Path of Science, where Popper (1959: 278) claims that "the striving for knowledge and the search for truth are still the stronger motives of scientific discovery"[18]. Well known is Popper's recognition of Tarski's correspondence theory of truth in Sections 20 and 32 of his *Intellectual Autobiography*.[19]

In any case, a fully fledged scientific realism had already been defended by Claude Bernard, seventy years before Popper: "Men of science learn every day from experience; by experience they constantly correct their scientific ideas, their theories; rectify them, bring them into harmony with more and more facts, and so come nearer and nearer to the truth." (Bernard 1957: 12) Or as he says later: "The experimental method is concerned only with the search for objective truths." (p. 28) And also: "Experimental reasoning is the only reasoning that naturalists and physicians can use in seeking the truth and approaching it as nearly as possible." (p. 31) (Is not the quest for objective truth the regulatory idea of scientific practice for Popper?) We might almost dare to say that Bernard is ahead of Poincaré in what, since the late twentieth century came to be

[18] In German this was expressed in a slightly different way: "so ist doch das intellektuelle Streben, der Wahrheitsantrieb, wohl der stärkste Antreib der Forschung" ("The intellectual striving, the drive to truth, is the most powerful impulse of research")

[19] In his contribution to this volume, Luis Fernández Moreno criticises Popper's claim that Tarski has rehabilitated the theory of truth as correspondence and the notion of absolute truth.

known as *structural realism*: "In teaching man, experimental science results in lessening his pride more and more by proving to him every day that primary causes, like the objective reality of things, will be hidden from him forever and that *he can know only relations*. Here is, indeed, the one goal of all the sciences." (p. 28. My italics) A structural realism which, in any case, is not dogmatic but conjectural or fallible and falsifiable in the purest sense sustained many decades later by Popper: "we must relieve in science, i. e.., in determinism; we must relieve in a complete and necessary relation between things,…; but at the same time we must be thoroughly convinced that we know this relation only in a more or less approximate way, and that the theories we hold are far from embodying changeless truths. *When we propound a general theory in our sciences, we are sure only that, literally speaking, all such theories are false*.[20] They are only partial and provisional truths which are necessary to us, as steps on which we rest, so as to go on with investigation; they embody only the present state of our knowledge, and consequently they must change with the growth of science." (pp. 35-36)

And here is the central idea of Claude Bernard's critical scientific realism: "if we mean to find truth, we can solidly settle our ideas only by trying to destroy our own conclusions by counter-experiments." (56) "with the help of reasoning and of experiment [experimenters] try to connect natural phenomena … with their immediate causes." (p. 57)

7. Where does the idea of *instrumentalism* come from in the current philosophy of science?

The term *instrumentalism* certainly is conspicuous by its absence in *Beiden Grundprobleme*. A *strong positivism* is criticized in § 8 and pp. 323-324 and 378-37 and also the *pragmatism*, in p. 155, but without identifying any authors. The same occurs on pp. 324-325.

In *LSD* § 4 Popper identifies some 'older positivists', but in general he understands positivism in the form of neo-positivism, as for example in the second paragraph of § 9 and in §10 and §73 "Heisenberg's Programme and the Uncertainty Relations", and note *4 of §78.

Regarding instrumentalism, however, it is interesting that in a note following the publication of *LF*, note * 1, p. 59, of Chapter III: *Theories*, Popper claims to have designated as Instrumentalism the view represented in Vienna by Mach, Wittgenstein and Schlick, namely: "the view that a theory is *nothing but* a tool or an instrument for prediction" and he states that he analyzed and criticized this point of view in his articles of 1953 and 1956 compiled in *Conjectures and Refutations* of 1963. And in note * 4, p. 36, from Section 4: *The Problem of Demarcation* of *LSD* he claims that the instrumentalist tradition can be traced back to Berkeley, and further.

In other words, it is far from impossible that it was precisely Popper who introduced the name and concept of instrumentalism in the current philosophy of science.[21]

8. Conclusion

I think it is not unfair to affirm that the information that Popper accredits of contemporary European philosophers of science and of philosophically relevant contemporary scientists is very scarce. Popper seems to ignore of the philosophy of the science of his time more than he credits to know.

What would have been of Popper's epistemology and of the philosophy of science in general if Sir Karl had been aware of Claude Bernard's scientific methodology? Indeed, Popper was unaware of the existence of the French epistemologist. But already seventy years before him, Bernard had developed: 1. A perfectly argued theory of critical realism; 2. A view on the theory-load of the observational base; 3. A theory of induction with suggestive relations to abduction –naturally, for obvious reasons, not known as such by Bernard– and a vindication of the context of scientific discovery; and 4. A falsificationist methodology of empirical science that Popper himself could have made perfectly his own.

[20] This is very interesting indeed. Is not this the idea underlying the comparison of theories by their verisimilitude in the Popperian methodology of science?

[21] I suggest this idea in Rivadulla (2015: Chap. VI, § 3.)

What could have been and was not also applies in relation to the Austro-Hungarian doctor Ignaz Semmelweiss (1818-1865). Semmelweiss worked as an obstetrics assistant at the Vienna General Hospital between 1845 and 1855, when he returned to Budapest to take up the obstetrics chair at the university. Semmelweis's discovery during his stay in Vienna that puerperal fever was produced by the lack of hygiene of the gynaecologists is widely recognized by contemporary philosophers of science as an excellent example of abductive inference. Popper could have heard of him.

All the bibliography Popper mentions in *The Fundamental Problems* is in German. Even the one originally published in English is quoted from translations into German. In any case, this does not excuse him from calling ingenuous or radical inductivists to Bacon and Mill. In addition, the great question remains: What would have been of his anti-inductivism if he had known Fisher's inductive conception of the theory of inferential statistics? In any case, an important part of the problem of induction had already been solved by the British philosophers of the nineteenth century, almost a hundred years before Popper had offered his own proposal. Could not even Popper's anti-inductivism be described as radical or even naive? Not to mention that his later attempts to prove the impossibility of inductive probability failed disastrously (Cf. Rivadulla 1987 and 1994).

As far as the refutationist or falsificationist methodology of science is concerned, what made Popper produce a breakthrough in the philosophy of science was the emphasis he placed on that science must consciously seek the refutation of tentative hypotheses, that the scientific method is that of conjectures and refutations. But this methodology was already completely familiar both in insular and continental Europe for scientists like Bernard, Herschel, Jevons, Whewell, Stuart Mill and Pierre Duhem.

In any case, neither of these names is mentioned in Carnap's *Logical Structure of the World*, although Mill and Duhem are among the aforementioned authors who were read and discussed by the members of the Vienna Circle. Should we then excuse Popper by his ignorance of all the falsificationist philosophy of science that had preceded him?

It is unquestionable that Popper clearly advanced Hempel-Openheimer's model of scientific explanation. Now, as we have seen in Section 5, Popper himself acknowledged that the modern form of deductive explanation, before him, had already been anticipated by Stuart Mill. Thus Popper's criticisms of Mill's supposed naive inductivism are unjustified as Popper himself should have recognized the importance of the deductive reasoning in Mill's methodology of science.

Finally, I leave the readers of this article to personally decide on the lights and shadows that exist in the origins of the Popperian philosophy of science.

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