HANS REICHENBACH'S PROBABILITY LOGIC

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1 INTRODUCTION

Any attempt to characterize Reichenbach's approach to inductive reasoning must take into account some of the core influences that set his work apart from more traditional or standard approaches to inductive reasoning. In the case of Reichenbach, these influences are particularly important as otherwise Reichenbach's views may be confused with others that are closely related but different in important ways. The particular influences on Reichenbach also shift the strengths and weaknesses of his views to areas different from the strengths and weaknesses of other approaches, and from the point of some other approaches Reichenbach's views would seem quite unintelligible if not for the particular perspective he has.

Reichenbach's account of inductive reasoning is fiercely empirical. More than perhaps any other account it takes its lessons from the empirical sciences. In Reichenbach's view, an inductive logic cannot be built up entirely from logical principles independent of experience, but must develop out of the reasoning practiced and useful to the natural sciences. This might already seem like turning the whole project of an inductive logic on its head: We want an inductive inference system built on some solid principles (whatever they may be) to guide our scientific methodology. How could an inference procedure that draws on the methodologies of science supply in any way a normative foundation for an epistemology in the sciences?

For Reichenbach there are two reasons for this "inverse" approach. We will briefly sketch them here, but return with more detail later in the text: First, Reichenbach was deeply influenced by Werner Heisenberg's results, including the uncertainty principle, that called into question whether there is a fact to the matter – and consequently whether there can be certain knowledge about – the truth of propositions specifying a particular location and velocity for an object in space and time. If there necessarily always remains residual uncertainty for such propositions (which prior to Heisenberg seemed completely innocuous or at worst subject to epistemic limitations), then – according to Reichenbach – this is reason for more general caution about the goals of induction. Maybe the conclusions any inductive logic can aim for when applied to the sciences are significantly limited. Requiring soundness of an inference – preservation of truth with certainty – may not only

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be unattainable, but impossible, if truth is not a viable concept for empirical propositions. Once uncertainty is built into the inference, deductive standards are inappropriate for inductive inference not only because the inference is ampliative (which is the standard view), but also because binary truth values no longer apply.

Second, the evidence supporting Albert Einstein's theory of relativity, and its impact on the understanding of the nature of space and time revealed to Reichenbach the power of empirical evidence to overthrow truths that were taken to be (even by Reichenbach himself in his early years) necessarily true. The fact that Euclidean space had been discovered not only to be not necessary, but quite possibly not true — despite Immanuel Kant's transcendental proofs for its synthetic a priori status — called the state of a priori truths into question more generally. The foundations of any inference system could no longer be taken to be a priori, but had to be established independently as true of the world. Reichenbach refers to this confirmation of the correspondence between formal structures and the real world as "coordination" (although "alignment" might have been the more intuitive description of what he meant).

Einstein's and Heisenberg's findings had their greatest impact on Reichenbach's views on causality. Influenced by the Kantian tradition, Reichenbach took causal knowledge to be so profound that in his doctoral thesis in 1915 he regarded it as synthetic a priori knowledge [Reichenbach, 1915]. But with the collapse (in Reichenbach's view) of a synthetic a priori view of space, due to Einstein, Reichenbach also abandoned the synthetic a priori foundation of causality. Consequently, Reichenbach believed that causal knowledge had to be established empirically, and so an inductive procedure was needed to give an account of how causal knowledge is acquired and taken for granted to such an extent that it is mistaken for a priori knowledge. But empirical knowledge, in Reichenbach's view, is fraught with uncertainty (due to e.g. measurement error, illusions etc.), and so this uncertainty had to be taken into account in an inductive logic that formalizes inferences from singular (uncertain) empirical propositions to general (and reasonably certain) empirical claims. Heisenberg's results implied further problems for any general account of causal knowledge: While the results indicated that the uncertainty found in the micro-processes of quantum physics is there to stay, macro-physics clearly uses stable causal relations. The question was how this gap could be bridged. It is therefore unsurprising that throughout Reichenbach's life, causal knowledge formed the paradigm example for considerations with regard to inductive reasoning, and that probability was placed at its foundation.

The crumbling support for such central notions as space, time and causality, also led Reichenbach to change his view on the foundations of deductive inference. Though he does not discuss the foundations of logic and mathematics in any detail, there are several points in Reichenbach's work in which he indicates a switch away from an a prioristic view. The a prioristic view takes logic to represent necessary truths of the world, truths that are in some sense ontologic. Reichenbach rejects this view by saying that there is no truth "inherent in things", that necessity is a result of syntactic rules in a language and that reality need not conform to the syntactic rules of a language [Reichenbach, 1948]. Instead, Reichenbach endorsed a formalist view of logic in the tradition of David Hilbert. Inference systems should be represented axiomatically. Theorems of the inference system are the conclusions of valid deductions from the axioms. Whether the theorems are true of the world, depends on how well the axioms can be "coordinated" with the real world. This coordination is an empirical process. Thus, the underlying view holds that the axioms of deductive logic can only be regarded as true (of the world) and the inference principles truth preserving, if the coordination is successful – and that means in Reichenbach's case, empirically successful, or useful. In the light of quantum theory, Reichenbach rejected classical logic altogether [Reichenbach, 1944].

Instead of an a priori foundation of inductive logic, Reichenbach's approach to induction is axiomatic. His approach, exemplified schematically for the case of causal induction works something like this: We have causal knowledge. In many cases we do not doubt the existence of a causal relation. In order to give an account of such knowledge we must look at how this knowledge is acquired, and so we have to look closely at the methodologies used in the natural sciences. According to Reichenbach, unless we deny the significance of the inductive gap David Hume dug (in the hole created by Plato and Sextus Empiricus), the only way we will be able to make any progress towards an inductive logic is to look at those areas of empirical knowledge where we feel reasonably confident that we have made some progress in bridging that gap, and then try to make explicit (in form of axioms) the underlying assumptions and their justification (or stories) that we tell ourselves, why such assumptions are reliable.

There are, of course, several other influences that left their marks on Reichenbach's views. Perhaps, most importantly (in this second tier), are the positivists. Their influence is particularly tricky, since Reichenbach was closely associated with many members of the Vienna Circle, but his views are in many important ways distinctly "negativist": Reichenbach denies that there can be any certainty even about primitive perception, but he does believe — contrary to Karl Popper — that once uncertainty is taken into account, we can make progress towards a positive probability for a scientific hypothesis. We return to the debate with Popper below.

Second, it is probably fair to say that Richard von Mises, Reichenbach's colleague during his time in Berlin and Istanbul, was the largest influence with regard to the concept of probability. Since probabilistic inferences play such a crucial role in scientific induction, Reichenbach attempted to develop a non-circular foundation and a precise account of the meaning and assertability conditions of probability claims. Reichenbach's account of probability in terms of the limits of relative frequency, and his inductive rule, the so-called "straight rule", for the assertability of probability claims — both to be discussed in detail below — are perhaps his best known and most controversial legacy with regard to inductive inferences.

As with any attempt to describe a framework developed over a lifetime, we would inevitably run into some difficulty of piecing together what exactly Reichenbach meant even if he had at all times written with crystal clarity and piercing precision — which he did not. On certain aspects, Reichenbach changed or revised his view, and it did not always become more intelligible. However, areas of change in Reichenbach's account are also of particular interest, since they give us a glimpse into those aspects that Reichenbach presumably deemed the most difficult to pin down. They will give us an idea of which features he considered particularly important, and which ones still needed work. As someone, who in many senses sat between the thrones of high church philosophy of his time and (therefore?) anticipated many later and current ideas, Reichenbach's views are of particular interest.

2 PROBABILITY LOGIC: THE BASIC SET-UP

Reichenbach distinguishes deductive and mathematical logic from inductive logic: the former deals with the relations between tautologies, whereas the latter deals with truth in the sense of truth in reality. Deductive and mathematical logic are built on an axiomatic system. Whether the axioms are true of the world is open to question, and only of secondary interest in the deduction of mathematical theorems. Reichenbach admits that we appear unable to think other than by adhering to certain logical inferences, but that does not make deductive logic necessarily true of the world. We similarly appear quite unable to think of real space in terms of anything but Euclidean space, even though we know since Einstein (and the results of various crucial experiments) that real space is not Euclidean.

In contrast to the formal relations that are of interest in deductive logic, inductive logic is concerned with the determination of whether various relations between quantities are true in the world; the aim is to represent, or, as Reichenbach says, "coordinate" the real world with mathematical relations. A scientific law states a mathematical relation about certain quantities in the world. The task of inductive logic is to establish whether the mathematical relations described by the law, correspond to the relations between the real features in the world represented by symbols in the mathematical law. While the semantics of deductive logic are formal, and can be adjusted to fit syntactic constraints, we do not have such definitional freedom of interpretation when describing the real world. Thus, inductive logic must not only satisfy a formal semantics but enable a mapping between the syntactic representation (the mathematical law) and its interpretation (the real world quantities).

Reichenbach is a realist about the external world. But the access we are granted to the external world is indirect. He compares our experience about the external world to standing in a cloth cube, and drawing inferences about the objects outside the cube based on the shadows we see from the inside on the cube's surface [Reichenbach, 1938a]. The information we obtain about the external world is not only indirect but also inexact. No empirical procedure supplies perfectly "clean" data. The data is "unclean" for two reasons: First, in measuring a particular parameter, there is always an infinity of other small influences that make the measurement noisy. Second, if Heisenberg's uncertainty principle is not only epistemic, but indicates a true metaphysical uncertainty (and Reichenbach appeared to take this view despite being close friends and an admirer of Einstein), then there is no exact measurement to be had in the first place. Consequently, all scientific laws are established by probabilistic inferences from empirical samples. If the epistemological support for scientific laws is only probabilistic, then an inductive logic cannot be two valued, but must, like probability, be continuously valued. So Reichenbach's inductive logic is continuously valued between 0 and 1, where the "truth value" corresponds to the probabilistic support. Reichenbach terms his system "probability logic" and so shall we in what follows.

To define probabilistic support, Reichenbach again turns to the methodology used in science: Scientific laws are universal claims based on a finite sequence of observations. The uncertainty in the truth of the scientific law derives (at least in part) from the fact that at any point in the sequence of observations we do not know whether future observations will follow the pattern we have seen so far. Consequently, the probability value describing the support we have for a scientific claim must be a property of the sequence of observations we see. For Reichenbach the probability of an event is the limit of the relative frequency of events of the same type — what that means becomes one of Reichenbach's main problems — in an infinite sequence of events. (There are a few further constraints on the sequence, but we will leave them aside here.) This is what is referred to as Reichenbach's *limiting frequency* interpretation of probability.

But a logic, even a probability logic, is not about events, a logic is about inferences on propositions describing events. Consequently, Reichenbach must provide an account of how a probability that is defined as a limiting frequency in a sequence of events is represented in a probability logic. Reichenbach claims that the structure of events is mapped directly onto propositions in his probability logic that are "coordinated" with the events. That is, for each event in a sequence there is a proposition describing that event and the resulting sequence of propositions reflects isomorphically the structure of the sequences of events. Since the structures are isomorphic, probability values can be used interchangeably for the events and the propositions describing the events. The probability associated with a proposition in his probability logic is the limiting frequency of that proposition in a sequence of propositions describing a sequence of coordinated events in the external world. For example, if a ball is rolled down an inclined plane several times, and the time for its descent is measured on each trial, then there is a sequence of events on the inclined plane, each of which is associated with a proposition, e.g. proposition 1 might state: "The ball took 4.2 seconds ($\pm\delta$)." The probability of proposition 1 is the limiting frequency (of occurrence) of this proposition in the sequence of propositions describing the trials. (Since measurements of continuous quantities can only be stated within intervals — due to measurement errors — the limiting frequency of a proposition is non-zero. But, admittedly, Reichenbach fudges the details on this point.) The approach is very intuitive given scientific practice. It is basically a formal representation of the construction of histograms.

But the implication for a probability logic is significant: The probability logic must be a logic of sequences.

So probability values are limits of the relative frequency of propositions in the sequences. We still need an account of how these values are to be estimated, especially since empirical event sequences are necessarily finite, thereby leaving the limit of the infinite sequence undetermined. And whatever the method of estimation, we require a justification for its application in place of any other procedure.

Reichenbach's procedure for estimating the probability of event *types* is simple: One should use the frequencies in the available finite initial segment as if they were from the limiting distribution. As more of the sequence becomes available, the empirical distribution, and with it the probabilities, should be adjusted accordingly. This inductive inference rule is now known as the *straight rule*.

The justification of the straight rule has three parts: First, Reichenbach argues that we have recourse to higher order probabilities (supposedly based on more general abstract knowledge), that provide reason to believe in the approximate accuracy of the empirical distribution. Second, he claims that a hierarchy of higher order probabilities, in which no higher order claim is certain, need not lead to an infinite regress of probabilities (which we would be unable to determine). The regress can be truncated by blind posits – wagers — that can be substituted instead of probability values. Third, the straight rule estimate converges to the limiting frequency as accurately as one wants, and with a finite amount of data. Reichenbach recognizes that other inference rules have the same convergence guarantee, but claims that the straight rule is "simpler."

If one buys the claims, then one (supposedly) has a logic based on probabilistic inference. Unlike standard logical calculi it does not relate individual propositions, but sequences of propositions. It assigns to sequences of propositions a continuous value that is given by the limit of the relative frequencies in the sequences. For empirical claims, the value of this limit is estimated by the available scientific evidence and the straight rule, while the results of standard two-valued logic can be derived as a limiting case.

Reichenbach considers this to be the best we can do in light of the limitations posed by Hume's inductive gap. Certainty is no longer an achievable aim for induction, we can only speak of high probability. The continuous values of probability reflect a graded notion of truth (in the external world) of the conclusions reached by inductive inference. Reichenbach also sees his account as a vindication of the rationality of procedures of scientific inference. It provides a justification for an increased confidence in the truth of some scientific claims rather than others, as a function of sample size, or as a result of other similar findings in closely related fields. The calculus of probability, as part of this probability logic, provides the inference machinery to transfer probabilistic support between scientific claims. Any hope for a more sturdy bridge across the inductive gap is wishful thinking.

Needless to say, not everyone took this approach to be as successful as Reichenbach considered it to be, and we will review some of the criticisms below. But before, we will flesh out the various aspects of this probability logic in more detail.

3 PROBABILITIES AS LIMITING FREQUENCIES

For Reichenbach, probability, as it is used in science, is an objective quantity, not a subjective degree of belief. The main difficulty for such an account is to state precisely what such an objective probability is supposed to be, while providing justified grounds for making probability judgments. In fact, throughout his life Reichenbach worked on the foundations of probability, and his views changed.

In his doctoral thesis in 1915, Reichenbach argues that the probability of an event is the relative frequency of the event in an infinite sequence of causally independent and causally identical trials [Reichenbach, 1915]. Influenced by the neo-Kantians of his time (Ernst Cassirer, Paul Natorp etc.) Reichenbach took causality to be a primitive concept, more fundamental than probability. On this view, causal knowledge is synthetic a priori knowledge, and the proof for this status was supposedly given by Kant's transcendental deduction of the principle of causality. That is, according to (Reichenbach's reading of) Kant we have causal knowledge for individual events that enable us to determine causal independence and identical causal circumstances, and so, according to Reichenbach, we have a non-trivial, non-circular and objective basis on which to build the concept of probability. In particular, if one can show that causally independent and identical trials imply probabilistically independent and identically distributed trials, then the law of large numbers implies that the empirical distribution converges to the true distribution in probability.[1] Reichenbach was aware of the (weak) law of large numbers (although it is not discussed in any detail in his thesis), but he considered convergence in probability too weak. Relying on convergence in probability would imply that the notion of probability features in the definiens of the definition of probability, which would render the definition of the concept of probability circular. Reichenbach wanted to establish convergence with certainty.

To resolve this dilemma, Reichenbach (in 1915) again reached into the Kantian toolbox and provided a transcendental argument that there is a synthetic a priori principle — the principle of lawful distribution – that guarantees with certainty that every empirical distribution converges. The essence of the transcendental argument is as follows: If there were no such principle, scientific knowledge as it is represented in the laws of nature would be impossible. But obviously science is replete with knowledge about lawful causal relations. Scientific laws state general causal regularities but Kantian causal knowledge only supplies causal knowledge with regard to single events. Something is needed to aggregate the individual causal knowledge tokens into general causal laws. Hence, there must be such a principle. Given the principle, convergence is guaranteed with certainty, and even if we do not know when convergence will occur or at what rate, we are on the right track if we use the empirical distribution, since it must converge at some point. That was, in short, the argument of his doctoral thesis.

[1]The law of large numbers states that in a sequence of independent identical trials, for every $\epsilon > 0$ the probability that the frequency of success in the sequence differs from the true probability of success by more than ϵ, converges to zero as the number of trials n goes to infinity.

The argument is not very satisfying even if the gaps were filled in (e.g. from causally independent trials to probabilistically independent trials): Granted the nowadays implausible view that causal knowledge of token events is synthetic a priori, it is simply false to claim that there is a synthetic a priori guarantee of convergence of sequences to a limiting distribution — we know many sequences whose limiting frequencies do not converge.

Reichenbach must have at some point (if not all along) felt similarly uncomfortable with his account, since his argument changed significantly between his doctoral thesis in 1915 and the publication of the English edition of *The Theory of Probability* in 1949 [Reichenbach, 1949c].[2]

In 1927 Reichenbach indicated in notes[3] (and referred to earlier discussions with Paul Hertz) that convergence with certainty is untenable, and that one can only guarantee convergence in probability. This is a transitional thought of Reichenbach's. It is an observation on the law of large numbers, essentially, which posits a probability distribution from which initial segments of sequences are obtained by i.i.d sampling. The limiting frequency interpretation, by contrast, affords the certainty of convergence to the probability by the straight rule, provided there exists a limit value at all. In addition, he changed his mind on the order of the primitives: Once Einstein had shaken the synthetic a priori status of space and time, Reichenbach similarly reviewed the synthetic a priori status of causality and concluded that it was not causality, but rather probability that was the more fundamental notion, i.e. that causality is a relation that can only be inferred on the basis of probabilistic relations (plus some additional assumptions). Claims about single event causation – what now is referred to as actual causation — were considered elliptic, either implicitly referring to a sequence of events or fictitiously transferring the causal claim from the type level to the token level.

However, if causal relations are no longer fundamental, and probabilities are not to be taken as primitive, then Reichenbach had to find a new foundation for the concept of probability. This effort coincided with similar concerns by Richard von Mises. Von Mises was trying to establish a foundation of probability in terms of random events [von Mises, 1919]. While randomness was well understood pretheoretically, all attempts to characterize it formally turned out to have undesired consequences. The aim that both Reichenbach and von Mises shared was a reduction of the concept of probability to a property of infinite sequences of events, thereby avoiding any kind of circularity in the foundation.

Given scientific practice it seemed intuitive and appealing to think of objective probabilities in terms of the relative frequency in an infinite sequence of events — one only had to appropriately characterize the types of sequences that would be considered admissible as providing the foundation of probability. For example, one would not consider a sequence admissible, if it simply alternated back and

[2]For more details of the changes see the introduction to the translation of Reichenbach's thesis [Reichenbach, 2008].

[3]See reference HR 044-06-21 in [Reichenbach, 1891-1953], also discussed in the introduction in [Reichenbach, 2008].

forth between 1 and 0. While the limiting relative frequency of 1s is 1/2, one knows the next number with certainty given any initial segment long enough to exhibit the pattern. Von Mises therefore wanted to restrict his considerations to sequences of random events, since the notion of randomness captured the idea that one would not be able to make any money by betting on the next item in the sequence given the previous items. More formally, the lack of after-effect and the invariance to subsequence selection were considered necessary conditions for random sequences. The lack of after-effect captures the idea of being unable to make any money by betting on the next item of a sequence, given the previous items. In particular, the probability of any outcome should be the same, no matter what the previous outcomes were. Invariance under subsequence selection requires that the probability of an event is the same under any subsequence selection rule that depends only on the indices of the items. How these notions are spelled out formally differs among authors.

Reichenbach rejected the idea of random sequences because he saw no hope of being able to adequately capture randomness formally.[4] There were known theoretical difficulties in showing that all the conditions for randomness could be satisfied, and Reichenbach had pointed out some of them [Reichenbach, 1932a]. Reichenbach did not give up on the idea completely, but instead settled for a somewhat weaker constraint on sequences: normal sequences. Normal sequences form a strict superset of random sequences. A sequence of events is *normal* if the sequence is free of after-effect and the probabilities of event types is invariant under regular divisions. Reichenbach's definition of after-effect is not entirely clear, but roughly, in a sequence with after-effect an event E at index i implies for events at indices subsequent to i probabilities that differ from the limiting relative frequency of those events. Regular divisions are subsequence selection rules that pick out every k-th element of the original sequence for some fixed k. (In fact, the conditions are a little more intricate, but we leave that aside here.) The probability of event E then is the limiting relative frequency of E in a normal sequence of events.

This works as an abstract definition of probability, but it is not adequate to determine scientific probabilities. In empirical science the sequences of measurements are finite. The finite initial segment of a sequence gives us no information about the limiting distribution. Nevertheless, Reichenbach claims that we should treat the empirical distribution given by the finite initial segment of measurements as if it were (roughly) the same as the limiting distribution. He believes we have recourse to a higher order probability that specifies the probability that the limiting relative frequency of the event (its true probability) is within some (narrow) band of width δ around the empirical frequency. This higher order probability is also based on empirical data, but indirectly: it derives from a sequence of probability values of the first order, i.e. from a sequence of sequences of events. The idea is that it integrates data from sequences of different inductive inferences. Reichenbach gives one type of example in different forms that provides some idea of how

[4]Ernest Nagel agreed with this latter point, but believed that von Mises's weaker version of randomness could nevertheless be formalized [Nagel, 1936].

this is supposed to work (e.g. see [Reichenbach, 1949c, pp. 438-440]): Suppose we have a finite sequence of measurements,

$$M_1, M_2, M_3, M_4, M_5, \ldots, M_n$$

and we classify them as 1 or 0 depending on whether they fall into some prespecified narrow range around a fixed value. For example, suppose we have measurements of the gravitational constant and we classify the data points as 1 if they fall within the band $\gamma \pm \delta$ for some small value of δ, and 0 otherwise. So we can define a variable $X = I(|M - \gamma| \leq \delta)$, where $I(.)$ is the indicator function, and obtain a sequence of values of X consisting of 1s and 0s, depending on the original measurements:

$$1, 1, 0, 1, 0, \ldots 0.$$

Suppose further, that if we had infinite data, there would be a limiting distribution to the frequencies of 0s and 1s in the sequence, with $P(X = 1) = p$, and $P(X = 0) = 1-p$. The actual empirical distribution — determined by the relative frequency of 1s and 0s among the available measurements is $\hat{P}(X = 1) = \hat{p}$ and $\hat{P}(X = 0) = 1 - \hat{p}$. Reichenbach claims that there is a higher-order probability q that states that $P(|\hat{p} - p| < \epsilon) = q$ for small ϵ, i.e. the true distribution falls with probability q within ϵ distance of the empirical distribution. According to Reichenbach we have an estimate of such a higher-order probability q by considering several sequences of measurements, each with their own empirical distribution. So suppose we had three sequences of measurements (say, from different experiments of the gravitational constant on (a) the moon, on (b) some planet and (c) using the Cavendish balance):

$$\begin{aligned} a: &\quad 1, 1, 1, 0, 0, \ldots, 1 \\ b: &\quad 0, 1, 1, 1, 1, \ldots, 0 \\ c: &\quad 0, 0, 1, 0, 0, \ldots, 1 \end{aligned}$$

Each will have a certain empirical distribution, say $\hat{P}_a, \hat{P}_b$ and $\hat{P}_c$. These three empirical distributions form their own sequence of values of $\hat{p}$, namely,

$$\hat{p}_a, \hat{p}_b, \hat{p}_c$$

each specifying the relative frequency of 1s in the individual sequences. $\hat{p}_a, \hat{p}_b, \hat{p}_c$ again determine an empirical distribution, but now of higher-order probabilities. Suppose it is the case from the three initial distributions that $\hat{p}_a = 0.8, \hat{p}_b = 0.7$ and $\hat{p}_c = 0.79$. Again we can classify these values according to some approximation bound, e.g. $0.8 \pm \epsilon$, where $\epsilon = 0.05$. In that case the (empirical estimate of the) higher-order probability q is $\hat{q} = 2/3$. The relative frequency of 1s in a single row indicates the probability of truth of the statement about the gravitational constant for the particular test object, e.g. the planet. The second-order probability q across the different sequences indicates the probability that the first order probability claim is true. It is this kind of mutual validation of convergence across different measurement sequences that supplies, according to Reichenbach, a probability of convergence of any empirical distribution.

In another analogous example involving measurements of the melting point of different metals, Reichenbach argues that the fact that many metals have a melting point gives us reason to believe that a metal which we have so far not seen melt, will nevertheless probably have a melting point. Despite the fact that for this apparently "solid" metal the empirical distribution of measurements appears to indicate that the probability of having a melting point is zero, the second order probability that determines how indicative the empirical distribution is of the limit, will be very low, because the second order probability integrates the findings from the other metals. Reichenbach refers to these "cross-inductions" as providing a "network of inductions".

Another way of thinking about Reichenbach's approach is to consider a hierarchical Bayesian procedure: Measurement data is used to estimate certain distributional parameters of a quantity of interest. But one may describe these parameters by a higher order distribution with its own hyper-parameters. In that case several measurement sequences could be used to gain estimates of the hyper-parameters. Once these are estimated, one can then re-compute the lower level parameters given the estimated hyper-parameters. This enables a flow of information between different sequences of measurements via the hyper-parameters and therefore provides a broad integration of data from different sources. As in Reichenbach's account, one can continue this approach to higher orders with hyper-hyper-parameters. At some point the question will arise whether one has enough data to estimate the high-order parameters. Reichenbach claims that at some higher order, blind posits replace the probability estimates to avoid an infinite regress of higher-order probabilities.

Hierarchical Bayesian methods work well theoretically, but they hinge on being able to determine whether and in what sense events are similar such that they can be included in the same sample (that is then used to determine the probabilities). On Reichenbach's account the corresponding question regards the determination of reference classes.

Reference classes are tricky territory for Reichenbach, since he goes as far as to claim that we can determine the probability of a scientific theory. For example, to determine the probability that Newton's law of gravitation holds universally (rather than just for a particular test-object, as in the example above) Reichenbach claims that all available measurements of the gravitational constant must be placed in one sequence, and that

> "...we must construct a reference class by filling out the other rows [sequences of measurements] with observations pertaining to other physical laws. For instance, for the second row we can use the law of conservation of energy; for the third, the law of entropy; and so on." [Reichenbach, 1949c, p. 439f]

It seems obvious that the selection of the reference class is arbitrary here, but Reichenbach argues further that

> "...the reference class employed corresponds to the way in which a scientific theory is actually judged, since confidence in an individual law of physics is undoubtedly increased by the fact, that other laws, too, have proved reliable. Conversely, negative experiences with some physical laws are regarded as a reason for restricting the validity of other laws, that so far have not been invalidated. For instance, the fact that Maxwell's equations do not apply to Bohr's atom is regarded as a reason to question the applicability of Newton's or Einstein's law of gravitation to the quantum domain." [Reichenbach, 1949c, p. 440]

Just why the incompatibility of a set of equations, Maxwell's, with a model of the atom, should tend to invalidate another set of equations, Newton's, that are themselves incompatible with Maxwell's, we have no idea. We have no idea what the reference class here may be for such a probability transfer, nor what else the underlying reference class in this case might contain. 'indexcross-induction

It remains unclear what criteria Reichenbach had in mind to determine a reference class generally. Of course, the general idea is that events should somehow be of the same type, but not so similar that the variability of interest is precluded. Reichenbach claims that one should choose the narrowest reference class for which there are stable statistics (relative frequencies), and that the stability of statistics is determined at the level of advanced knowledge, i.e. at a high level of data-integration.[5] But this is obviously not an acceptable suggestion — it begs the question: The whole aim is to determine the limits of relative frequencies; requiring stable statistics in the first place is unhelpful. Trivially stable statistics are always available at the narrowest of all non-empty classes, the class containing a single event. Obviously, this could not have been Reichenbach's intention either. Reichenbach's recourse to advanced knowledge for these determinations of reference classes for lower level frequencies may be understood as pointing to blind posits — that the best one can offer is an educated guess, or just a guess. But then why not just guess at the lower level frequencies? Reichenbach discusses the determination of reference classes at length, but it is far from a precise account. Maybe there is ultimately some intuitive reference class, even when broad cross-inductions are made in science, but doubts remain whether there is any hope of spelling out such inferences in a formal probabilistic framework, and whether the result would then provide the basis for objective probabilities.

We summarize Reichenbach's account of the foundations of probability as follows: Probability is defined as a property of infinite normal sequences of events.

[5]Using the example of measurements of the gravitational constant Reichenbach points out that the sequence of measurements for the planet Mercury converges to a different limit than those of the other planets, which should therefore lead to a reconsideration of the reference class of planets into reference classes of planets near the sun, and those further away [Reichenbach, 1949c, p. 439].

Normal sequences capture many of the features of random sequences. Since we have no knowledge of the limit of infinite sequences of events, we build our inferences on finite initial segments of such sequences. We are sure to be on the right track as long as the higher order probabilities look promising. Higher order probabilities look promising when inductions from a broad variety of different measurement sequences provide similar results. Probabilities, including higher order probabilities, are therefore objective features of the world, since they are relative frequecies in sequences of events.

Tidy as this sounds, it seems to founder on the issue of choice of reference class. Reichenbach's doctoral thesis would have provided a seemingly simple answer to these difficulties: There, the reference class of events was easily determined by the class of causally independent and causally identical trials, and synthetic a priori knowledge. Having abandoned the a priori shore, Reichenbach found himself at sea.

4 PROBABILITY LOGIC

Once the binary truth values of traditional logic are replaced with continuously valued probabilities, then, according to Reichenbach, all forms of uncertainty present in the inferences of empirical science can be represented in a formal inference framework. By providing an axiomatization, Reichenbach places his probability logic within the formalist tradition of Hilbert and avoids recourse to an a prioristic foundation. He argues that the formalist requirements are achieved by showing that his probability logic requires no more than the axioms of standard propositional logic together with an interpretation of probabilities as a property of infinite sequences. Inductive reasoning is thereby reduced to deductive reasoning plus induction by enumeration of the appropriate sequences. Tautologies of traditional two-valued logic follow supposedly as a special case of this continuously valued logic. The sequences relevant to deductive logic are constant, and therefore their properties and the resulting inferences can be determined with certainty.[6] For empirical truths, on the other hand, the properties of the sequences cannot be determined with certainty — the sequences are only given extensionally — and therefore only weaker truth values (between 0 and 1, excluding the boundaries) are assigned. The probability logic provides a calculus for inferences given such weakly supported propositions. The inferences follow those of standard probability calculus, and so the intended model of reasoning in the empirical sciences is achieved. The justification for application of the probability logic is given by a convergence argument. We will discuss the details of the logic in three parts — the logical syntax and semantics, the interpretation, and the justification.

[6]Nevertheless, it remains unclear what Reichenbach would have thought about mathematical statements whose truth or falsity we do not (yet) know. What probability, if any, would he have assigned to the Goldbach conjecture?

4.1 Logical Syntax and Logical Semantics

Reichenbach's probability logic starts with the usual syntax of propositional logic: letters representing propositions are connected by the usual connectives of "and", "or", "negation", "implication" and "equivalence" (bi-conditional). However, unlike propositional logic, probability logic does not assign truth or falsehood to a proposition but a degree of probability p. In principle there are innumerable ways to interpret this new form of truth value, since it is introduced simply as part of a formal system. But since the aim is to capture standard probabilistic reasoning in the logic, the probability associated with a propostion should reflect the use of probabilities in science, which Reichenbach takes to be — as we saw above — the limit of relative frequencies.

Unlike propositional logic, probability logic cannot be compositional.[7] While in propositional logic, the truth values of individual propositions determine the truth value of any complex proposition, this is not the case for probability logic. Given the probability of proposition A and the probability of proposition B, the probability of the proposition $A \vee B$ is underdetermined. This is an obvious consequence from the undetermination in the mathematical calculus of probability, in which the set of marginals does not determine the joint distribution. Consequently, the specification of "truth"-tables in probability logic depends on the specification of a third quantity fixing the joint probability of the two (or more) propositions, and thereby determining the probability value of composite formulas. Reichenbach uses the conditional probability of B given A, since it can easily be formulated as a subsequence selection procedure. Given the marginals and the conditional, the probability value of any composite formula involving the standard binary operators is defined by the standard rules of the mathematical calculus of probability, e.g.

$$\begin{aligned} P(A \vee B) &= P(A) + P(B) - P(A)(B|A) \\ P(A \equiv B) &= 1 - P(A) - P(B) + 2P(A)P(B|A). \end{aligned}$$

The standard set of axioms for propositional logic are augmented by a set of axioms that Reichenbach had developed as the foundation of mathematical probability calculus [Reichenbach, 1949c, pp. 53-65].

UNIVOCALITY:

$$(p \neq q) \supset [(A \underset{p}{\ni} B).(A \underset{q}{\ni} B) \equiv (\bar{A})]$$

[7] In criticism of Reichenbach's probability logic (see below) Russell, Tarski and Nagel refer to a logic with the feature of compositionality — that the truth (or probability-) value of a complex proposition is a function only of the truth (or probability-) values of its component propositions — as the logic being "extensional". In contrast, in an "intensional" logic the truth value depends also on the content of the individual propositions. Their terminology is misleading, because it overloads the term "extensional" also used for sequences that can only be defined by enumeration. Furthermore, as Reichenbach notes in response (and we discuss below), his probability logic does not fit this dichotomy.

NORMALIZATION:

$$(A \supset B) \supset (\exists p)(A \underset{p}{\Rrightarrow} B).(p = 1)$$

$$\overline{(\bar{A})}.(A \underset{p}{\Rrightarrow} B) \supset (p \geq 0)$$

ADDITION:

$$(A \underset{p}{\Rrightarrow} B).(A \underset{q}{\Rrightarrow} C).(A.B \supset \bar{C}) \supset (\exists r)(A \underset{r}{\Rrightarrow} B \vee C).(r = p + q)$$

MULTIPLICATION

$$(A \underset{p}{\Rrightarrow} B).(A.B \underset{u}{\Rrightarrow} C) \supset (\exists w)(A \underset{w}{\Rrightarrow} B.C).(w = p \cdot u)$$

While the notation is cumbersome, the axioms are intended to express three simple notions: The first axiom ensures that the value of a probability is unique, the second axiom ensures that any probability with a non-empty conditioning set has values between 0 and 1, inclusive. Axiom 3 is supposed to ensure that the probability of mutually exclusive events is the sum of the event probabilities, and axiom 4 is the chain rule: $P(C, B|A) = P(C|B, A)P(B|A)$. The first three axioms are similar to Andrey Kolmogorov's axiomatization of probability, however the third axiom only ensures finite additivity [Kolmogorov, 1933]. In Kolmogorov's case, the chain rule follows from the previous three axioms, but Reichenbach requires the additional fourth axiom to switch between logical conjunction and mathematical multiplication.

Unfortunately, these axioms are not sufficient to provide an axiomatization of probability, since they do not ensure that the space the probabilities are applied to is closed under complementation and countable union, i.e. that it forms a sigma-field. In fact, as van Fraassen shows, limiting relative frequencies in infinite sequences do not actually satisfy these constraints [van Fraassen, 1979].

The set of axioms of probability are extended by one additional rule — the rule of induction, or the so-called "straight rule" [Reichenbach, 1949c, p. 446]:

> "**Rule of Induction:** If an initial section of n elements of a sequence x_i is given, resulting in the frequency f^n, and if, furthermore, nothing is known about the probability of the second level for the occurrence of a certain limit p, we posit that the frequency $f^i (i > n)$ will approach a limit p within $f^n \pm \delta$ when the sequence is continued."

Reichenbach considers this rule[8] to be the only necessary addition to the otherwise entirely formal logic to get inductive inferences off the ground: All inductive inferences on complex claims can be reduced by application of the earlier axioms to

[8] Reichenbach took this rule to instantiate C.S. Peirce's self-correcting method. See footnote on same page as citation.

this simple induction by enumeration. The rule is part of the meta-language, and cannot, like other rules of deductive inference, be reduced to the object language. It therefore does not follow the logical form of the previous axioms.

Leaving the details aside, if the axioms did form a logic that built up from sequences of propositions and performed the inferences of the mathematical probability calculus as sub-sequence selection operations on infinite sequences, then Reichenbach would have constructed a purely syntactic inductive calculus that includes probabilistic and deductive inferences based entirely on the enumeration of sequences.

But the situation is not quite so clear: Despite the appearance of a standard syntax on the surface, the proposed set of axioms together with the rule of induction contain a mixture of notations which is never fully spelled out: The probability axioms use mathematical relations and existential quantification over variables representing real numbers. This suggests that arithmetic must form part of the language. But neither the mathematical machinery nor even its first order component is extended to the variables representing sequences. These are open formulas, presumably universally quantified, following a propositional language extended by the probability implication. Reichenbach leaves the task of an explicit account of the syntax covering these two systems to the reader.

The formal semantics is thoroughly non-standard: a continuous truth value is determined by the limit of the relative frequency in the sequence associated with each proposition, or — for a complex formula – as a function (using standard probabilistic inference) of the relative frequencies in each of the sequences corresponding to the individual propositions, and the subsequence corresponding to the conditional probability. But this only hints at a formal semantics and it is by no means clear whether the gaps can actually be filled in, especially since the account depends on the accepted syntax: A formula with iterated probability conditionals, such as $(A \underset{p}{\Rrightarrow} B) \underset{q}{\Rrightarrow} C$, must either be disallowed by the formal semantics, or it must be interpretable in terms of subsequence selection rules. The former seems unlikely given Reichenbach's desire to cover all types of probabilistic inferences. In the latter case a formal semantics in terms of subsequence selection rules is ill-defined for iterated probabilistic conditionals, because the antecendent of the second probability implication, i.e. $(A \underset{p}{\Rrightarrow} B)$, is not the type of object that lends itself to a sequence interpretation in any obvious way.

4.2 Interpretation

In the first few decades of the 20th century, when Reichenbach developed his probability logic, there were several other proposals to generalizing two-valued logic to multi-valued and continuously valued logics to formalize modal reasoning. Reichenbach considered these attempts to be largely misguided, since they ended up as formal constructs with little or no relation to the use of modality in natural language [Reichenbach, 1934].

In contrast, with the inclusion of standard probabilistic reasoning, Reichenbach saw the crucial advantage of his probability logic in being able to model — in the sense of a rational reconstruction – scientific reasoning. That is, Reichenbach considered the formal semantics of his logic to be well "coordinated" with real scientific inference, because he had provided a procedure to go from experimental evidence to complex propositions, and back. As far as he was concerned, all the "coordination" work had already been done in developing the foundation of probability. Scientific evidence comes in the form of a sequence of events, the data. Scientific probabilities correspond to the relative frequencies of events in such a sequence of events. The evidence is described by propositions; each proposition simply describes an event, one datum. As a result, we obtain a sequence of propositions whose structure is isomorphic to the sequence of events, and consequently the probabilities can be used interchangably for the sequence of events and the sequence of propositons. The scientific situation is matched in the logic and therefore the interpretation and application of the logic to science is obvious — essentially inbuilt.

Almost. First, probability logic is a calculus of infinite sequences, but in science data is always finite. Second, in natural language we often assign probabilities to singular propositions for which there is no obvious corresponding sequence. It appears at least possible, that there are similar situations in science.

We start with the second: probabilities of singular propositions. Reichenbach claims that probabilities associated with single propositions are posits, or wagers (but not in any strict sense of Bruno DeFinetti). These posits can be either blind or appraised. Posits are blind when no data is available to inform the probability. Reichenbach does not give any explicit constraints on the form of a blind posit, but implicitly it is quite obvious that they should resemble a flat prior assigning equal probabilities to every possibility. A posit becomes appraised as soon as evidence becomes available, and should then correspond to the relative frequency of the relevant event in the data. For example (Reichenbach's example [Reichenbach, 1949c, p. 366f]), consider the proposition that Caesar was in Britain at a particular time. This proposition can be associated with a probability p, which would be a guess (blind posit) if no relevant evidence is known. But one relevant sequence of data, Reichenbach suggests, is the sequence of reports by historians about Caesar's activities. The probability of Caesar's visit to Britain is then the relative frequency that Caesar's presence in Britain at the time in question is reported in these historical records, and the initial blind posit p then becomes appraised.[9] The suggestion is that probabilities of single propositions are ultimately fictive or elliptic, referring to implicit sequences of relevant events.

Such a rendition seems unsatisfying. Perhaps Reichenbach's own example is not an ideal illustration. The transfer of probabilities from sequences to single

[9]We do not know how Reichenbach would distinguish the complete lack of mention of Caesar's whereabouts at a particular time, from the explicit mention of the absence or presence. It is possible that the selection of the appropriate reference class of events is supposed to address this problem, but there is no explicit procedure.

propositions seems more plausible for more scientific cases. For example, the claim that the probability that a particular individual atom will decay after time interval t may perhaps more reasonably be thought of as meaning that the probability refers to the relative frequency of decay after time t in a set of atoms. The reference to a larger population (even if perhaps not a sequence) seems more convincing here.

Even if we are able to make sense of single event probabilities as elliptic references to some larger reference class, such a reference class will always be finite in the empirical sciences. This then leads us back into the first concern. Reichenbach argues that his rule of induction solves the problem: Given a finite sequence, we have a determinate relative frequency for the event of interest. We posit this frequency as the limit of the relative frequency. This is a blind posit, since there is no reason to believe that the empirical distribution is indicative of the limit. But this posit can become appraised if we have several sequences of similar type. Consequently, the initial blind posit of the limit in an individual sequence is revised to the appraised posit.

Reichenbach's basic idea is that in the sciences, as in his logic, one pretends *as if*: The empirical distribution in the finite initial segment of a sequence should be treated as if it were infinite and therefore indicative of the properties of the infinite sequence. The properties of the infinite sequence should be posited (blindly) based on the initial segment. By aggregating the data in different ways into different sequences, these blind posits are supposed to become appraised, as if many blind eyes make vision. Reichenbach argues that under the assumption of a flat prior, his rule of induction corresponds to a Bayesian update (at least for the first update), and that appraised posits simply correspond to informative priors.[10] His hierarchy of higher order probabilities therefore reflects exactly the structure used in hierarchical Bayesian methods (though these were still unknown at his time).

How exactly a posit becomes appraised, and why its appraised value is unique, remains unclear. If sequences of measurements can be arranged in different ways that change which events are regarded as first order events[11], then the simple rule of induction reflecting the empirical frequencies will conflict for higher order claims with the results of a Bayesian update (even if the prior at the lowest level is flat). It appears that the rule of induction should only be applied at the most fundamental data level, thereby (presumably) also preserving the objectivity of probability statements. Higher order claims about convergence, or the integration of information from different domains (so-called "advanced knowledge"), however, is supposed to occur by a Bayesian update. Reichenbach does not discuss the mixture of these updating techniques and the implications anywhere.

[10]See [Reichenbach, 1949c, p. 326-333 and p. 441].

[11]Reichenbach's example with the measurements of the gravitational constant, discussed in the section on probability above, appeared to involve exactly such different forms of representation of the data.

4.3 Justification

Assuming the method of application of the probability logic is now clear, the remaining task is to explain why it is the right method to use.

Reichenbach's justification of the inductive rule, a convergence argument, is presumably his most disputed legacy. It goes like this: If we indeed use the empirical distribution to determine our probabilities as if it were the limiting distribution, then, as long as we adjust the empirical distribution whenever more data comes in, our probability judgments will converge to the true probabilities, if the empirical distribution has a limit.[12] The last condition is crucial. Of course, convergence can only occur if there is something to converge to, but not every infinite sequence has a convergence limit for the distribution of relative frequencies of its items. If there is a limit, then there is for every ϵ some N such that the empirical distribution is at most ϵ different from the limiting distribution. The catch is at what point the quantification over all distributions enters into the convergence statement. Since Reichenbach only assumes the existence of a limit (and even that only conditionally), he is only guaranteed pointwise convergence, i.e. that for every ϵ, and every limiting frequency, there is an N that ensures that the empirical frequency is within ϵ of the limit. This is to be distinguished from uniform convergence, where for each ϵ there is an N such that the divergence is bounded for all distributions. For uniform convergence, one can specify confidence intervals and convergence rates, for pointwise convergence one cannot. Consequently, Reichenbach's convergence argument — he calls it the principle of finite attainability [Reichenbach, 1949c, p. 447] — is extremely weak: the existence of a limit alone provides little assurance that the empirical distribution is representative, no matter how large the sample size: Although for every positive ϵ, for some finite sequence the straight rule will be within ϵ of the limit ever after, the length of that sequence is unknown: at no point does one know whether one is in the vicinity of the true distribution.

Reichenbach bolsters his justification with reference to his network of inductions: He argues that the network of higher order inductions ensures that convergence to the true distribution is faster than it would be just based on inductions on individual sequences. Reichenbach does not give a formal definition of the speed of convergence, but intuitively he thinks that convergence is faster because all the cross-inductions inform any inductive inference in a particular domain (via higher order inductions) by integrating findings from other domains. Given his use of higher order probability statements for the convergence statements, he even seems to suggest that uniform convergence may be obtained. Reichenbach gives no proof of such a result, it is not true without further assumptions, and if uniform convergence is not the appropriate characterization of faster convergence, then it remains unclear what the benefit of faster pointwise convergence is.

These concerns only apply if there is a limit in the first place. Reichenbach

[12]Incidentally, this might be the reason why Reichenbach did not consider it important to distinguish when the rule of induction and when a Bayesian update is appropriate: Since they both converge to the same limit, the concern is irrelevant. Of course, one may worry what happens before convergence.

argues that if there is no limit, then all bets are off anyway, i.e. then there is no alternative procedure that could generate inductive knowledge. Reichenbach compares his procedure to a clairvoyant, who claims to know the limiting distribution of the sequence [Reichenbach, 1949c, p, 476]. One cannot check — other than by induction — whether the person is in fact clairvoyant. The advantage of the proposed procedure is, according to Reichenbach, that it at least guarantees pointwise convergence, whereas the person claiming to be a clairvoyant could simply be wrong. None of his arguments are compelling; at the crucial junctures he is hand-waving.

An alternative way of looking at the question of justification is not to ask whether the proposed procedure succeeds in what it claims to do, but rather ask whether it is unique in what it claims to be doing. The answer is No, and Reichenbach discusses this.[13] There are many other procedures that exhibit pointwise convergence if there is a limit. In particular, the set of procedures that work like the straight rule, but which have an arbitrary function added that also converges pointwise, or procedures that make any arbitrary guesses up to a point in a sequence and use the straight rule thereafter, or procedures that add a function to the straight rule that converges to 0 as the sequence length increases. Reichenbach's response is somewhat confusing: On the one hand he acknowledges that such manipulated distributions might even lead to faster convergence under some circumstances, and that therefore these procedures are similarly legitimate; on the other hand he argues that his straight rule is in some not further specified sense unbiased and functionally simplest. Neither argument survives more careful analysis. Thus we are left with an extremely weak justification. Reichenbach thinks this is the best we can hope for without fooling ourselves with regard to the width of the inductive gap.

At various points, Reichenbach's justifications have a much less formal and objective character and appear more pragmatic in nature (e.g. [Reichenbach, 1949c, p. 481]). Reichenbach regarded probability as providing a guide for action, and so to a certain extent — since he denied that there can be any certain empirical knowledge — he regarded his theory as a reasonably workable procedure for providing a good guess as to which scientific theories might turn out to be useful. In particular, he claimed that knowledge of the existence of the actual limit of a data sequence would not make any difference to the practice of science. However, if this is his view, it is not clear why he did not simply focus on probabilities in finite sequences. At one point, in a reply to Russell (see below) he does just that, suggesting that all real probabilities are finite frequencies, and the discussion of limiting convergence is a fiction to justify our inductive procedures.

Last, let us return to the initial intention of representing inductive reasoning in science. Do we find evidence in scientific practice of the type of justification Reichenbach gives? In many cases pointwise convergence is not considered adequate. Instead, many scientists often work with much stronger assumptions — such as Gaussianity or some other parametric assumptions about the distributions

[13]See [Reichenbach, 1949c, p. 447 and section 88].

of events — which then allow them to derive rates of convergence and confidence intervals. Reichenbach may reply that such scientists are simply in denial about the limits of their knowledge, and that his inductive logic tells them why.

Some of the more recent developments in statistics and computer science using non-parametric approaches work with weaker assumptions. The assumptions are stronger than those underlying Reichenbach's probability logic, but the assumptions are still so weak that they only support asymptotic normality, i.e. that confidence intervals can only be given for the limiting distribution. If convergence rates are not known such methods are subject to similar concerns of what inferences one can draw given finite empirical data. In some sense, large simulations that are nowadays used to get an idea of the convergence rates for these procedures can be seen as providing the basis for "appraised posits" regarding the convergence rate. Maybe Reichenbach would feel vindicated.

5 CRITICS: POPPER, NAGEL AND RUSSELL

Reichenbach's proposal(s) for an inductive logic were widely read by the "scientific philosophers" of his time, and criticism came from all sides, most prominently from Karl Popper, Ernest Nagel and Bertrand Russell. Perhaps the most detailed and concise summary of points of criticisms was given shortly after the publication of the German edition of *The Theory of Probability* [Reichenbach, 1935c] in a review by Nagel in *Mind* [Nagel, 1936] (but see also [Nagel, 1938]). Some of the issues Nagel points to were not new, but had already been made by Popper, Tarski, Hertz and others. Some of the criticisms can be found again in Russell's *Human Knowledge, its Scope and Limits* [Russell, 1948]. Apportioning particular aspects of criticism to particular authors would therefore be misleading. Instead we focus primarily on Popper's criticism with regard to Reichenbach's assessment of probabilities for scientific theories, Nagel's criticism of the straight rule, and Russell's criticism of the logical foundation of the probability logic, full-well acknowledging that in each regard other authors (also beyond these three) have contributed to the relevant points.

In *The Logic of Scientific Discovery* [Popper, 1934] Karl Popper strongly rejects Reichenbach's probability logic, even before its most comprehensive exposition in *The Theory of Probability* is published in German in 1935. Popper had read many of Reichenbach's papers outlining his approach in *Erkenntnis*. He reiterates his views in a review of Reichenbach's *Theory of Probability* in *Erkenntnis* in 1935 [Popper, 1935]. He shares Reichenbach's view that we cannot determine scientific theories to be true, but he further thinks that one cannot even assign a positive probability to them. He sees no way that Reichenbach can get around either an aprioristic foundation of probability or an infinite regress of higher order probabilities, the first of which Reichenbach himself would deny, and the second of which Popper regards as inadequate to determine numerical values. Reichenbach had pointed to two alternatives to determine numerical probabilities of scientific theories. The first is to determine a sequence of singular statements that are experimentally testable consequences of the theory. The relative frequency of confirmed consequences in that sequence then determine the probability of the theory. The second alternative is to place the theory itself in a sequence of theories from the same reference class, and consider the relative frequency of true theories in that reference class to be indicative of the probability of the theory in question.‘indexRussell, B.

Popper dismisses the second alternative as ludicrous. First, there is no unique reference class of theories (even if there were a sufficient number of theories) that would determine the relative frequency, and second, even if there were, then the fact that theories in that sequence can be determined to be true or false (in order to determine the relative frequency) makes the determination of a probability of a theory redundant in the first place. Popper, of course, doubts that individual scientific theories can be determined to be true at all. To say that Newton's law of gravity is true, is just not the same as saying that the coin came up heads. If the theories themselves are only determined probabilistically, then that leads to an infinite regress of higher order probabilities. Since each of the probabilities in the infinite regress is smaller than 1, the probability of the statement at the first level must necessarily be zero. This point was pressed upon Reichenbach from many sides, including Russell, who saw it as an indication that two-valued logic is more fundamental than probability logic. C.I. Lewis debated the same point with Reichenbach in regard to the foundations of epistemology in an exchange of papers in the 1950s [Lewis, 1946; Lewis, 1952]. Reichenbach's response on this matter in a letter to Russell is opaque, but the idea seems to be that the higher order theories are not independent in probability, and so their joint probability is not their product, and hence does not go to 0:

> "Combining the probabilities of different levels into one probability is permissible only if special conditions are satisfied; but even then this combination cannot be done by mere multiplication. Let a be the statement: 'the probability of the event is 3/4', and let b be the statement 'the probability of a is 1/2'. In order now to find out what is the probability of the event, you have to know what is the probability of the event if a is false. This probability might be greater than 3/4. These values do not go to 0 (see *Wahrscheinlichkeitslehre* [German edition of *The Theory of Probability*], pp. 316-317). The product which you calculate is the probability, not of the event, but of the total conjunction of the infinite number of propositions on all levels, which of course = 0." [Reichenbach, 1978]

Popper (and Nagel) regard the first alternative as similarly hopeless, because they doubt that a scientific theory can be represented as an infinite conjunction of singular statements that can be tested individually. Furthermore, — an argument made by Popper, Nagel and several others — Reichenbach's view would imply that a theory that was disconfirmed by 10% of its predictions would be considered true with probability 0.9. More likely though, the critics suggest, it would be considered false.

Reichenbach's response (to Nagel's version of this criticism) in [Reichenbach, 1939a] and [Reichenbach, 1949c, p 436], does not address the points. He evades the criticism by claiming that his account provides a consistent interpretation of what scientists might mean by assigning a probability to hypotheses, and in the English addition of *The Theory of Probability* he claims:

> "...if the limits of exactness are narrowly drawn, there will always be exceptions to scientific all-statements; [...] it is true that for wide limits of exactness, [...] a case of one exception is regarded as incompatible with the all-statement [...] This attitude can be explained in two ways. First, the degrees of probability for such all-statements are usually so high that one exception, in fact, must be regarded as a noticeable diminution of the degree of probability. Second, one exception proves that an all-statement is false, and we dislike using an all-statement as a schematization if it is known that the all-statement is false." [Reichenbach, 1949c, p. 436]

It is a puzzling claim for someone to make who takes all empirical claims to be probabilistic. In the case of a highly confirmed theory a purported counterexample would be a black sheep among many positive instances. Given that in Nagel's example the probability of the theory is determined by the relative frequency of positive instances (rather than by a Bayesian update), the impact of the counterexample on the probability of the theory shoul be relatively minor. Hence, we do not claim to understand how this statement improves Reichenbach's situation in light of the criticism.

For Popper, any third procedure to determine the probability of scientific hypotheses — such as using posits — is subjective.

Nagel [1936] commends Reichenbach's efforts to give a precise presentation of the frequency interpretation of probability, but he does not share Reichenbach's conviction that such an interpretation together with the proposed formal machinery in form of a probability logic is sufficient to represent and justify formally inductive inferences, nor does he believe that it is an adequate description of inductive inferences in the sciences.

The whole problem, as Nagel sees it, is that probability statements are not verifiable, because they are based on sequences whose limit we do not know. We do not even know if the sequence has a limit at all. Nagel is unconvinced by Reichenbach's proposal of "inductive verifiability". He points out the lack of mathematical proof that a sequence of higher-order probabilities or a network of cross-inductions lead to faster convergence. And even if they did, Nagel does not see any value in a convergence guarantee if the point of convergence is not known. In a response to a similar point made by Hertz [1936], Reichenbach only reiterates his view that his inductive procedure is the best one can hope for and that no other procedure will do better [Reichenbach, 1936].

Much later a detailed analysis of the possibility of proofs for the claims underlying Reichenbach's justification of induction and his use of higher order inductions is given in [Creary, 1969]. Creary concludes his assessment with the remarks (ch. 5, p. 129):

> "...we have argued that:
>
> 1. MC [method of correction, using cross-inductions] provides no rationale for the choice of a lattice [e.g. as in the earlier example, generated from the sequences of measurements of the gravitational constant from different planets] into which to incorporate a given sequence.
> 2. Mere superiority of MC to RRI' [Reichenbach's Rule of Induction, i.e. the straight rule; see above[14]] is not sufficient to justify the choice of MC over simpler lattice-convergent alternatives other than RRI'.
> 3. The superiority (indeed, even the parity) of MC vis-a-vis RRI' does not follow from theorems (1)-(4) (Reichenbach's theorems of convergence underlying his justification, [Reichenbach, 1949c, pp. 466-467]; or [Creary, 1969, p. 119]).
> 4. The theorems (1)-(4) themselves depend upon assumptions which would prevent any results established with their help from having the sort of justificatory import intended by Reichenbach."

The assessment is negative in every respect and vindicates Nagel's concern. More generally, Nagel does not think that Reichenbach's probability logic provides an adequate description of scientific practice. According to Nagel, scientific statements are considered probable not on the basis of some formal account of probability, but instead because there are no alternative plausible candidate theories, the theory has a certain aesthetic appeal, or because it is (largely) consistent with the available evidence. Nagel emphasizes throughout his review that he believes that there are several more or less formal notions of probability, and that several other human aspects enter into the judgment of the probability of truth of a scientific theory.

Nagel also picks up on a point of criticism pressed upon Reichenbach by Tarski [Tarski, 1935]. The concern is whether the probabilities (which are supposed to replace truth values) in the probability logic, constitute a syntactic relationship between statements only, or a semantic relation between statements and what is described by the statements. Nagel argues that Reichenbach's probability implication appears to involve both a "semantic and syntactic characterization" of relations between statements. It is not entirely clear, what Nagel means, but one source of contention appears to be that the assignment of probability values to composite statements in Reichenbach's probability logic involves not only the

[14] Creary includes some slight modification on p. 115 of [Creary, 1969].

probabilities of the individual statements, but a third (conditional) probability, specifying the amount of "coupling" between the statements. This third probability, since it is based on a subsequence selection procedure, appears to introduce an undesired further syntactic constraint into the semantic relation of the statements and what the statements refer to. Nagel (and Tarski) refer to this problem as the probability logic not being "extensional".

Reichenbach accepts that his probability logic does not conform to such strict constraints of "extensionality",[15] but that the probability logic is also far from being the opposite, namely "intensional", i.e. that the probability values of statements depend on the content of what they describe. Reichenbach considers his introduction of the conditional probability to be a natural extension of the traditional dichotomy that is necessary when considering continuous valued logics [Reichenbach, 1939b]. It is doubtful whether this would have satisfied Nagel or Tarski, but it is also somewhat unclear in this discussion what the standards are, and how they can be applied to probabilistic logics.

Russell [1948] shares the concern about the status of Reichenbach's probability logic, but for a different reason. He uses a very intuitive mathematical example of the probability that an integer chosen at random will be a prime [Russell, 1948, pp. 366-368] to illustrate his point. He shows that depending on the order in which integers are arranged, Reichenbach's definition of the probability, can be made to return any value between 0 and 1. This leads to the uncomfortable conclusion that Reichenbach's definition of probability depends on the order within sequences. In fact, Reichenbach, in his response to Russell [Reichenbach, 1978] agrees with this conclusion, and emphasizes that this aspect is one of the important contributions of his theory over theories of probability based on classes, such as Maynard Keynes'. Russell, in contrast, takes this constraint to indicate that Reichenbach's foundation of probability cannot be stated in abstract logical terms, because the order of events seems to introduce a contingent, non-logical aspect.

Russell has little hope that the probability logic could be shown to be fundamental. He thinks that

> "...there is a great difficulty in combining a statistical view of probability with the view, which Reichenbach also holds, that all propositions are only probable in varying degrees that fall short of certainty. [...] Statistical probability can only be estimated on a basis of certainty, actual or postulated." [Russell, 1948, p. 368f];

and he points to the fact that even in Reichenbach's rendition, two-valued notions of truth still play the most fundamental role, which suggests that Reichenbach did not even achieve the goal he set himself.

Russell's view of Reichenbach's theory can be summarized as follows: If the probability values of statements depend on the limit of an extensionally given

[15] We used the term "compositionality" above.

sequence, they are for ever unattainable. If they depend on a hierarchy of higher-order probabilities, those higher-order probabilities cannot all be determined. If they are given by posits, then those posits are sensitive to the order of events. Russell did not share Reichenbach's indifference with regard to the truth value of posits, and it is doubtful whether he accepted Reichenbach's response that "blind posits are justified as a means to an end, and that no kind of belief in their truth is required" [Reichenbach, 1978, p. 407]. For Russell, infinite regresses and hypothetical posits or infinite (extensionally given) sequences cannot form the foundation of a logical calculus.

Instead, Russell suggests that Reichenbach should build his probability logic on finite sequences. Probabilities, as they are used in the sciences only refer to reasonably large, but not infinite, populations anyway. The finite aspect would then also ensure that probability values can be precisely estimated, and probabilistic statements can be determined to be true or false, thereby returning to a two-valued logic as a foundation.

To a certain extent, Russell's criticism repeats Nagel's, who regarded Reichenbach's rule of induction not as progress across the inductive gap, but just as a restatement of the original problem. Nagel had argued that since Reichenbach's definition of probability is given in terms of two-valued logic, the entire probability logic should be re-statable in two-valued logic (since Reichenbach places no constraints on the complexity of statements). If such a translation is possible, then what use is the probability logic? If such a translation is not possible, then in what sense does the purported solution to the inductive problem in probability logic provide a solution to the traditional problem of induction in two-valued logic?

Reichenbach responds by distinguishing the formal probability logic, which he says can indeed be reduced to two-valued logic, and the applied probability logic, to which two-valued logic can at best be an approximation [Reichenbach, 1935d]. He compares the situation to a representation of non-Euclidean space in Euclidean space, but at least for us, the analogy does not make things clearer.

6 REICHENBACH ON THE ATTEMPTS OF OTHERS AND ON STANDARD PROBLEMS

Unsurprisingly, Popper's falsificationist account of theory testing was most unpopular with Reichenbach. Reichenbach considered Popper to be in denial about scientific practice and the implications of Popper's own theory [Reichenbach, 1935b]. Reichenbach thought one has a choice: Either one can take into account the actual non-definitive nature of scientific results, in which case a probabilistic account is necessary, or one can schematize the procedures and instead of probabilities, just use 0 and 1 as a discrete representation. If one does the latter, then one has to accept that scientific theories can both be verified and falsified. If one does the former, then it is impossible to verify or falsify with certainty. In either case, the asymmetry Popper tries to place between verification and falsification does not exist.

Furthermore, Reichenbach considered Popper's measure of corroboration and severity of testing to be a probability in disguise, only that Popper's account lacks the precision to actually make it a metric. Reichenbach was not genteel about the matter: He compares Popper to a fruit vendor, who stacks all the good fruit at the front of the display, but only fills the bags from the back, and then denies that his method has anything to do with the differential quality of the fruit. Popper uses a method to differentiate unfalsified theories, but presents no justification of why, what he is doing is reliable. Instead, he derides any attempts at justification of the methods as "metaphysical beliefs" (which neither Popper nor Reichenbach want their work to be accused of).

Against the "deductive" approaches to probability logic, such as those of Rudolf Carnap and Carl Hempel ([Reichenbach, 1949c, p. 456-461], see also [Reichenbach, 1935a]), Reichenbach argues that they do not provide an adequate space to define a probability metric. In retrospect this criticism is ironic, since Reichenbach himself did not specify a space that satisfies the conditions of a sigma field. Carnap introduces probabilities on an a priori foundation, which leads Reichenbach to claim that it is therefore difficult to distinguish the resulting theory from "a prioristic methods like the principle of indifference" [Reichenbach, 1949c, p. 456]. It is difficult to know what Reichenbach might have meant here. His usual criticism against the principle of indifference concerns its subjective component, whereas his criticism of a priori methods usually concerns the justification for such knowledge. Whatever the specific concern, the basis for determination of the probability metric is at issue. Against Hempel (and Helmer and Oppenheim), Reichenbach argues that they acknowledge that there are no a prioristic grounds to select a probability metric, but that their efforts to use some initial data and a maximum likelihood assumption to determine the probability space depends on the assumption that the data are independent [Reichenbach, 1949c, p. 456]. In contrast, his network of inductions that integrates information from many sequences of measurements in "advanced knowledge" does not make such an assumption, but tests it. He appears to regard the fact that assumptions are not set in stone as an important advantage of his system (see e.g. [Reichenbach, 1949c, p. 464]).

Approaches based entirely on maximum likelihood Reichenbach sees as answering the wrong question, and therefore as inappropriate tools for science. In a criticism of Fisherian statistics [Reichenbach, 1949c, p. 454f] Reichenbach points out that the likelihood of a hypothesis is not of interest, but rather the inverse probability, i.e. the probability of the hypothesis given the data. Maximization of the likelihood is therefore misleading, since a consideration of prior probabilities may still reverse the ordering of the hypotheses based on the likelihood. Once the inverse probability is known, likelihoods can be disregarded. Reichenbach believed that his account of induction by enumeration would provide the prior probabilities that are required.

More generally, Reichenbach thought that once the probabilistic aspect of inductive inference had been taken into account in form of his probability logic all the so-called inductive paradoxes of his time also disappeared. He does not go into much detail, but it is obvious that he did not see reason for much debate. For example, he considered Hempel's ravens paradox to be based simply on a misapplication of converse reasoning to probabilistic inference [Reichenbach, 1949c, p. 434f]: "If something is a raven, then it is black." can be highly probable, but that does not imply that "If something is non-black then it is a non-raven." is highly probable as well. It depends, as can easily be seen from a simple application of Bayes rule, on the base-line probability of ravens and black things. There is, according to Reichenbach, no paradox to be had in the first place, he simply takes a Bayesian view of probabilistic confirmation relations, and the problem is solved.

Reichenbach's discussion of Goodman's new riddle of induction is similarly brief [Reichenbach, 1949c, p. 448f]. He argues that the "grue"-predicate does not form a good basis for induction because it violates the principle that one should use the narrowest reference class. Reichenbach admits that the rule of induction does not ensure against false posits in the short run. That is, if grue is defined to mean green until time t and then blue, and we have a sequence of elements that are in fact all green, then "All emeralds are grue." would be confirmed (and therefore source of erroneous posits) up to time t, but then disconfirmed. As such, Reichenbach sees no problem with such an inference, since in the long run, one converges to the truth. But he argues that

> "...in advanced knowledge the inference can be shown to be inferior because it violates the rule, 'Use the narrowest common reference class available.' The property C [grue], by its definition, is identical with 'not B' [not green] from the $n+1$st element [time t]; since the reference class 'not B' [not green] is narrower than C [grue] it should be used as a basis for the inference (in other words, the property with respect to which the first n elements should be counted is 'not B' [not green])."

This argument makes no sense: Up to sequence item n grue and green have exactly the same statistics. How is one supposed to know in advance which one to choose? It appears that contrary to claims elsewhere, the "narrowness" of reference classes has nothing to do with stable statistics, but instead with other independent unspecified criteria of determining events.

As we argued earlier, Reichenbach's account of reference classes was never precise, but this response appears to misunderstand Goodman's concern altogether. Goodman's riddle points to the entire question of what should constitute a reference class (using Reichenbach's terminology), i.e. which elements should be included in a sequence that is used as a basis for a probability judgment. As far as we can tell, Reichenbach's response simply misses the point.

7 COMMENTARY

Reichenbach's inductive logic is a strange mix of mathematical precision and dodging the details. The aim is quite clear: Reichenbach intends to provide a probability logic that (a) is objective — hence the frequency interpretation; (b) not a prioristic — hence the empirical focus; (c) provides a rational justification of inductive reasoning in science — hence the straight rule; and (d) is sensitive to the uncertainties present in science — hence the many levels of probabilities.

The problem is that beyond the formalization of probability in terms of limits of relative frequencies, there is no real need for all the logical machinery of his account. If anything, it makes the account more cumbersome and confusing. The main lacuna of the inductive logic, as pointed out by several others, is the justification in terms of the straight rule. Reichenbach does not provide any mathematical proofs for his justification of faster convergence in terms of higher order probabilities, and it is doubtful whether they can be supplemented without adding further substantial assumptions at some level.

But even with regard to representing the uncertainty present in scientific inferences, it is not clear whether Reichenbach's probability logic really captures what is going on. The problem is that in science there are many different forms of uncertainty that Reichenbach represents in terms of just one type of probability: First, there is uncertainty because data is noisy, because measurements are subject to many residual influences, even in a well controlled experiment. This is the kind of uncertainty the theory of error deals with. Second, even if the data were not noisy, a finite number of measurements always underdetermines the law-like relationship which exists between the physical quantities. Third, there may be true uncertainty in the physical quantity. Interpreting Heisenberg's uncertainty principle as implying metaphysical uncertainty would yield uncertainty of this third type. Reichenbach at different points considers all three types of uncertainty.

While the first two sources of uncertainty can be regarded as epistemic, the last one is metaphysical. That is, if we had direct access to the truths of the real world, only the third type of uncertainty would remain. We do not have direct access to the truths of the world, so the scientific task is to reduce, as far as possible, the uncertainties of the first two types. Traditional accounts of inductive logic only attempted to account for the uncertainty of the second type, i.e. the uncertainty of which hypothesis is true given the data. Reichenbach considered these views to be wishful thinking, since they assumed that our scientific data was certain. Due to errors, scientific measurements are never certain, and so Reichenbach held the more general view that no empirical knowledge can be certain. The problem is that his probability logic does not separate the contribution of uncertainty from the different sources.

Consider the following example. Suppose for the moment, that we have a finite set of noise-free data for quantities (x, y) that are known to not be subject to any metaphysical uncertainty, i.e. we only have uncertainty of type two. Suppose further, that these data points all happen to lie on a straight line described by the formula $ax + b$ for some real numbers a and b. Given that the data are noise-free, whatever the true functional form of the law, it must pass through each data point. But obviously that does not uniquely define the function, since anything could happen in those parts of the space, for which no data points are available. So, the evidence does not imply the true law, and the question for Reichenbach is which functional form, if any, is better confirmed, more likely to be true or best justified — and in what sense?

Reichenbach does not resort to a naive answer that the data implies or that it is uniquely rational to believe that the simplest function (on some measure of simplicity) must be the true or best confirmed function. Instead he argues that we know for each function f that passes through the data points an (objective) probability p_f that f is the true function. This probability p_f is derived from several other, say, k data sets. If in $k \cdot p_f$ of those datasets the same function f describes the data, then p_f is considered to be the probability of function f being true.[16]

So far this is not some naive Bayesian (or other) bias based on the original data set that prefers simpler theories. But Reichenbach cannot (and does not seem to think he can) avoid the simplicity bias entirely, since of course there are still several functions that are confirmed by all the data-sets, but which differ from one another in parts of the space where none of the k data-sets contains a data-point. Here Reichenbach then does resort to a Bayesian account that prefers simpler theories — and no more detailed argument is given.

Even if this were an acceptable account for this second type of uncertainty, we still have to integrate the two other types: noise, and metaphysical uncertainty. One may, given the appropriate data sets, be able to use the differential likelihood to distinguish hypotheses that constrain parameters representing metaphysical uncertainty, from those hypotheses that do not. However, the moment that uncertainty due to noise is considered, the problem of identifying and allocating the uncertainty is in principle massively underdetermined. The hope of every scientist is that one finds circumstances in which the different sources of uncertainty can be teased apart. But Reichenbach makes no attempt in this direction with his formalism, although he certainly was aware of the problem: He was familiar with Heisenberg's results, which he took to imply metaphysical uncertainty, and he was well-versed in the field of error statistics.

The clean way to account for different sources of uncertainty is to represent them in the inference explicitly and separately, as is done in (for example) Jeffrey conditionalization [Jeffrey, 1983]. Jeffrey conditionalization explicitly represents the noise in the data as separate from the uncertainty with regard to functional form. It makes the confounding of these two types of uncertainty explicit, but also indicates how different assumptions can be used to tease them apart. Reichenbach, of course, never knew Jeffrey conditionalization, but we can guess that he would object to the proliferation of different probability distributions, which he would want to regard as objective probabilities, but for which an objective foundation seems far-fetched.

[16]There is an issue of whether the data sets are independent samples, but we leave that aside here.

Our rendition of Reichenbach's probability logic will surely not have done justice to every aspect of Reichenbach's attempts to provide an account of inductive reasoning. We have our doubts whether a clean account of the probability of scientific theories can be given in terms of Reichenbach's probability logic, or — for that matter — in terms of any purely formal system at all. We have also been critical of Reichenbach's justification of the straight rule. However, we do think that Reichenbach's approach to inductive inference that started by looking at actual scientific reasoning, is valuable. It presents a "proper" opposition to Popper's falsificationist accounts and avoids getting stuck in the philosophical quagmire of logical confirmation theory. Most of Reichenbach's probability logic is now mainly of historical interest, but some of his ideas regarding the search for objective probabilities in a Bayesian framework are still present in philosophical circles, and some ideas similar to his mathematical theory have been developed together with the appropriate precise mathematical formalism in modern statistics.

Notes on Sources: Our reconstruction of Reichenbach's probability logic is based primarily on his account in *The Theory of Probability* [Reichenbach, 1935c; Reichenbach, 1949c] and the various papers in *Erkenntnis* and elsewhere before [Reichenbach, 1931b; Reichenbach, 1932b; Reichenbach, 1934; Reichenbach, 1935e]. The development of Reichenbach's foundations of probability can be found in publications throughout his life, but especially in the following: [Reichenbach, 1915; Reichenbach, 1920; Reichenbach, 1925; Reichenbach, 1929; Reichenbach, 1932a; Reichenbach, 1933]. For further reference see also [Reichenbach, 1930; Reichenbach, 1931a]. *Experience and Prediction* [Reichenbach, 1938a] helps to piece together the big picture, and his many comments and responses to criticism in various journals, primarily [Reichenbach, 1935d; Reichenbach, 1936; Reichenbach, 1939b; Reichenbach, 1940; Reichenbach, 1949a], but see also [Reichenbach, 1938b; Reichenbach, 1939a; Reichenbach, 1941; Reichenbach, 1948], fill in some of the issues that remain unclear in the more thorough presentations of his view. Needless to say, on certain aspects we remained at loss with regard to what exactly Reichenbach had in mind.

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