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The Sessions on Induction and Probability at the 1935 Paris Congress: An overview

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Résumé : Le premier Congrès pour l'unité de la science (Congrès international de philosophie scientifique) qui s'est tenu à Paris en 1935 comprenait deux sessions, consacrées l'une à l'induction, l'autre aux probabilités. Des représentants éminents du mouvement pour une philosophie scientifique ont présenté des communications dans ces sessions : dans la première sont intervenus Hans Reichenbach, Moritz Schlick et Rudolf Carnap, dans la seconde, Reichenbach, Bruno de Finetti, Zygmunt Zawirski, Schlick et Janina Hosiasson, — dans cet ordre. Les sujets abordés concernaient la nature des lois scientifiques, le problème du sens et le principe de l'empirisme, ainsi que des questions connexes portant sur la confirmation des hypothèses scientifiques. Sur la nature des probabilités, les principales interprétations étaient représentées, à savoir le logicisme, le fréquentisme et le subjectivisme. Fut également examinée la possibilité de construire une logique probabiliste. Ce chapitre passe en revue les différentes contributions présentées dans ces deux sessions, à partir du texte publié dans les actes du Congrès.

Abstract: The First International Congress for the Unity of Science (Congrès international de philosophie scientifique) held in Paris in 1935 hosted two sessions devoted to "Induction" and "Probability" respectively. Outstanding representatives of the movement for scientific philosophy read papers in those sessions: the one on Induction hosted papers by Hans Reichenbach, Moritz Schlick, and Rudolf Carnap, while the one on Probability hosted papers by Reichenbach, Bruno de Finetti, Zygmunt Zawirski, Schlick, and Janina Hosiasson—in that order. The topics addressed concern the nature of scientific laws, the problem of meaning, and the principle of empiricism, together with the related issue of confirmation of scientific hypotheses. The nature of probability was also addressed, covering all major interpretations, namely logicism, frequentism, and subjectivism. The possibility of building a

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probability logic was also explored. In this paper, the contributions delivered at the sessions on Induction and Probability are surveyed, based on the version published in the fourth volume of the proceedings.

1 The first Congress for the Unity of Science

A peculiar feature of the First Congress for the Unity of Science (Congrès international de philosophie scientifique) held in Paris in 1935 is that it hosted two sessions devoted to "Induction" and "Probability" respectively. The other Congresses of Scientific Philosophy—five more were organized between 1936 and 1941—included only a few papers on the foundations of probability, read within sessions devoted to general methodology, but no session was expressly devoted to probability and/or induction. The session on Induction at the Paris Congress included papers by Hans Reichenbach, Moritz Schlick and Rudolf Carnap, while the session on Probability included papers by Reichenbach, Bruno de Finetti, Zygmunt Zawirski, Schlick and Janina Hosiasson—in that order. The proceedings of such sessions are published in volume IV of the *Actes du Congrès international de philosophie scientifique (Sorbonne, Paris 1935)* appearing in *Actualités scientifiques et industrielles*, n. 391, 1936 [Actes 1936].

The topics addressed concern the nature of scientific laws, the problem of meaning and the principle of empiricism, together with the related issue of confirmation of scientific hypotheses. The nature of probability was addressed, covering all major interpretations, namely logicism, frequentism, and subjectivism. The possibility of building a probability logic was also explored. In what follows, the contributions delivered at the sessions on Induction and Probability are surveyed, based on the version published in the fourth volume of the proceedings.

2 Schlick's contributions

2.1 Schlick's paper in the session on Induction

Schlick's paper belonging to the session on Induction, called "Sind die Naturgesetze Konventionen?" ["Are Natural Laws Conventions?"] [Schlick 1936a], tackles the vexed question of the status of scientific laws, arguing against the thesis that laws of nature are conventions. To start with, Schlick draws a distinction between conventions and genuine assertions, pointing out that "the validity of a convention is of our own making" [Schlick 1936a, 438]. He then clarifies that the question whether laws are conventions concerns natural laws, not the laws we encounter in logic and mathematics:

> For in logic and mathematics the symbols have precisely the meaning which is bestowed upon them by what is expressly written down or formulated in some other fashion. Mathematics and logic do not point beyond themselves; they do not transcend their own realm of symbols; here there is no fundamental difference between theorem and definition. [Schlick 1936a, 441]

The difference between logic and mathematics on the one hand and the empirical sciences on the other rests on the distinction between *sentence* [*Satz*] and *proposition* [*Aussage*], which plays a key role within Schlick's conception of a natural law. In Schlick's words: "we shall mean by 'sentence' a sequence of linguistic signs with the help of which something can be asserted" [Schlick 1936a, 441]; by contrast, "we shall mean by a 'proposition' such a sentence together with its *meaning*" where the "meaning" of a sentence is "the set of rules which are stipulated for the actual application of the sentence, that is, for the practical use of the sentence in the representation of facts. In short, a 'proposition' is a 'sentence' insofar as it actually fulfils the function of communication" [Schlick 1936a, 441–442].

Having drawn this distinction and made clear the difference between the laws belonging to the formal sciences and those occurring in the empirical sciences, Schlick examines in some detail two examples, the "energy principle" and Galileo's "law of inertia", arguing that they cannot be regarded as either definitions or sentences. On the one hand, they are not definitions because only facts can confirm or refute them; on the other, they are not sentences because their applicability to facts requires some meaning to be attached to them. The equations of physics, like the axioms of geometry, are "mere sentences": "each by itself is subject to arbitrary changes in formulation" [Schlick 1936a, 443]. By contrast

> [...] the proper content of a natural law may be seen in the fact that to certain grammatical rules (for instance, a geometry) some quite definite propositions correspond as true descriptions of reality. And this fact is completely invariant with respect to any arbitrary changes in notation. [Schlick 1936a, 443]

In conclusion, the meaning of scientific laws is given by the rules that state how they are to be used to describe facts, but while such rules are conventions, natural laws are not.

As Schlick emphasized,

> [...] all genuine propositions, as for instance natural laws, are something objective, something invariant with respect to the manner of representation, and not dependent in any way upon

> convention. What is conventional and, hence, arbitrary, is only the form of expression, the symbols, the sentences, thus only something external or superficial which is immaterial to the empirical scientist. [Schlick 1936a, 444]

This claim goes hand in hand with the conviction, put forward at the end of the paper, that "in science, as in knowledge generally, we search for nothing but the truth" [Schlick 1936a, 444]. Since only propositions, not sentences, can be true or false, natural laws, being propositions, can fulfil the ultimate goal of science.

To sum up, Schlick's rebuttal of conventionalism is grounded in the claim that at the core of scientific knowledge, there exists a set of propositions that are not chosen at will, but have an objective character deriving from the fact that they are supported by experience.

2.2 Schlick's paper in the Probability session

Schlick's paper read during the session on Probability, called "Gesetz und Wahrscheinlichkeit" [Law and Probability] [Schlick 1936b], addresses two questions: (1) "When does science speak of a law?" and (2) "How does it employ the concept of 'probability'?". In reply to the first question, Schlick maintains that the fundamental character of a law is *regularity*: "regularity is merely another name for conformity to a law" [Schlick 1936b, 447]. By contrast, chance is taken to be "the opposite of 'law' ". Probability applies to chance, and the "so-called laws of probability are the rules of chance" [Schlick 1936b, 446]. Chance is characterized as *irregularity*, so that "the attempts of the probability theorists to define chance are in fact directed at describing the particular kind of 'irregularity' or lack of rules, which is the opposite of lawfulness" [Schlick 1936b, 447]. This leads Schlick to conclude that "we attribute chance to those events which obey the rules of the probability calculus" [Schlick 1936b, 449]. The probability calculus is "a purely mathematical (logical) discipline, which can be constructed, just like arithmetic or 'pure' geometry, in complete independence of any facts of experience" [Schlick 1936b, 449].

Regarding his interpretation of probability, Schlick endorses Bernard Bolzano's logical approach, according to which probability is a "degree of validity" [*Grad der Gültigkeit*] relative to a proposition describing a certain hypothesis with respect to other propositions describing the possibilities that are open to it.[1] In assigning a value to a proposition stating a hypothesis, we can proceed either *a priori*, by carefully considering the physical attributes of a given object, such as a roulette wheel or a die, or *a posteriori*, relying on past experience of frequencies. Schlick believes that

1. See [Bolzano 1837], to which Schlick refers.

> [...] the two methods do not differ in principle. For in the *last* resort all our general statements about reality go back to the fact that we often or always observed certain sequences. [Schlick 1936b, 453]

After having observed frequencies in the past, we take them as an indication of the fact that the hypothesis we formulated stands with the possibilities open to it in the same ratio as that given by observed frequencies. On this ground, Schlick claims that the logical definition suggested by Bolzano and the definition in terms of frequencies can be brought together. However, it is worth noting that when making such claim, Schlick does not have in mind the frequency approach developed by Richard von Mises and Reichenbach, but merely the idea that information about frequencies guides the assessment of probabilities in the way described above. In his article "Causality in contemporary physics", Schlick explicitly rejects von Mises's and Reichenbach's frequency interpretation, on account of the fact that according to such an interpretation, "it would everywhere be necessary to proceed to the limit for infinitely many cases—which for empiricism is naturally a senseless statement" [Schlick 1931, 201]. In the same article, Schlick adds that "The only usable method for defining probabilities is, in fact, that which utilizes logical ranges (Bolzano, von Kries, Wittgenstein, Waismann)" [Schlick 1931, 201].

Schlick is no less critical of the subjective view of probability, and his Paris paper maintains that:

> probability statements have a perfectly *objective* meaning, and are not, say, an expression of subjective states of expectations. They say something about the relation of two or more propositions to one another in respect of their truth; they have no concern with whether or not we *believe* in the truth of those propositions. [Schlick 1936b, 454]

Schlick rejects the idea that the probability calculus can be conceived as a generalized logic, extensively discussed during the Paris Congress. Probability, he claims, is not

> [...] a mean between truth and falsity. Every statement is either true or false [...] and probability is something that attaches to the statement *beyond* this, namely in relation to other statements. Truth and falsity are *not* the upper and lower limits of probability, for if they were so, it would have to be a contradiction to attribute truth and probability simultaneously to one and the same proposition. [Schlick 1936b, 454–455].

The conclusion reached at the end of the paper is that the two questions posed in the beginning boil down to one and the same, because the first can be reduced to the second. To substantiate this claim, Schlick compares the notion of probability with the basic concepts of geometry, holding that there is a strong analogy between them, for “in both cases it is a matter of setting up such definitions as enable us to arrive at maximally convenient descriptions and prognoses of facts” [Schlick 1936b, 455]. It should not pass unnoticed that the last claim provides a connection between the two papers delivered in the Paris Congress sessions on Induction and Probability.

3 Carnap’s contribution to the session on Induction

Carnap’s contribution to the session on Induction bears the title “Wahrheit und Bewährung” [Carnap 1936b]. An expanded English version of this paper appeared in 1949 under the title “Truth and Confirmation” [Carnap 1949], which includes some new passages, and a few passages taken from the 1946 article “Remarks on induction and truth” [Carnap 1946]. It is interesting to compare the text published in the proceedings of the Paris Congress with the article published in 1949. The Paris version of the paper sets the notion of *confirmation* in opposition to that of *truth*, on the account that truth is time-independent, while confirmation is time-dependent. By contrast, a passage Carnap added to his 1949 paper claims that only the *pragmatic* concept of confirmation is time-dependent, while the *semantic* concept is “independent of the temporal aspect” because “in using this concept we are merely asserting an analytic or logical truth which is a sheer consequence of the definition of ‘degree of confirmation’ (weight, strength of evidence) presupposed” [Carnap 1936b, 119].

A major difference between the paper read in Paris and its 1949 version is that the latter mentions the concept of *degree of confirmation*, which is absent from the earlier version. Albeit Carnap started working intensively on probability around the mid-forties, the notion of degree of confirmation made its first appearance in his writings at the end of “Testability and meaning” [Carnap 1936a, 1937]. This is evidence that Carnap started thinking of the quantitative notion of confirmation—together with the option of adopting probability as a tool for dealing with significance and confirmation—already in the mid-thirties, although perhaps when attending the Paris Congress those ideas were still in a fledgling state. Surely Carnap’s interest in probability originated from the problem of cognitive significance, and more particularly from the need to overcome the strictures connected with the verifiability theory of meaning that imprinted the first stage of logical empiricism.

Carnap’s shift from a strict to a liberalized version of the principle of empiricism is documented by the comparison between the Paris paper and the article published in 1949. While admitting that knowledge of the invariable truth of synthetic propositions is inaccessible, Carnap was not ready to give up the notion of truth. In this connection, one of the passages added in the 1949 paper claims disagreement with Felix Kaufmann, Otto Neurath and Reichenbach, who

> [...] are of the opinion that the semantical concept of truth, at least in its application to synthetic sentences concerning physical things, ought to be abandoned because it can never be decided with absolute certainty for any given sentence whether it is true or not. I agree [*so the passage continues*] that it can never be decided. But is the inference valid which leads from this result to the conclusion that the concept of truth is inadmissible? [Carnap 1936b, 122–123]

In order to allow for a negative answer to this question, a liberalized version of the principle of empiricism had to be adopted. In "Truth and confirmation" such a *weaker principle of empiricism* P* is stated as follows:

> A term (predicate) is a legitimate scientific term (has cognitive content, is empirically meaningful) if and only if a sentence applying the term to a given instance can possibly be confirmed to at least some degree. [Carnap 1936b, 123]

Carnap points out that "possibly" should be taken to mean "if certain specifiable observations occur", and "to some degree" should not be "meant as necessarily implying a numerical evaluation". He also clarifies that

> P* is a simplified formulation of the "requirement of confirmability" [*reference in footnote to "Testability and meaning"*] which, I think, is essentially in agreement with Reichenbach's "first principle of the probabilistic theory of meaning" [*reference in footnote to* "Experience and Prediction"], both being liberalized versions of the older requirement of verifiability as stated by C. S. Peirce, Wittgenstein, and others. [Carnap 1936b, 123]

The agreement with Reichenbach pronounced in the above-mentioned passage sounds puzzling because Carnap and Reichenbach had quite different ways of dealing with the theory of meaning. If there is agreement, it does not go beyond the conviction they both shared that the strict principle of empiricism should be abandoned in favour of a liberalized principle. On the one hand, Carnap was one of the chief proponents of the principle of strict empiricism, but did not hesitate to revise it as soon as he became aware of its difficulties. A comparison between the paper published in the proceedings and its revised 1949 version gives evidence of this shift, which presumably started soon after the Paris Congress.

On the other hand, Reichenbach—who had been working on probability in connection with the interpretation of contemporary science since the mid-twenties—was always critical of verifiability and soon urged the need to go beyond it. He emphasized the close ties between the significance of scientific statements and their predictive character, which is a condition for their testability. At the same time, he reaffirmed that "the theory of knowledge is a theory of prediction" [Reichenbach 1937, 89], and put forward his theory of probability as a "theory of propositions about the future" [Reichenbach 1936b, 159]. Such a theory includes in the first place a probabilistic theory of meaning:

> The theory of propositions about the future will [...] be a theory in which the two truth-values, true and false, are replaced by a continuous scale of probability. [Reichenbach 1936b, 154]

Reichenbach's probabilistic theory of meaning

> [...] substituted probability relations for equivalence relations and conceived of verification as a procedure in terms of probabilities rather than in terms of truth [...] it abandoned the program of defining "the meaning" of a sentence. Instead, it merely laid down two principles of meaning; the first stating the conditions under which a sentence has meaning; the second the conditions under which two sentences have the same meaning.
> [Reichenbach 1951, 47]

It is noteworthy that Reichenbach intended his own probabilistic approach as a confutation of the reductionist attitude he attributed to logical empiricists including Carnap, whom he already charged with reductionism and lack of consideration for the probabilistic aspects of science in his review of the *Aufbau* published in 1933, where he writes:

> It is a puzzle to me just how logical neo-positivism proposes to include assertions of probability in its system, and I am under the impression that this is not possible without an essential violation of its basic principles. [Reichenbach 1933, 407]

Later on, Reichenbach criticized Carnap's reduction chains as defined in "Testability and meaning" claiming that they represent "too primitive instruments for the construction of scientific language" [Reichenbach 1951, 48] because they rely on logical implication, not on probability.

4 Reichenbach's Paris papers

At the Paris Congress, Reichenbach delivered one paper in the Induction session, namely "Die Induktion als Methode der wissenschaftlichen Erkenntnis" [Reichenbach 1936a], and one in the Probability session, called "Wahrscheinlichkeitslogik als Form wissenschaftlichen Denkens" [Reichenbach 1936c]. The two papers are strictly related: not surprisingly, given that Reichenbach regarded the inductive method as inextricably intertwined with probability and the main purpose of his contributions is to argue in favour of a strict connection between probability and induction.

The link between induction and probability is provided by the *rule of induction* [*Induktionsregel*], which right at the beginning of Reichenbach's first paper is stated as follows: take a series of n events, which are partly P and partly *non*$-P$, and take m to be the number of P, and $h_n = m/n$ the frequency of P among the n events which have been observed; the rule of induction propounds to assume that when the series is prolonged, the frequency h_n will converge towards a limit close to h_n. It corresponds to what in *The Theory of Probability* Reichenbach calls *induction by enumeration*, namely a method which is "based on counting the relative frequency [of a certain attribute] in an initial section of the sequence, and consists in the inference that the relative frequency observed will persist approximately for the rest of the sequence; or, in other words, that the observed value represents, within certain limits of exactness, the value of the limit for the whole sequence" [Reichenbach 1949, 351]. This formulation highlights the link between induction and probability: probabilities are determined by induction by enumeration, and conversely induction is performed in a probabilistic fashion. Whenever we assess the probability of an uncertain event we make a wager, pretty much like the gambler who "has to make a prediction before every game, although he knows that the calculated probability has a meaning only for larger numbers; and he makes his decision by betting, or as we shall say, by *positing* the more probable event" [Reichenbach 1949, 314]. Reichenbach calls a probability attribution a *posit*, namely "a statement with which we deal as true, although the truth value is unknown" [Reichenbach 1949, 373].

The theory of probability—or inductive inference, since for Reichenbach the two amount to the same thing—is a *theory of posits.* Partly in the paper belonging to the session on Induction and partly in the one belonging to the session on Probability, Reichenbach outlines the main features of his theory of posits. Posits differ depending on whether they are made in a context of *primitive* or *advanced* knowledge; posits made in a state of advanced knowledge are called *appraised*, whereas posits made in a state of primitive knowledge are called *anticipative*, or *blind.* Within primitive knowledge, no prior information on probabilities is available and use is made of the rule of induction, whereas advanced knowledge includes information on priors, to which the calculus of probabilities can be applied. All posits are characterized by a weight, but while appraised posits, which are made in the context of advanced knowledge, have a definite weight, blind posits, which are made in the context of primitive knowledge, have unknown weight, and are approximate in character. However, if the sequence has a limit, anticipative posits can be corrected. According to Reichenbach, this is simply a consequence of the convergence assured by the rule of induction. That of being *self-correcting* is the crucial feature of this procedure—called the *method of concatenated inductions*—that starts with blind posits and goes on to formulate appraised posits that become part of a complex system.[2]

The self-corrective character of the procedure provides the grounds for its justification, and more generally for the justification of induction. The argument put forward by Reichenbach focusses on the rule of induction, which is the building block of his method of concatenated inductions. Starting from the tenet that induction cannot be justified on logical grounds, as convincingly argued by David Hume, Reichenbach seeks a justification on pragmatical grounds, and argues that inductive inference, and more in particular the rule of induction, can be justified on the basis of its predictive success, which makes it the best possible guide to the future. This is so precisely because of its self-corrective character. The argument is stated in *The Theory of Probability* as follows:

> the rule of induction is justified as an instrument of positing because it is a method of which we know that if it is possible to make statements about the future we shall find them by means of this method. [Reichenbach 1949, 475]

In other words, the rule of induction is a *necessary condition* for making good predictions. As claimed in the first of Reichenbach's Paris papers: "the application of the rule does not guarantee success, but without using it, success is not to be obtained at all" [Reichenbach 1936a, 4, my translation].

Reichenbach's second paper is devoted to probability logic, based on the idea that the probability calculus can be translated into a multivalued logic in which the values "true" and "false" are the limiting cases of probability.[3] He points out that the attempt to combine probability with the logic of truth faces a peculiar problem, arising from the fact that when a statement about a future event contains a probability value, such a statement can be verified only after the event has occurred. This calls for some way of bringing together probability, which can take many values, with the two values of truth and falsehood. Reichenbach thinks that the problem is solved by the frequency theory, which combines statements about single events and statements about frequencies in the proper way, because "the frequency interpretation derives the degree of probability from an enumeration of the truth values of individual statements" [Reichenbach 1932, 311]. Under the frequency interpretation, probability refers to sequences (namely to series of statements), while truth refers to single sentences, but since the propositional sequence "can be conceived as an extension of the concept of statement" [Reichenbach 1932, 312], probability logic can be seen as a logic of propositional sequences and "appears as a generalization of the logic of statements" [Reichenbach 1932,

2. For more on Reichenbach's inductivism see [Galavotti 2011], see also [Glymour & Eberhardt 2016].

3. Chapter 10 of Reichenbach's *The Theory of Probability* spells out the topic in some detail [Reichenbach 1949, chap. 10].

313]. Reichenbach's position in this connection was sharply criticized by Janina Hosiasson in the paper she delivered in Paris.

Instead of going into the details of their exchange on the topic, it is worth mentioning that towards the end of his second paper, Reichenbach puts forward a view of science as a continuous search for the truth [*Richtung zur Wahrheit*], which advances by augmenting the degree of probability of knowledge [see Reichenbach 1936c]. This process is realized by formulating increasingly accurate appraised posits, according to the method of concatenated inductions. Moreover, science taken as a whole can be seen as a blind posit. This picture is rooted in Reichenbach's conviction that there are true probability values characterizing chance phenomena, which are in general unknown but can be approached by means of the inductive method. Such a conviction represents a cornerstone of the frequency theory, which embodies an objective notion of probability as opposed to the subjective interpretation upheld by authors like Frank Ramsey and Bruno de Finetti, also shared by another participant in the Paris Congress, namely Janina Hosiasson.[4]

5 About Hosiasson

The paper delivered by Hosiasson during the Paris Congress, bearing the title "La théorie des probabilités est-elle une logique généralisée? Analyse critique" [Hosiasson 1936], is entirely devoted to a criticism of Reichenbach's probability logic, her main thesis being that the theory of probability cannot be considered a generalization of the logic of statements, whatever meaning is assigned to the notion of "generalization". Hosiasson's paper also moves some objections against Zygmunt Zawirski, who in an article published in 1934 (in Polish) had also made an attempt to treat probability as many-valued logic. Zawirski took part in the Paris Congress delivering a paper titled "Les rapports de la logique polyvalente avec le calcul des probabilités" [Zawirski 1936], which contains a revision of his earlier position, partly provoked by some objections moved by Hosiasson in former debate. Attempts to build probability logic conceived as a generalization of two-valued logic were in tune with the spirit of the time, imbued with a deep trust in the power of logic and axiomatization, and received great impulse from the work of Jan Łukasiewicz, Hugh MacColl and others. However, the programme of dealing with probability within the framework of many-valued logic did not last long, and the attempts in that direction made by authors like Reichenbach and Zawirski did not impact much on subsequent literature. For this reason, and given the technical character of the topic, it will not be examined in depth herein.

Instead, it does not seem out of place to mention that Janina Hosiasson was an original thinker, unduly overlooked by subsequent literature. A representative of the Lvov-Warsaw School active before the Second World

4. On the different interpretations of probability see [Galavotti 2005].

War,[5] Hosiasson focussed on the foundations of probability, induction and confirmation. Under the influence of the Polish logician Stefan Mazurkiewicz, who in 1932 published an axiomatization of probability which she espoused with minor changes, and of John Maynard Keynes' *Treatise on Probability* (1921), Hosiasson adopted an epistemic approach to probability, embracing a perspective close to subjectivism. She regarded probability as *justified belief*, grounded on the fundamental assumption of mathematical expectation, and assigned probability the task of providing a guide to decision and action. According to Hosiasson, justified belief is the best form of knowledge that can be obtained, whereas absolute truth and completeness of knowledge are unattainable. Probability is therefore necessary to the advancement of knowledge, which proceeds inductively. Moving from this conviction Hosiasson set herself the goal of analysing the nature of probabilistic induction, and her work in that connection anticipated subsequent research in a number of ways.[6]

Hosiasson's "Why do we Prefer Probabilities Relative to Many Data?" [Hosiasson 1931] addresses the question: "How do we account for probabilities in particular cases?", answering that "in a considerable number of cases in ordinary life we take account of them by considering the amount of something which could be said to be a mathematical expectation" [Hosiasson 1931, 30]. This brings her close to Frank Plumpton Ramsey's perspective, with the difference that Hosiasson takes an axiomatic approach, assuming mathematical expectation as an axiom rather than a principle of psychology, as Ramsey did. By appealing to mathematical expectation, she deliberately shares Ramsey's pragmatist stand, as suggested by her claim that "taking gains or mathematical expectations into consideration could be considered as an epistemological answer only from a pragmatist point of view" [Hosiasson 1931, 36]. Noteworthy, in the same paper, is footnote 15 in which Hosiasson writes:

> I am greatly indebted for clearness on this question to an unpublished paper by Mr F. P. Ramsey on "Truth and probability" which the kindness of Mr Braithwaite has enabled me to read. I had, however, previously thought independently on similar lines. [Hosiasson 1931, 30]

This is a remarkable claim, bringing evidence that Hosiasson was among the first to embrace a subjective view of probability.

Hosiasson's conception of knowledge also bears strong resemblance to that upheld by Ramsey. According to her definition, knowledge is "an aggregate of opinions justified, actual and connected—in an adequate degree" [Hosiasson 1948, 253]. The similarity with Ramsey's view of knowledge as "obtained by a reliable process" is striking [see Ramsey 1990, 110–111, note "Knowledge" (1927)]. Moreover, like Ramsey, Hosiasson regarded knowledge as belief of a special sort, not just entertained by individuals, but apt to be shared by the

5. See [Woleński 2017] for more on the Lvov-Warsaw school.
6. For more on Hosiasson, see [Galavotti 2008].

community of men. The advancement of knowledge requires induction and probability, and depends on the increase in weight of a certain hypothesis in the light of new evidence.

Hosiasson's best-known article, namely "On Confirmation" [Hosiasson 1940], contains a proposal for a solution in probabilistic terms to the "raven paradox"—also known as "Hempel's paradox", because it undermines the notion of confirmation developed by Carl Gustav Hempel [see Hempel 1937].[7] Unlike Hempel, who focussed on a qualitative notion of confirmation, Hosiasson opted for a quantitative approach based on the possibility of discriminating between instances of the paradoxical and non-paradoxical kind, the idea being that a non-paradoxical instance of a certain hypothesis increases its prior probability to a greater degree than a paradoxical instance. In "Studies in the logic of confirmation" [Hempel 1945], Hempel deems Hosiasson's discussion of the issue "illuminating". Furthermore, in 1966, Patrick Suppes proposed a solution to the raven paradox along Bayesian lines, claiming to have borrowed the leading idea from Hosiasson [see Suppes 1966].

6 Bruno de Finetti's contribution to the session on Probability

Bruno de Finetti's paper, bearing the title "La logique de la probabilité" [de Finetti 1936], contributes to the debate on probabilistic logic by outlining what the author calls the logic of *trievents*, namely a three-valued logic obtained as a "superposition" to two-valued logic by adding to the two truth values "true" and "false" the third value "null". In this way, de Finetti obtains a system of propositional logic which is monotonic, unlike other many-valued logics, such as that of Łukasiewicz which is non-monotonic. De Finetti's logic is about conditional events, and subsequent literature has shown it more relevant to the logic of conditionals than to probability theory. It was precisely in this spirit that an English version of the paper was published in 1995. As convincingly argued by Alberto Mura, de Finetti's logic anticipated the work of other authors, including Stephen Cole Kleene's "strong material implication" and Stephen Blamey's partial logic.[8]

According to de Finetti, a trievent corresponds to $A|B$ (A given B), defined as "the logical entity which is considered: 1°) *true* if A and B are true; 2°) *false* if A is false and B is true; 3°) *null* if B is false" [de Finetti 1936, 184]. A trievent is a function of B and $(A \cap B)$ alone; in case B does not happen $A|B$ is undecidable, or void, in the sense that it cannot take any value. Then, the value "N" (null) is not a truth value on a par with "true" and "false", but expresses

7. For more details, see [Galavotti 2008].
8. See [Mura 2009] for further details.

the impossibility of assigning a truth value. In an essay called *L'invenzione della verità*,[9] de Finetti illustrates the idea by means of the following example:

> If, for instance, I say: "supposing that I miss the train, I shall leave by car", I am formulating a "trievent", which will be either true or false if, after missing the train, I leave by car or not, and it will be null if I do not miss the train. [de Finetti 2006, 103], quoted from [Mura 2009, 204]

The notion of trievent reflects the distinction, de Finetti considered vitally important, between referring to an event *as a statement* and referring to an event *as a condition*, see [de Finetti 1995] and [Mura 2009, 204]. At the same time, such a notion highlights the fundamental role played by conditional probability within de Finetti's perspective. It should not pass unnoticed that there is a perfect match between the definition of a trievent and that of conditional probability, namely: $pA|B = p(A \cap B)/pB$; provided $pB > 0$.

The logic of trievents is in full agreement with the subjective interpretation of probability, of which de Finetti is unanimously considered one of the founders, together with Frank Ramsey. The second part of de Finetti's Paris paper outlines the subjective outlook, according to which probability is a quantitative expression of the degree of belief in the occurrence of an event, entertained by a person in a state of uncertainty. Probability is taken as a primitive notion endowed with a psychological foundation, which needs an operative definition in order to be measured and used in practice. Albeit, this can be done in a number of ways, the paper de Finetti delivered in Paris adopts the notion of betting, maintaining that probability expresses "the conditions under which one judges it equitable to bet [on the occurrence of an event]; an individual is *coherent* if there exists no combination of stakes which permits a sure win in betting with him on the basis of probabilities which he has evaluated" [de Finetti 1936, 186].[10] *Coherence* is the cornerstone of the subjective theory of probability, due to the fact that the laws of probability can be derived from the assumption of coherence—a result stated by Ramsey and proved by de Finetti. For subjectivists, all coherent probability functions are admissible and disagreement is permitted because probability evaluations are not taken to be univocally determined by empirical evidence—such as frequencies—but to be affected also by contextual (subjective) elements—such as experience and the personal abilities of evaluators.

A crucial component of the subjective approach is the claim that with increasing evidence, the opinions of different people will converge. This is guaranteed by the result known as *de Finetti's representation theorem*, which shows that the adoption of Bayes' method, taken in conjunction with

9. Originally written in 1934, but published (posthumously) only in 2006 [see de Finetti 2006].

10. In later writings, de Finetti adopted alternative methods for measuring probability as degree of belief, such as scoring rules—see Galavotti [Galavotti 2001] for more on this.

exchangeability, leads to convergence between subjective degrees of belief and observed frequencies.[11] Events belonging to a sequence are exchangeable if the probability of h successes in n events is the same, for whatever permutation of the n events, and for every n and $h \leq n$. It should be pointed out that in his Paris paper, de Finetti does not use the term " exchangeability", which was suggested to him in 1939 by Maurice Fréchet, instead he speaks of "equivalent" events [*événements équivalents*]. Exchangeability, used in connection with Bayes's rule plays a crucial role within de Finetti's perspective, where it provides the foundation of the whole of statistical inference. The justification of this method is given by its success, as the best method allowing for good forecasting.

Part of de Finetti's subjectivism is the conviction that probability is always definite and known, and consequently there are no unknown true probabilities. That objective probability should be discarded as a metaphysical concept is a claim that appears in many of de Finetti's writings, including the preface to the English edition of *Theory of Probability* [de Finetti 1970] where he wanted printed in capital letters "Probability does not exist". The same idea is expressed in the paper delivered in Paris, where he states that "all metaphysical 'explanation' [...] explains nothing but hides substantial problems and profound reasons behind words stripped of sense" [de Finetti 1936, 187].

Having said that, it must be added that de Finetti took seriously the problem of the objectivity of probability evaluations, or in other words the problem of defining "good probability appraisers", to use an expression borrowed from the Bayesian statistician Irving John Good. In order to understand de Finetti's position, one should bear in mind his recommendation that the definition of probability should not be conflated with its evaluation. While probability is defined as subjective degree of belief, its evaluation depends on both objective elements, including frequencies and symmetries, and subjective components such as intuition, analogy, and expertise. In particular, de Finetti repeatedly stressed in his writings that frequency, and more generally empirical information of all sorts, is a fundamental ingredient of the evaluation of probability, as clearly stated in the following passage:

> Whatever can be taken as objective regarding the events under consideration is not at all rejected by the subjective approach; in particular, frequency and all that can be asserted about frequency finds room there. [de Finetti 1949, 87, my translation]

De Finetti's Paris paper ends with a sharp criticism of the frequency theory. His main objection regards the notion of limit, considered inapplicable to scientific practice, firstly because indefinitely long sequences are not observable, and secondly because a limiting value of probability is compatible

11. This result, already found by de Finetti in 1928, is spelled out in some detail in [de Finetti 1937].

with a very large number of negative observed results. For instance, a very long sequence of throws of a coin where the frequency of tails tends to ½ as the number of trials increases, is compatible with the fact that the first 10,000 trials give tails, but in such a case no experimenter would assign probability $\frac{1}{2}$ to heads/tails. For de Finetti probability is about single events rather than indefinitely long sequences of repeatable events, and their evaluation is more complex than envisaged by frequentists, as it depends on myriad ingredients, partly empirical and partly subjective.

7 Concluding remarks

On the whole, the papers delivered during the two sessions on Induction and Probability at the Paris Congress testify to the richness of the debate held during the meeting. A major focus of attention at that stage of the development of scientific philosophy was the confirmation of scientific hypotheses, together with the related issue of the principle of empiricism. In that connection, Schlick and Carnap's papers were in line with the mainstream attitude embraced by logical empiricists. However, soon after the Paris Congress Carnap's thought underwent major changes, and one can speculate that the Paris debate fostered such a shift.

Given the logical empiricists' deep trust in the clarifying power of logic, which made them regard it as the ideal tool for the realization of the unity of science and the construction of scientific philosophy, it is not surprising that at the time of the Paris Congress, so much effort was devoted to the project of building a logic of probability. As observed earlier, subsequent developments have shown such a project was short-lived, so that in retrospect the critical remarks raised by Hosiasson in her Paris paper look decidedly far-sighted.

The interpretation of probability was widely debated in Paris. Schlick took sides with the logical theory against frequentism and subjectivism, while Reichenbach argued in favour of the frequency view and described in some detail his own approach, which strays in several ways from that upheld by von Mises.[12] In particular, Reichenbach's paper outlines the "method of concatenated inductions", spelled out in more detail in *Experience and Prediction* [Reichenbach 1938]. By the time of the Paris Congress, Reichenbach had published the first edition of *The Theory of Probability*, namely *Wahrscheinlichkeitslehre* [Reichenbach 1935] which does not contain the distinction between primitive and advanced knowledge, nor the pragmatic argument for the justification of induction. The fact that his Paris paper addresses such topics shows that for Reichenbach, the year 1935 marks a time of transition.

Neither Carnap nor Hosiasson address the issue of the nature of probability in their Paris papers. At the time, Hosiasson had already embraced the

12. See [Galavotti 2011] for more on this.

subjective approach in her previous work, while Carnap was on the verge of turning to probability in order to deal with confirmation, but his first papers on probability date back to the mid-forties.

In 1935, de Finetti had already developed his subjective theory of probability together with his "representation theorem", which are described in his contribution to the Paris Congress. In addition, the latter contains an exposition of his logic of trievents, which revealed its potential many years later, in connection with the logic of conditionals. This adds to the interest of his paper, and more in general of the sessions on Induction and Probability held in Paris during the Congress of Scientific Philosophy.

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