INDUCTIVE PROBABILITY AND SCIENTIFIC RATIONALITY*

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1. Introduction

Science has been considered more often than not as an overwhelmingly rational activity: Its main theoretical goal, the search for truth, is a rational one, what is possible due to the fact that it has survival value; in the second place, in order to achieve this goal, science is committed to logic and empirical evidence, i.e. science has developped what is usually called the *scientific method*, whose application guarantees the rationality of the products of science (laws, theories, etc). Certainly, not all philosophers of science agree with this view of scientific rationality. Firstly, it does not reign unanimity about the goal(s) of science; secondly, there is neither a unique concept of truth that could be shared by all epistemologists, and, thirdly, there are even some philosophers, like Quine and Kuhn, who deny that logic and experience play any decisive role in the election of theories; their well known theses of the underdetermination of science and the scientific incommensurability constitute two very disputed questions in contemporary metascience.

But, since both: the problem concerning the goal(s) of science and its related truth concept *and* the challenge of underdetermination and incommensurability surpass to a great extent the scope of this article, I can only add, that those philosophers who believe in a *realistic* approach to the rationality of science, are obliged to contribute to the solution of these problems.

To repeat, this is a task which I do not want to account for here, but I think it is possible to explain science's rationality from an epistemological realistic point of view.

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*This paper has been written at the University of Frankfurt a.M. during a period as a fellow of the Alexander von Humboldt Stiftung.

Prof. Wilhelm K. Essler kindly read and commented this paper, and Mrs. Barbara Brüning corrected my English style. I am very indebted to both of them.

The main problem I am concerned with in the following, is that of the possibility of inductive probability. Its philosophical relevance can easily be seen, if you realize that scientific rationality is by no means linked to the existence of a unique particular scientific methodology. The scientific method can be characterized, as Harvey Siegel[1] points out, as the commitment to evidence. But this does not imply the existence of an indisputable particular scientific methodology. The history of modern epistemology supports this claim. The Popper-Carnap controversy can be seen as a good example of two different concepts of scientific methodology, both committed to the weight of evidence. Lakatos' (methodological) sophisticated falsationism tries to provide am improvement of Popper's theory of scientific rationality[2] and Stegmüller and can be partially understood as an attempt at rationalizing Kuhn's 'irrational' approach to science[3]. Carnap's inductive-probabilistic (Bayesian) methodology, Popper's methodology of conjectures and refutations, and Lakatos' sophisticated falsationism are certainly different methodologies; but this does by no means imply that science's commitment to evidence should have to be given up. Kuhn's case is somehow different, because[4] he is providing a sociological account of scientific growth, rather that an (epistemological) theory of scientific rationality.

I very recently -but also very briefly- posed the question[5] whether or not Popper's critical methodology should be preferred to the Bayesian one, in the sense that is secures the rationality of science better than the latter does. In the remaining pages I will discuss this problem in more detail.

Since Popper himself asked following questions[6]: (*i*) Who is right: those philosophers that assert that the positive result of the testing of a scientific hypothesis amounts to establishing its probability, or he, Popper, that denies this conception? and (*ii*) Is there any consistent (not probabilistic) measure for the assessment of hypotheses that have surmonted rigorous tests, a measure capable of differentiating between better and worse tested hypotheses?, the discussion of them will occupy us in the next two sections. Let me start with the second question.

2. The trouble with Popper's measure of the corroboration degree

Since it is beyond any doubt, that the measure Popper has in mind in question (*ii*) is his degree of the corroboration of scientific theories, I will try to answer it, step by step, dividing it in two subquestions.

2.1. *Is there some (Popperian) not probabilistic measure of the corroboration degree of scientific hypotheses?*

In an earlier paper[7] I have answered: Yes, there is. In fact, Popper's measure $C(h, e)$[8] for the degree of the corroboration of a scientific hypothesis h relative to a strong evidence e, which h has successfully explained, seems not to be -at the first sight- a probabilistic one, because the inferior and superior limit values it can assume are, respectively, -1 and + 1, whereas the corresponding values for the probability function $p(h,e)$ are, respectively, 0 and 1.

2.2. *Is $C(h, e)$ the measure of the discrimination between good and badly tested hypotheses, Popper looked for?*

The answer has to be negatively now. Popper's arguments of the *equiprobability of possibilities* and of the *independence of possibilities*[9] amount to claiming, that, if h is a universal hypotheses, then is $p(h) = 0$, and $p(h,e)=0$ too, even if e does confirm (or, in Popper's sense, corroborate) it. It follows then that

$$(1) \qquad C(h, e) = [1-p(e)] / [1+p(e)],$$

because of the fact, that, when $h \vdash e$, is $p(e,h)=1$ and $p(h \wedge e)=p(h)$.

(1) looks at the first sight very sound: since $p(e)$ measures the strength of the proof which h has been subjected to, the less strong the proof is, the more probable is prediction e by h, and the smaller the corroboration degree of h relative to e. And, on the other hand, when $p(e)$ is very small, i.e. it is *a priori* very unlikely that h will give any satisfactory account for the strong evidence e, then a successful explanation of e by h will approximate $C(h,e)$ very closely to 1.

Nevertheless an unnoticed, subtle and extraordinary switch has taken place, to wit: (1) is not -against Popper's wishes- a (not probabilistic) measure of the corroboration degree of a universal hypotheses, *but a probabilistic one*, because, depending on being $p(e)=0$ or $p(e)=1$, will be $C(h,e)=1$ or $C(h,e)=0$, respectively; i.e., what $C(h,e)$ really measures is the *probabilistic support* a more or less strong evidence e lends to h. And, since the determination of what counts as a strong or weak test for a given hypothesis, measured by $0 \leq p(e) \leq 1$, can only be a subjective (personal or collective) one, it might therefore be possible to give a Bayesian interpretation of (1).

Last, and because of the subjective character of the $p(e)$ assessment, it is very difficult to see how could (1) reflect the differences of corroborability among different universal hypotheses.

3. Probabilistic support and inductive probability

But Popper's failure to give a positive answer to question (*ii*), i.e. an answer in accordance with his underlying methodological intuitions, does not imply that question (*i*) should also be answered in the following terms: only those philosophers are right who think that the positive result of the test of a scientific hypothesis contributes to increase its probability to be true. For it is not justified, from the non-existence of a Popperian corroboration measure, to conclude the possibility of inductive probability. Furthermore, in order to arrive at this conclusion, it is firstly necessary to show whether or not Popper has been successful in proving the impossibility of inductive probability. Therefore, the content of this section has to be expounded in two steps.

3.1. *Has Popper proved the impossibility of inductive probability?*

Since the publication of his *Logik der Forschung* in 1934 Popper tried to show, in several arguments, the impossibility of inductive probability, i.e. to establish that an increasing amount of confirmatory evidence e of a given hypothesis h does by no means contribute to increase the probability of h to be true. As I have elsewhere[10] presented and discussed these arguments, I only want to analyze here the very recent defense by Donald Gillies of the Popper-Miller argument.

Gillies' defense[11] of Popper-Miller's *proof*[12] of the impossibility of inductive probability amounts to claiming that even if some evidence e probabilistically supports a given hypothesis h, it does not inductively support h.

Probabilistic support takes place, when a certain hypothesis h

has been empirically confirmed by some evidence e, i.e. when prediction e by h occurs, provided $h \vdash e$ and $1 > p(e) > p(h) > 0$. In this case obviously is $p(h, e) > p(h)$, and the difference

$$s(h, e) = p(h, e) - p(h)$$

measures the *probabilistic support* which e lends to h.

Now, since

$$\vdash h \leftrightarrow (h \vee e) \wedge (e \rightarrow h),$$

Gillies contents rightly that

$$s(h, e) = s(h \vee e, e) + s(e \rightarrow h, e)$$

[consider to this effect, that i) since $h \vdash e$, $p(h \wedge e) = p(h)$ and $p(h \vee e) = p(e)$, and ii) since $e \vdash h \vee e$, $p(h \vee e, e) = 1$], and as $s(h \vee e, e)$ represents purely deductive supports, then if probabilistic support -expressed by $s(h, e)$- were also inductive support, the term $s(e \rightarrow h, e)$, so Gillies, has to contain it. The trouble is, that whereas $s(h \vee e, e)$ is always positive, $s(e \rightarrow h, e)$ is always negave, because[13] $p(e \rightarrow h, e) < p(e \rightarrow h)$. Gillies[14] concludes: "It therefore follows that there cannot be inductive support of the kind that the Bayesians postulate".

Gillies' contention that the inductive support e lends to h must be contained in $s(e \rightarrow h, e)$ grounds, I think, on Popper-Miller's assumption that, since $h \vee e$ -the first factor of h- follows deductively from e, $e \rightarrow h$ -the second factor of h that does not follow deductively from e- has to contain all of h that follows inductively from e. Now, since it does not make any sense to affirm that what does not deductively follow from something has to follow inductively from it, there is no reason to except that $s(e \rightarrow h, e)$ contains the inductive support e confers to h. Following, Gillies' and Popper-Miller's argument do not prove anything. (So, I agree with Charles S. Chihara, when he contends, that the argument's conclusion: "it cannot be inductive support" is unjustified[15]).

From a pure probabilistic point of view, the only thing we are justified to say about $s(h, e)$ and $s(e \rightarrow h, e)$ is that, as logical consequences of our premises: $h \vdash e$ and $1 > p(e) > p(h) > 0$, is positively relevant to h but negatively relevant to $e \rightarrow h$. And, as we do not have any reason to believe that $e \rightarrow h$ follows inductively from e, then it is not justified to interpret the negative relevance of e to $e \rightarrow h$ as meaning that probabilistic support cannot be inductive support.

Finally we have not to be surprised by the fact that, although h and $e \rightarrow h$ are probabilistically equivalent given e, $s(h,e)$ is positive whereas $s(e \rightarrow h,e)$ is negative. For "h and $e \rightarrow h$ are not logically equivalent, and what is positively relevant for the one needs not to be so for the other"[16].

3.2. *Inductive probability and scientific methodology*

Let us then go ahead, and investigate what conclusions we can arrive at, when we take into account more than one single evidence favourable to h. If we assume that e_k and e_ℓ are different predictions of h, both fulfilling the same premises as above, then we obtain, from the definition of $p(h, e_k \wedge e_\ell)$, that

$$(1) \qquad p(h, e_\ell \wedge e_k) > p(h, e_k) \text{ iff } p(e_\ell \wedge e_k) > p(h).$$

Now, the left part (1) expresses the probabilistic support which new available data e_ℓ confer to h, and, since from a Bayesian-subjectivistic point of view probability means degree of belief, then, if we trust on the predictions of h more than on the truth of h itself, an increasing confirmation of these predictions will, according to (1), increase our confidence in h(i.e., the probability for h to be true).

On the other side, we have

$$p(h, e_\ell \wedge e_k) > p(h, e_k) \text{ iff } p(e_\ell, e_k) < 1.$$

And since

$$p(h, e_\ell \wedge e_k) > p(h, e_k) \text{ iff } p(e_\ell, e_k) > p(h, e_k),$$

it follows, that

$$p(h, e_\ell \wedge e_k) > p(h, e_k) \text{ iff } p(h, e_k) < p(e_\ell, e_k) < 1;$$

in other words,

$$p(h, e_\ell \wedge e_k) > p(h, e_k) \text{ iff } 0 < p(e_\ell, e_k) < 1.$$

But

$$0 < p(e_\ell, e_k) < 1 \text{ iff } p(e_k) > p(e_\ell \wedge e_k).$$

Following,

$$(2) \quad p(h, e_\ell \wedge e_k) > p(h, e_k) \text{ iff } p(e_k) > p(e_\ell \wedge e_k).$$

Taking together (1) and (2) we finally obtain that

$$(3) \quad p(h, e_\ell \wedge e_k) > p(h, e_k) \text{ iff } p(e_k) > p(e_\ell \wedge e_k) > p(h),$$

what seems to be very reasonable from the point of view of probabilistic support.

What result (3) is telling us is, that the less probable the predictions implied by are, the greater is the probability of to be true, if they occur. Very improbable (happened) predictions of a given hypothesis make it very probable, and the more probable the less probable they are. *The Bayesian concept of probabilistic support encompasses then Popper's methodological concept of corroboration.* But, if probabilistic support can approximate 1, provided all the available evidence confirms the hypothesis at issue, then there is no reason available why probabilistic support does not be also inductive support.

4. Conclusion

From section 3. it follows that probabilistic support cannot be distinguished from inductive probability, and from subsections 2.2. and 3.2. I conclude that Popperian critical methodology can be understood as a special case of Bayesianism. If this is right, I will have contributed to simplify the actual situation in the philosophy of science, where the scientific method seems to be accomplished by different scientific methodologies.

NOTES

1 *Cfr.* Siegel (1985)

2 *Cfr.* Rivadulla (1988c)

3 *Cfr.* Rivadulla (1986), pp. 272-273.

4 *Cfr.* Rivadulla (1988b)

5 *Cfr.* Rivadulla (1988a), pp. 60-62.

6 *Cfr.* Popper (1983), pp. 220-221

7 *Cfr.* Rivadulla (1987a), pp. 187

8 *Cfr.* Popper (1935), *IX

9 *Cfr.* Rivadulla (1989), Sections 2.1. and 2.2.

10 *Cfr.* Rivadulla (1987b) and (1989)

11 *Cfr.* Gillies (1986) and Chihara & Gillies (1988)

12 *Cfr.* Popper & Miller (1983) and (1984)

13 *Cfr.* Rivadulla (1987b), pp. 354-355

14 *Cfr.* Chihara & Gillies (1988), p. 3

15 *Cfr.* Chihara & Gillies (1988), p. 4

16 *Cfr.* Rivadulla (1987b), p. 357

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