Reichenbach, Russell and the Metaphysics of Induction

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Abstract

Hans Reichenbach's pragmatic treatment of the problem of induction in his later works on inductive inference was, and still is, of great interest. However, it has been dismissed as a pseudo-solution and it has been regarded as problematically obscure. This is, in large part, due to the difficulty in understanding exactly what Reichenbach's solution is supposed to amount to, especially as it appears to offer no response to the inductive skeptic. For entirely different reasons, the significance of Bertrand Russell's classic attempt to solve Hume's problem is also both obscure and controversial. Russell accepted that Hume's reasoning about induction was basically correct, but he argued that given the centrality of induction in our cognitive endeavors something must be wrong with Hume's basic assumptions. What Russell effectively identified as Hume's (and Reichenbach's) failure was the commitment to a purely extensional empiricism. So, Russell's solution to the problem of induction was to concede extensional empiricism and to accept that induction is grounded by accepting both a robust essentialism and a form of rationalism that allowed for a priori knowledge of universals. In this paper the significance of Reichenbach's solution to the problem of induction will be made clearer via the comparison of these two historically important views about the problem of induction. The modest but important contention that will be made here is that the comparison of Reichenbach's and Russell's solutions calls attention to the opposition between extensional and intensional metaphysical presuppositions in the context of attempts to solve the problem of induction.

Keywords: Induction, Extension, Intension, Metaphysics, Pragmatics.

1. Introduction

This paper addresses the problem of induction through the lens of Reichenbach's and Russell's attempts to resolve Hume's infamous problem in terms of their particular metaphysical commitments. This is potentially of great historical interest in and of it itself because they had a brief but relatively unknown exchange on the matter. But, it is also of contemporary interest, especially in virtue of the importance that metaphysical assumptions play in both attempts to ground induction and given the recent resurgence of metaphysics as a meaning-

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ful part of philosophy. Reichenbach's 1949 letter to Russell, in particular, makes it clear that the difference between these two attempts at solving the problem of induction is deeply rooted in their differing metaphysical commitments. In that letter Reichenbach addresses Russell's criticism of the pragmatic vindication of induction from Russell's 1948 book and this provided a useful opportunity for Reichenbach to both clarify his own views on induction and its metaphysical grounds and to show what the pragmatic vindication really amounts to. Here Reichenbach's and Russell's stances on the problem of induction and its general metaphysical grounds will be examined in some detail and an important and contemporarily relevant point about the interplay between metaphysical presuppositions and methodological resources will be made in light of the lessons learned from comparing their views.

2. Reichenbach and Russell on Induction: Setting the Stage

Hans Reichenbach's pragmatic treatment of the problem of induction (presented and developed in his 1938, 1949a, 1932/1949b and 1949c) is of great interest both historically and methodologically. However, various influential commentators have dismissed it as a pseudo-solution, relegated it to the scrap heap of bad philosophical theories or simply regarded it as problematically obscure.[1] So, it is not wrong to assert that Reichenbach's pragmatic vindication of induction has few contemporary followers, that it is not well-regarded and that it is not even clearly understood. This last point is, in large part, due to the difficulty in understanding exactly what Reichenbach's solution is supposed to amount to, especially as it appears to offer no epistemic response to the inductive skeptic. As Laurence Bonjour claims,

> the significance of Reichenbach's pragmatic justification remains obscure. As he himself insists, that justification still yields no reason at all for thinking that inductive conclusions, or any of the myriad further beliefs which are epistemically dependent on them, are to any degree likely to be true. The sort of justification in question is thus not epistemic justification, as that concept was construed above; to show that beliefs are justified in this alternative way does not answer, or even purport to answer, the basic skeptical worry about induction, and is indeed quite compatible with the deepest degree of skepticism. It is thus hard to see why it should be regarded as any sort of solution to the classical problem of induction (BonJour 1986: 99).

The more general dismissal of Reichenbach's views on induction and the negative assessment of his pragmatic vindication surely depends in some part on his adherence to the controversial frequency interpretation of the concept of probability.[2] This contention is especially poignant and likely to be part of the problem in virtue of the wide-spread popularity of subjectivism about probability that has dominated probability theory since Reichenbach introduced his views on the matter. So, this aspect of Reichenbach's views on induction and probability explains in part the charge of obscurity levelled against his pragmatic vindica-

[1] See, e.g., Skyrms 1966, Salmon 1966, BonJour 1986, BonJour 1992, BonJour 1998, Rosenkrantz 1981 and Kelly 1991.
[2] Hájek 1997 and 2009.

tion, but the issue of the interpretation of the concept of probability will not be the main focus here as it has been treated at length in a variety of other places.[3]

More importantly then, there is *another* prominent but much underemphasized aspect of Reichenbach's views that demands more attention and which helps both to more fully explain the significance of Reichenbach's views on induction and to dispel much of its alleged obscurity. This is his thorough commitment to an extensional metaphysics that compliments his empiricism. The contention made here is then that his commitment to a purely extensional metaphysics plays a deeply important role in this matter and that the failure to pay more careful attention to the role that extensionalism plays in his account of induction in part explains the negative reactions to Reichenbach's solution. That this aspect of Reichenbach's work has not been sufficiently emphasized is likely a consequence of the well-known anti-metaphysical stance of the Berlin Group and many of their contemporaries.[4] It is likely that their avowed doctrinal rejection of metaphysics obscured the significance of the underlying metaphysical commitments crucially involved in Reichabach's views on induction and which forced him to adopt a radical and purely pragmatic approach to the justification of induction.

For rather different reasons, the significance of Bertrand Russell's (1912 and 1948) classic attempts to solve Hume's problem is also both obscure and controversial. Russell accepted that Hume's reasoning about induction was basically correct, but he argued that given the centrality of induction in our cognitive endeavors something must be wrong with Hume's basic assumptions. What Russell effectively identified as Hume's (and ultimately Reichenbach's) failure was the commitment to a purely extensional empiricism. So, Russell's solution to the problem of induction was to concede extensional empiricism and to accept that induction is grounded by accepting both a robust essentialism and a form of rationalism that allowed for *a priori* knowledge of universals. Ultimately, this was supposed to be captured by a set of a priori knowable principles that would make inductive inference rational and would permit us to answer the inductive skeptic in an epistemic manner. Of course, this is especially ironic as Russell himself championed an extremely austere form of metaphysical conservatism in his own work at times.[5]

To be sure, neither of those views of induction is without its critics. On the one hand, as we have already seen, Reichenbach's solution importantly faces the charges of obscurity and of offering no epistemic response to the inductive skeptic. On the other hand, Russell's solution looks to be objectionably *ad hoc* absent some non-controversial and independent arguments to the effect that the universals that are necessary to ground the uniformity of nature actually exist and that they are epistemically accessible. This particular charge is especially likely to arise from those, like Reichenbach, who incline towards purely extensional forms of empiricism. In any case, here the significance of Reichenbach's and Russell's solutions to the problem of induction will be made more clear via the comparison of these two historically important views about the problem of

[3] See, e.g., Skyrms 1966, Salmon 1966, Galavotti 2011 and Teng and Kyburg 2001.
[4] See Carnap 1931, Ayer 1936, Friedman 1999 and Creath 2017 on the anti-metaphysical commitments of the logical empiricists/positivists and Reichenbach 1936 and Rescher 2006 on the Berlin Group and its related views.
[5] See Russell 1918.

induction. The modest but important contention that will be made here is that the comparison of Reichenbach's and Russell's solutions calls attention to the opposition between extensional and intensional *metaphysical* presuppositions in the context of their particular attempts to solve the problem of induction.[6] It will be show that, in effect, what Reichenbach does is to establish an important epistemic limitation of extensional empiricism. So, it will be argued here that there really is nothing especially obscure about Reichenbach's thoughts on induction at all and his views are not just an anachronism. He was simply working out the limits of extensional empiricism with respect to inductive inference. In fact, Reichenbach conveys this very point to Russell in his 1949 letter addressing Russell's criticisms of his approach to the problem of induction.[7]

More broadly, the point that can be drawn from looking at this bit of history is that methodological and epistemological debates like this one about the probity of induction cannot easily be disentangled from the associated metaphysical issues. In the narrow context of this particular debate what we can learn from the Russell/Reichenbach exchange is that the sort of justification that can be given for induction depends deeply on the austerity of one's metaphysics. In essence, in this debate it appears to be the case that the demand for ontological austerity comes with a price; viz. the need to entertain non-epistemic forms of justification. In the larger and more recent context of the debate about the justification of induction what has recently transpired is an increasingly widespread recognition that offering a substantial, successful and non-pragmatic justification of induction requires conceding both extensional and intensional metaphysics in favor of even more inflationary hyper-intensional metaphysics.[8]

[6] The fact that this particular aspect of these two very important twentieth century treatments of induction has not received more attention is curious, particularly as the distinction between intensionality and extensionality and the philosophical issues surround this distinction was *the* central feature of Carnap's (1947) magnum opus (which Reichenbach surely read) and it was an absolutely fundamental aspect of logical positivism, logical empiricism and related movements such as Reichenbach's Berlin Group. The distinction between intensional and extensional logics is of crucial historical importance in understanding all of these views and it is just as important for understanding Reichenbach's and Russell's views on induction as are the more familiar cluster of issues concerning empiricism, verification, etc. But, the intensionality/extensionality distinction has neither received the same degree of general attention—as Friedman (1999) and Creath (2107) amply demonstrate, largely via omission—nor, as the considerations presented here show, has it received adequate attention in the specific context of Russell's and Reichenbach's important debate about induction.

[7] This approach stands in sharp contrast to his early work which resembles Russell's approach in holding that the principle that grounds induction is synthetic a priori. See Eberhardt 2011 for discussion of Reichenbach's early views. He explicitly rejects this view in his later work and tells Russell in a 1949 letter that "Induction does not require an intensional logic" (Reichenbach 1949d: 410). In other words it does not require the assumption of robust laws of nature.

[8] See, for example, Kelly 2014, Spohn 2005 and Ortner and Leitgeb 2009. Following, Nolan (2014: 151) the distinction between extensionality, intensionality and hyper-intensionality can be made linguistically as follows. A linguistic position is extensional if other expressions with the same extension can be substituted into that position *salva veritate*. Importantly here, the extension of a predicate is the set of objects to which it correctly applies. A linguistic position is (merely) intensional just in case it is not extensional and expressions that are necessarily equivalent can be substituted in that position *salva*

This, in turn, reflects a more general movement in the broader discipline of metaphysics towards inflationary views involving hyper-intensionality.[9] In effect, all of this suggests that there are sometimes severe methodological and epistemological costs associated with austere extensionalist metaphysical views and this recognition has seemingly helped to fuel a renaissance in inflationary metaphysics that is surely—at least in part— motivated by the radical methodological costs often associated with the sort of metaphysical conservatism adopted by Reichenbach, Quine and others.[10]

3. Reichenbach's Pragmatic Vindication

Let us begin by recalling that Reichenbach's pragmatic justification of induction is based on the following (reconstructed) line of argumentation (i.e. *the basic Reichenbach argument*):

P1: Either nature is uniform or it is not.
P2: If nature is uniform, then scientific induction will be successful.
P3: If nature is not uniform, then no method will be successful.
∴ If any method of induction will be successful, then scientific induction will be successful.[11]

But, according to Reichenbach and echoing Hume, we cannot know whether nature is uniform or not because it is neither a matter that can be settled *a priori* nor is it a matter that we can non-circularly establish *a posteriori* if all we are perceptually acquainted with are particulars. So, as Reichenbach sees it, although we know that if any method is successful, then scientific induction will be successful, we cannot know that any method really is successful. The gist of his attempt to justify inductive practice then comes from the idea that while we do not know that any method will actually be successful we also do not know that no method will be successful. Given this result and the fact that scientific induction can be shown to be an optimal method (in this important sense of "optimality") we ought to accept induction as being justified, at least pragmatically speaking. As we shall see, what is at the heart of this view is Reichenbach's metaphysical commitment to a form of extensional empiricism that tolerates only the existence of and knowledge of particulars.

In any case, as Salmon correctly pointed out in his 1966, the Reichenbach argument depends on a false dichotomy. The uniformity of nature is, of course, not an all or nothing matter. We can, of course imagine possible worlds that contain only individuals with degrees of uniformity that vary radically. So, the uniformity of nature seems to be a matter of degree, and it is at least plausible to

veritate. Importantly here, the intension of an extensional set of objects is the defining property they share in common. Finally, a linguistic position is hyper-intensional just in case it is neither extensional not merely intensional. So, in hyper-intensional contexts even necessary equivalents cannot be substituted *salva veritate*.

[9] See, for example, Cresswell 1975 and Nolan 2014.

[10] See, for example, Quine 1948.

[11] This presentation of a simplified version of Reichenbach's main argument is taken from Skyrms 1966. It is important to note at this juncture that the various criticisms of Reichenbach's views, other than Russell's, will (for the most part) be ignored here. To address all of those criticisms would require too much space, and the point of this paper is more historical in any case.

believe that a measure of the uniformity of extensional worlds might be. If this turns out to be viable, given the space of possible worlds U, we could define a measure $m(x)$ on U such that $m(x)$ maps the elements of U into the continuous open interval [0,1] representing the uniformity of that extensional world. This suggests that Reichenbach's attempt to justify induction needs to be retooled in order to accommodate a concept of world-uniformity that admits of continuous degrees. When this is done we can usefully reformulate the basic Reichenbach argument as follows. Consider our world w_a (the actual world), where $w_a \in U$, with a fixed, but, unknown measure of uniformity, the set of all inductive methods Υ,[12] where $y_i \in \Upsilon$ and such that each inductive method has a probability of arriving at a true conclusion in its domain of application,[13] a function $f(m(w_n), y_n)$ that maps worlds with degrees of world-uniformity and inductive methods into the space of probabilities,[14] and a constant λ that represents the chance probability of an inductive method succeeding at a world.[15] If we understand ε as the degree of world-uniformity required for any inductive rule to be reliable with a reliability greater than chance,[16] i.e. greater than λ, then the more *sophisticated Reichenbach argument* can be stated as follows:

P1′: If the probability that $m(w_a) = 1$ is 1, then scientific induction will be successful.

P2′: If it is probable that $1 > m(w_a) > \varepsilon$ with probability less than 1 but greater than λ, then scientific induction will be successful with probability p, where $p > \lambda < 1$.

P3′: If it is probable that $\varepsilon > m(w_a) > 0$ with probability greater than 0 but less than λ, then scientific induction will be successful with probability p, where $p < \lambda < 0$.

P4′: If the probability that $m(w_a) = 0$ is 1, then no inductive method will be successful.

∴ If any inductive method will be successful, scientific induction will be successful.

[12] Inductive methods are, simply, rules for accepting conclusions concerning unobserved cases based on observed cases.

[13] The concept of the domain of application of an inductive method will be discussed at some length in what follows.

[14] The function $f(m(w_n), y_n)$ seems, intuitively, to be a natural sort of function, as degrees of world-uniformity seem to be closely related to the probability with which a method produces true conclusions. What $f(m(w_n), y_n)$ is supposed to yield is a probabilistic measure of the general reliability of a given method at a world with a given measure of uniformity, and, as we shall see subsequently, what this function really represents is the set of worlds where an inductive method with a well-defined probability of arriving at the correct value of a stable frequency will actually produce the correct values.

[15] In other words, λ represents the threshold at which methods are no better at producing true conclusions than randomly selecting conclusions from the set of all statements of a given language £, and, as we shall see, a method that performs at with a success rate no better than chance is no method at all. However, the general successfulness of an inductive method will turn out to be a more complex matter involving two aspects. The first concerns the reliability of the procedure in its domain, and the second concerns whether there exist elements of that domain at a world.

[16] Scientific induction is Reichenbach's inductive rule, and this rule will be presented formally in what follows.

It should be noted that Reichenbach's conclusion still holds in this case and we will consider the significance of this conclusion in what follows. However, before we proceed to do so, it will be instructive to reconstruct Reichenbach's and Russell's treatments of induction in much greater detail order to see just what they amount to and what they imply about inductive inference.

4. Reichenbach's Conception of Scientific Induction

The primary motivation that drove Reichenbach to propose his pragmatic justification of induction concerns a central feature of the frequency interpretation of the probability calculus. Familiarity with the details of the probability calculus will be assumed here, and with the fact that it is compatible with at least several interpretations. The axioms of the probability calculus are, of course, as follows:

(A.1) $P(a) \geq 0$ for all a in the domain of P(•).
(A.2) $P(t) = 1$ if t is a tautology.
(A.3) $P(a \vee b) = P(a) + P(b)$ if a and b and $a \vee b$ are all in the domain of P(•), and a and b are mutually exclusive.

Recall that on Reichenbach's frequency interpretation of probabilities such quantities are to be construed as tautological consequences of the probability calculus.[17] More importantly, probabilities are to be regarded as measures of the limit of the relative frequency with which one contingent property is associated with another in an infinite sequence. More formally, the relative frequency of a pair of properties in a sequence is to be defined as follows:

$$F^{n}(\mathrm{A}, \mathrm{B}) = N^{n}(\mathrm{A}, \mathrm{B})/N^{n}(\mathrm{A})$$

Here $F^{n}(\mathrm{A}, \mathrm{B})$ is the frequency of associated As and Bs in a sequence of length n. Given this conception of relative frequency we can then define the concept of probability as follows:

$$P(\mathrm{A}, \mathrm{B}) = \lim_{n \to \infty} F^{n}(\mathrm{A}, \mathrm{B})$$ [18]

Having introduced this notion of probability Reichenbach then proposes the rule of induction that states:

> 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 1949c: 47).

However, these definitions give rise to some very difficult but well-known problems concerning the existence of infinite sequences and the existence of such convergent limits.[19] We, as a matter of fact, are only ever aware of sequences that, as Reichenbach claims, "are not *intensionally* given, but are presented to us only by enumeration of their elements, i.e. are *extensionally* given" (Reichenbach 1949a: 309), and it seems that any such sequence of observed associations will

[17] See Reichenbach 1949b and Weatherford 1982, chapter 4.
[18] See 1949c for details concerning how this derivation is carried out.
[19] Sequences with convergent limiting frequencies are just those sequences that settle into stable frequencies in the limit.

be finite. Upon considering further extensional enumeration of the elements of a given observed sequence we find that such extended sequences are, in point of fact, compatible with any value of the limit frequency. If this is so, we might ask why we are entitled in any way to assume that the frequency of such an association in even very long sequences of observed associations in a population will justify our assertion that that frequency will not diverge in further extensive enumerations of that sequence.

Reichenbach tied the frequency interpretation of the concept of probability into the problem of induction in virtue of the following central claim:

> The aim of induction is to find series of events whose frequency of occurrence converges toward a limit (Reichenbach 1938: 350).

Of course, Reichenbach saw that this was just the *classical* problem of induction in a somewhat new guise, and he ultimately showed two things. First, he showed that, by definition, *if such a limit exists*, then the procedure of scientific induction will be successful, and, second, that scientific induction is at least as good as any other method in discovering what is really the case concerning the frequency of an association in a sequence. Reichenbach explains,

> Let us assume for the moment that there is a limit towards which the sequence converges, then there must be an n from which on our posit [the rule of induction] leads to the correct result; this follows from the definition of the limit, which requires that there be an n from which on the frequency remains within a given interval δ. If we were to adopt, on the contrary, the principle of always positing a limit outside $f^n \pm \delta$ when a frequency f^n has been observed, such a procedure would certainly lead us to a false result from a certain n on. This does not mean that there could not be other principles which like the first [the rule of induction] would lead to the correct limit. But we can make the following statement about these principles: even if they determine the posit outside $f^n \pm \delta$ for a smaller n, they must, from a certain n on, determine the posit within $f^n \pm \delta$. All other principles of positing must converge asymptotically with the first [the rule of induction] (Reichenbach 1949b: 316).

What he showed was that if a limit exists for a sequence, then by repeated application the rule of induction will lead to the value of that limit to any desired degree of approximation in a finite number of applications and that all other methods will asymptotically converge with the results of the rule of induction. So, in spite of the fact that we cannot know that the limiting frequencies of sequences exist, we might as well simply accept the rule of induction because it is the best method of all methods. In virtue of this Reichenbach's inductive rule might be leveraged into a bulwark against inductive skepticism if it can be shown justified in some sense. All methods are, in a sense, parasitic on the rule of induction. Again, this pragmatic answer to the problem of induction arose directly out of Reichenbach's recognition that, in point of fact, *we cannot know that such limits exist in our world*. We cannot know whether such convergent limits exist based on the empirical observation of associations in finite, extensionally given, sequences. So, we are stuck in the situation that either no method at all works or induction is the best of all methods. Reichenbach explicitly acknowledges this and explains that

> Now it is obvious that we have no guaranty that this aim is at all attainable. The world may be so disorderly that it is impossible for us to construct series with a limit. Let us introduce the term "predictable" for a world which is sufficiently ordered to enable us to construct series with a limit. We must admit, then, that we do not know whether the world is predictable. But, if the world is predictable, let us ask what the logical function of the principle of induction will be (Reichenbach 1938: 350-51).

In terms of the sophisticated Reichenbach argument this can be expressed as follows. If worlds are extensional then, we cannot know the real value of $m(w_a)$. Nevertheless, it will be true that if $1 \geq m(w_a) > \varepsilon$, then scientific induction will be successful. If this is not the case, then no method will be successful and the possibility of doing science is a wash. So, as Reichenbach sees it, we are faced with a puzzle and he sees the way out as follows:

> If we cannot realize the sufficient conditions of success, we shall at least realize the necessary conditions. If we were able to show that the inductive inference is a necessary condition of success, it would be justified; such a proof would satisfy any demands which may be raised about the justification of induction (Reichenbach 1938: 349).

Building on this he then tells us of the principle of induction and its frequency interpretation that,

> This procedure must at sometime lead to the true value *p*, if there is a limit at all; the applicability of this procedure, as a whole, is a necessary condition of the existence of a limit at *p* (Reichenbach 1938: 351).

But, this does not yet indicate what sort of further justification can be given for the inductive principle and Reichenbach is clear that all of this noted thus far is compatible with a thoroughgoing skepticism about inductive inference. So, what about the matter of justifying the sufficient condition of inductive success (i.e. the existence of the limits of relative frequencies)?

Reichenbach tells us that we can treat the existence of such limits of relative frequencies as *posits*, where posits are not to be treated as beliefs in the normal sense, but rather as a kind of wager concerning what would be most advantageous to us. It is then here that we find the introduction of the idea of the justification of induction as *pragmatic vindication*. Reichenbach explains that,

> It is evidently the concept of posit which we have to employ for an explanation of this method. If in the finite section given we have observed a certain frequency f^n, we posit that sequence, on further continuation, will converge towards the limit f^n (more precisely: within the interval $f^n \pm \delta$). We posit this; we do not say that it is true, we only posit it in the same sense as the gambler lays a wager on the horse which he believes to be fastest. We perform an action which appears to us the most favorable one, without knowing anything about the success of this individual action (Reichenbach 1949b: 315).

Furthermore, as all other rules are parasitic on the rule of induction it is only natural to lay our wager on that rule. We are wagering that $1 \geq m(w_a) > \varepsilon$. So, the sort of justification his argument provides is clearly a matter of pragmatics.

But, in any case, the kind of wager involved in positing the existence of convergent limits in infinite sequences is not the typical kind of wager that a gambler makes. Normally, a gambler at least knows the odds with which he is confronted and so can make an informed decision about what outcome to bet on (i.e. which is the best bet) but in the case of the limits of infinite sequences we are making the posit that the limit converges to f^n blindly; i.e. we are making this posit when do not know the odds and so we do not know if it is the best posit.

Reichenbach claims that in such cases we are making what he calls an *approximative posit* concerning the existence of such limits. We are blindly wagering that $1 \geq m(w_a) > \varepsilon$. As we have seen, Reichenbach shows that *if we are right about the existence of such a limit* (if this blind wager is correct) then, induction will be successful and if any other method is successful, then scientific induction will be successful. *If we are wrong about the existence of such a limit* (if this blind wager is not correct), then if any other method is successful, then scientific induction will be successful in this more restricted sense. Therefore, scientific induction is at least optimal in this specific sense. However, as Bonjour notes in the passage quoted in section 2, this by no means shows that induction is justified in the traditional sense, and Reichenbach's view is apparently compatible with radical skepticism concerning the probity of induction. It may simply be false that $1 \geq m(w_a) > \varepsilon$ and given extensional empiricism we cannot know whether this claim is true or false. So, as far as we know, the method of induction might well be the best of a bad lot. Nonetheless, Reichenbach argues that there is a sense in which his argument *vindicates* induction. It does show that if any method works, then induction works. We do not *know* that it is unreliable, but we know that it is the best method if any method is reliable. Importantly, we do not know that the claim that there are such limit frequencies is false. Reichenbach explains that,

> to renounce the assumption of induction would be necessary only if we knew that the assumption is *false*. But that is not the case—we *do not know* if it is true or false. And that is quite another matter! Without believing that the assumption is true or false we are still justified in defending it in the same way we make a wager. We want to foresee the future, and we can do it if the assumption of induction is justified—and so we wager on this assumption. If it is false, then our efforts are in vain; but if we use the principle of induction we have at least a *chance* of success (Reichenbach 1936: 157).

So why not commit ourselves to the use of scientific induction? Of course, this will not likely be a satisfactory justification for someone who has sympathies with BonJour's inductive skeptic, but it is clearly to our practical advantage if scientific induction turns out to be reliable. More importantly for the purposes of this discussion, what this result really establishes is that given extensional empiricism induction can *only* be pragmatically justified in the sense of Reichenbach's vindication.[20]

[20] See Shaffer 2017a for a fully formal reconstructions of Reichenbach's pragmatic vindication of induction based on both the maximin principle and the dominance principle.

5. Russell's Justification of Induction

Unlike Reichenbach, Russell ultimately found that consideration of the problem of induction demanded that we give up the commitment to a purely extensional empiricism in favor of an intension friendly quasi-empiricism. In his *The Problems of Philosophy* Russell explains that the principle of induction has two parts as follows:

> (a) When a thing of a certain sort A has been found to be associated with a thing of a certain other sort B, and has never been found to be dissociated from a thing of the sort B, the greater the number of cases in which A and B have been associated, the greater is the probability that they will be associated in a fresh case in which one of them is known to be present.
> (b) Under the same circumstances, a sufficient number of cases of association will make the probability of a fresh association a nearly certainty, and will make it approach certainty without limit (Russell 1912: 66).

He then notes that the assignment of these sorts of probabilities is always relative to evidence, relevantly here these assignments are relative to the known cases of A and B being associated. But, there may always be other data which we are not in possession of that would force us to accept that we have wrongly estimated the probabilities in question. So, he concludes that evidence cannot disprove the principle of induction because the failure of an A's being associated with a B, when they have been associated in the past, cannot refute the claim that they are probably associated. More crucially, Russell also specifically tells us that,

> the principle of induction, while necessary to the validity of all arguments based on experience, is itself not capable of being proved by experience, and yet is unhesitatingly believed by everyone, at least in all its concrete applications (Russell 1912: 70).

So, the principle of induction can neither be proved nor disproved *by experience*. But, what is then crucial in Russell's thinking is his claim that,

> all knowledge which, on the basis of experience tells us something about what is not experienced, is based upon a belief which experience can neither confirm nor confute, yet which, at least in its more concrete applications, appears to be a firmly rooted in us as many of the facts of experience (Russell 1912: 69).

Russell then proceeds to argue that if the principle of induction is justified at all, then it must be justified *a priori*. The only other possibility, which Russell briefly notes in both *The Problems of Philosophy* (Russell 1912: 60-90) and in *Our Knowledge of the External World* (Russell 1914: 44), is that such a principle is analytic and so is to be accepted or rejected as a matter of convention. However, then, of course, it would not be epistemically justified at all, and so our inductive practices would also fail to be justified. So ultimately in his 1912 Russell famously sets out to explain how we can have *a priori* knowledge of the principle of induction, the principle of the uniformity of nature that would ground our knowing that $m(w_a) = 1$. In doing so he explicitly concedes extensional empiricism and (in terms of the sophisticated Reichenbach argument) he argues that we can know a priori that the probability $m(w_a) = 1$ because we can know a priori that universals sufficient to ground inductive practice exist.

Russell's desire to offer a solution to Hume's problem then became an exercise in explaining our knowledge of universals and this is because Russell saw that the ever-changing order of particular experiences would have to exhibit real and general structure if induction was to be justified.[21] Russell begins by explaining that,

> all our *a priori* knowledge is concerned with entities which do not, properly speaking *exist*, either in the mental or on the physical world. These entities are such as can be named by parts of speech which are not substantives; they are such entities as qualities and relations (Russell 1912: 90).

Moreover, these sorts of things are ultimately identified as universals and real objects instantiate such relations. But since universals have a form of being very different from ordinary physical objects (or sense-data), knowledge of them must be acquired in a very different manner than that by which we come to know particulars. According to Russell, knowledge of many universals (e.g. redness) is acquired through perception, and in doing so he argues that there is more in the content of our experiences than just information about concrete particulars.[22] But, in many cases, knowledge of the relations between universals is not purely empirical in nature. Unlike our knowledge of universals such as redness, whiteness, etc., that involves fairly straightforward abstracting from our acquaintance with a number of particulars that share some universal in common, our knowledge of relations—specifically of the principle of induction—is of a rather different nature, primarily because it is much more abstracted and distant from our acquaintance with particulars. In fact, Russell (1912: 103) tells us that, "*all a priori knowledge deals exclusively with relations of universals*". Russell explains that in the case of these sorts of logical principles we then can only have immediate or intuitive knowledge of such truths and that what is known in this manner is self-evident even though what is known this way "exists" only in some fairly robust Platonic sense outside of the physical world. He is nevertheless clear that self-evidence is a matter of degree and that the principle of induction is not as self-evident as some other logical principles (Russell 1912: 117), but it is known a priori in this direct manner. More importantly, it is a relation that is a universal and it alone grounds inductive practice. If there is no such relation and there is no such structure to the world, then no inductive inferences are justified and this required Russell to concede the austere metaphysical atomism and empiricism that he subscribed to in his 1918. But, Russell did not have much more to say about the issue of induction and its grounds or about resolving the incongruity of his views about this issue in his 1912 and 1918 works until considerably later in 1948. This is simply because during this extended period his attention was directed to other issues having more to do with social and political philosophy.

The more robust and sophisticated approach to the problem of induction that was suggested by Russell in his 1912 is developed and more fully and extended in his 1948 book *Human Knowledge.* It is here then that he fully frees him-

[21] Russell defended the existence of universals on a different basis in his earlier 1911 and later in his 1948.

[22] See Russell 1936: 140 and 148-49.

self from the chains of his 1918 extensionalism and empiricism and articulates how this allows for the justification of induction. Moreover, in this work he makes his objections to Reichenbach's view clear. Russell sums up his developed view in the following passage:

> Assuming it admitted that if an inductive inference is to be valid, there must be some relation between α and β, or some characteristic of one of them, in virtue of which it is valid, it is clear that this relation must be between *intensions*—e.g. between "human" and "mortal", or between "ruminant" and "dividing the hoof". We seek to infer an extensional relation, but we do not know the extensions of α and β when we are dealing with empirically given classes of which new members become known from time to time (Russell 1948: 405).

So, in this later work, Russell understands the principle of induction to be essentially intensional as opposed to extensional. Moreover, therein he explicitly addresses Reichenbach's solution from this perspective and argues that Reichenbach's rule of induction is false. This argument is crucial in revealing what Russell believes is fundamental for the solution to the problem of induction *pace* Reichenbach. Russell's argument begins by characterizing the posit that grounds Riechenbach's approach to induction as follows:

> When a large number of α's have been observed, and have all been found to be β's, we should assume that very nearly all α's are β's. This assumption is necessary (so he maintains) for the definition of *probability*, for all scientific prediction (Russell 1948: 413).

But, Russell (1948: 413-14) argues that this principle can be shown to be false and that Reichenbach's view entails a problematic infinite regress.

To this end Russell introduces the following argument. Let is suppose that we have observed some number, $a_1, a_2, \ldots, a_n$, of members of a class α that also have been discovered to be members of class β. Consider also that the next observed α is a_{n+1}. If a_{n+1} is a member of β we can substitute for β a new class having all of the members of β except a_{n+1}. For this constructed class the Reichenbachean rule breaks down. Russell then says,

> This sort of argument is obviously capable of extension. It follows that if induction is to have any chance of validity, α and β must not be any classes, but classes having certain properties and relations (Russell 1948: 414).

So, according to Russell,

> The problem of induction, on the contrary, demands intensional treatment. The classes α, β that occur in inductive inference are, if it is true, given in extension so far as the observed instances $a_1, a_2, \ldots, a_n$ are concerned, but beyond that point it is essential that, as yet, both the classes are known in intension (Russell 1948: 414).

But, it is on this basis he concludes explicitly that, "Reichenbach's posit for induction is therefore both too general and *too extensional* (Russell 1948: 415 [my emphasis])." Russell's contention about the regress arises in the following manner. If we do not adopt an intensionalist metaphysics, then all we have are extensionally given frequencies. But, such probabilities and the sequences on which they are based entail an infinite hierarchy of levels of higher-order probabilities that cannot be stemmed rationally. His own view (Russell 1948: 471) is then that induction can be grounded and the regress stemmed by appeal to the following a priori principles concerning causality, natural laws and common structure:

> R1: When a number of similar structures of events exist in regions not widely separated, and are arranged about a center, there is an appreciable probability that they have been preceded by a central complex having the same structure, and they have occurred at times differing from a certain time by amounts proportional to their distance from this central structure.
>
> R2: Whenever a system of structurally similar events is found to be connected with a center in the sense that the time when each event occurs differs from a certain time by an amount proportional to the distance of the event from this center, there is an appreciable probability that all the events are connected with an event at the center by indeterminate links having spatio-temporal contiguity with one another.
>
> R3: When a number of structurally similar systems, such as atoms of this or that element, are found to be distributed in what appears to be a random manner, without reference to a center, we infer that there are probably natural laws making such structures more stable than others that are logically possible, but are found to occur rarely or never.

Whatever one might think about the specifics of these (rather obscure) principles, it should be clear that they are supposed to solve the problem of grounding induction by introducing intensions into the world via the idea of structures and natural laws connecting types of structures. More interestingly, Russell (1948: 472) tells us that, "the above three principles, if accepted, will, I think, afford a sufficient a priori basis for most of the inferences that physics bases on observation". So, in his later work we can see that Russell was still committed to the idea that a priori truths about intensions are what ground induction and that he explicitly rejected both extensional solutions and those based on pure forms of empiricism.

6. Inductive Methods and Reliability

Having examined Reichenbach's purely extensional and empiricistic conception of scientific induction and Russell's intensional and rationalistic conception of induction we can now turn out attention to the more general concept of an inductive method and to the concept of the reliability of such methods in order to tease out some important lessons from the comparison of these views. Recall that Reichenbach's justification of scientific induction essentially amounts to the claim that if any method at all works, then scientific induction will work, and so scientific induction is the best method available (i.e. it is optimal) even if it is just the best member of a bad lot. This is so because Reichenbach believes that we cannot establish whether or not the convergent relative limiting frequencies of observed sequences exist and so we cannot empirically establish that $1 \geq m(w_a) > \varepsilon$. But, the ultimate reliability of the rule of induction depends, by definition, on the existence of such convergent limits and throughout his arguments he presupposes extensional empiricism. Russell, on the other hand, introduces universals that we can come to know through the use of pure reason coupled with observation as the grounds for such order. Before proceeding with unpacking what these views jointly imply, however, it will be instructive to examine the concepts of reliability and of a reliable inductive method from the conceptual point of view before confronting the real substance of the difference between Russell's and Reichenbach's approaches to the justification of induction.[23]

The concept of an inductive method desired here is, trivially, a sub-species of the concept of a method, and, in this case, the sort of method in which we are interested is a procedural method or rule for making logical inferences from what we have observed to what we have not observed and from making inferences from what we have observed to what is always the case. What we ideally wish to have in our possession is an algorithmic method of generating these desired conclusions from the inputs which we have available to us based on observation. However, we know that by definition inductive methods are in some sense not perfectly truth-preserving, and, hence, that we must always accept the possibility that the conclusions of inductive inferences can be false when the premises are true. So, for induction to be reliable it does not need to be the case that $m(w_a) = 1$. This requires only that $m(w_a) > \varepsilon$. This is simply the recognition that inductive inference is a species of nonmonotonic inference, but, nonetheless, it does seem to be the case that we believe that, at least sometimes, it is rational to make such inferences. This is simply because induction allows that the conclusions of such inferences are probable on the assumption that we have true premises from which they follow. So, we are obliged to provide some appropriate form of justification for such procedures even though they sometimes fail to produce true outputs. What we would like to know is how much credence we should give to such outputs, and this, intuitively, ought to reflect how reliable such methods, procedures, algorithms, rules, etc. are. It will also be instructive to examine what it is that makes such inferences unreliable or prone to error, and in doing so we will find that there are really two sense of the term 'reliable' involved.

So, what do we mean by the phrase 'reliable method'?[24] Of course, borrowing a turn of phrase from Peter Lipton,[25] what we mean is that a method is 'truth-tropic'. That is to say, a method is a (perfectly) reliable method, at least in one sense, if and only if it will (at least at some point) produce the results it is supposed to produce. But, the reliability of such methods varies and it can, and should, be understood probabilistically. A reliable inductive method is one that *tends* to produce the correct results more often than it produces incorrect results in its domain of application; i.e. one that will likely allow us to arrive at the truth and avoid falsity with some likelihood that is greater than relying on chance and merely selecting an output at random from the set of all possible outputs concerning its domain of application. So, a minimally reliable method, by definition, is one that produces correct results with a degree of likelihood bet-

[23] There are, of course, other sense of reliability that have been discussed in the literature on induction, algorithms and methods, but here we are concerned only with the very general sense of reliability that is closely tied to epistemological reliabilism.
[24] Kelly 1991 and Kelly 1996 introduce far technically more sophisticated notions of reliability, but the simpler notion used here is sufficient for the purposes at hand.
[25] See Lipton 1991.

ter than chance and the sophisticated Reichenbach argument represents the reliability of the inductive principle as the greater than chance probability that $1 \geq m(w_a) > \varepsilon$. In fact, it should then be obvious that a procedure that performs at or below a mere chance level of success is really no method at all. As we have just seen, such a procedure is extensionally equivalent to having no rule at all and simply making a random selection. But, this sense of reliability, *reliability$_1$*, applies only within the *domain of application* of the method in question. In other words, in this sense of reliability, a method is a reliable$_1$ method, if and only if, it is likely that it will produce the correct results when applied to those things it is supposed (or designed) to be applied to.[26]

In this sense we might consider using a metal detector to be a reliable$_1$ way of detecting sufficiently large concentrations of metallic elements, but this sense of reliability does *not* imply, and should not be taken to imply, *that there is anything in the domain of application of that method*; i.e. that there are such metals. It is no criticism of a metal detector to say that it is unreliable$_1$ in world in which there are no metallic elements. Such a detector might still very well be a reliable$_1$ detector of concentrations of metallic elements even if there were no such things in that particular world. The metal detector would still, more or less effectively, detect large concentrations of metallic elements even if there happen not be any such things in the world where the detector exists.

In any case, borrowing some insights from epistemological reliabilists and from the work both of Hume and Descartes, we must recognize that the general reliability of a belief forming mechanism or of an inference procedure is not purely a logical or conceptual matter concerning the procedure in question. Rather, the general reliability of such procedures is, at least in part, a function of the physical (and even metaphysical) features of the environment in which that procedure in employed. Hume's essential insight was that in highly uniform environments we would be entitled to regard (straight-rule) induction as a generally reliable procedure, but that we do not know and cannot non-circularly establish that we inhabit such an environment on the basis of merely observing particulars. So, we do not know what probabilities we should assign to the conclusions of our inductive inferences in our world. Reichenbach recognized and sought to prove essentially that scientific induction is a reliable$_1$ procedure, but what he saw as the real and more troubling problem with scientific induction was that it is not reliable in the sense that we do not, and apparently cannot, know that there exists anything in the domain of application of scientific induction (i.e. whether any convergent sequences exist). So, the general reliability of a method or procedure requires that it also be reliable$_2$, and a method is *reliable$_2$,* if and only if, it is reliable$_1$ *and* there exist entities in the domain of application of that method. But, as Reichenbach saw it, in order to be non-pragmatically justified in believing that scientific induction is generally reliable we would need to show that such sequences exist in our world (or, at least that it is likely that such sequences exist in our world), and it does not seem to be the case that we can show that scientific induction is reliable$_2$ if we are committed to purely extensional empiricism. We simply do not have the ability to observe such limiting frequencies directly if extensional empiricism is true. In effect, given an extensional empiricism we cannot project any regularities that hold among observed

[26] This point is similar to those made by Goldman (1986) concerning the relationship between reliability and one's environment.

particulars to unobserved particulars because logically atomic statements that represent observations of particulars contain no information about any other such statement. But, we can vindicate induction in the pragmatic sense given such a metaphysical view and so we do not need to resort to the illicit a priori assumption of intensional logic to make it rational to apply the inductive rule. As Reichenbach tells Russell explicitly in a 1949 letter, "Induction does not require an intensional logic" (Reichenbach 1949d: 410). But, if we retain extensional empiricism then the rationality of induction does require broadening our notion of what is rational to include pragmatic justifications. Pace Reichenbach it is then clear that Russell was prepared to cede the purely extensional empiricism he adhered to early on in his work and to replace it with a more metaphysically permissive view that countenanced non-reducible intensional entities (i.e. universals) as the metaphysical basis required to assure that induction would be reliable$_2$. In doing so he was essentially endowing the world with a metaphysical structure robust enough to ensure such projectability. Moreover, he did so in such a way that the realibility$_2$ of induction is an a priori matter because he treats the existence of the universals that ground the reliability$_2$ of induction as an a priori truth of metaphysics. Russell essentially argues that this metaphysical truth guarantees that $m(w_a) > \varepsilon$.

Consider, again, our case of the metal detector. We saw that such a procedure is reliable$_1$ if it is an effective procedure for detecting sufficiently large concentrations of metallic elements; if it will detect concentrations of metallic elements at a rate better than chance. But, such a method or procedure will be reliable$_2$ if and only if, it is realiable$_1$ *and* metallic elements of the sort the detector was designed to detect exist. So, the general reliability of the metal detector depends on both of these senses of reliability being satisfied. In the case of scientific induction we saw earlier that Reichenbach proved that scientific induction is a reliable$_1$ method, but based on his staunchly held empiricist, extensionalist and verificationist leanings Reichenbach concluded, *pace* Russell, that we cannot know that scientific induction is reliable$_2$, or even that it is probable that scientific induction is reliable$_2$. But, if we want to do science on a rational basis we have to countenance a pragmatic vindication of our practices of induction or we must simply give up on science altogether. He asserts this very point about the distinction between the reliability$_1$ of induction versus its reliability$_2$ and its importance for science in terms of his own preferred analogy as follows:

> We are in the same situation as a man who wants to fish in an uncharted place of the sea. There is nobody to tell him whether or not there are fish in this place. Shall he cast his net? Well, if he wants to fish I would advise him to cast his net, at least to take the chance. It is preferable to try even in uncertainty than not to try and be certain of getting nothing (Reichenbach 1936: 157).

To this he adds the following instructive claim:

> To restate the point in terms of the illustration above: the chances of our catching fish increase with the use of a more finely meshed net; *we ought therefore to use such a net even if we do not know whether there are fish in the water or not* (Reichenbach 1936: 158 [my emphasis]).

However, it cannot be emphasized too strongly that this conclusion follows only if we do not have empirical access to real intensional structures like those Russell introduced to ground induction. Of course, as we have already seen, in explicitly eschewing such metaphysical commitments Reichenbach explains that if we want to do science *in the extensionalist and empiricist metaphysical framework* we can only adopt and deploy the inductive rule on the basis of pragmatic considerations. So, in virtue of these claims and those we canvassed earlier, Reichenbach is clear that we cannot know that convergent limiting frequencies exist if extensional empiricism is true. But, if we don't commit to using the inductive rule, then the very possibility of science is undermined. So, given what is epistemically at stake and if we commit to extensionalist empiricism, should use it on this pragmatic basis despite the epistemic limitations of our inductive practices.

What shall we make of all of this? On the one hand (and *pace* BonJour and others who have leveled the charge of obscurity at Reichenbach's solution), there is then nothing really obscure about Reichenbach's view at all. He was merely claiming that the pragmatic vindication of induction was the only option open to those who subscribe to extensional empiricism and the price he was forced to pay for subscribing to this conservative epistemic and metaphysical framework was that of simply having to accept the conclusion of BonJour's not entirely hypothetical inductive skeptic. But, again, there is no obscurity in this at all and vindication offers a way to adroitly *avoid* such skepticism, even if only by expansion of the relevant conception of rationality. On the other hand, in order to ground induction in such way as to *answer* the inductive skeptic, Russell was prepared to pay the price of ceding his basic commitment to a simple Humean extensional empiricism and to accept a metaphysically dubious and inflationary form of essentialism on an a priori basis.

Reichenbach, of course, did not see Russell's rejection of extensional empiricism as necessary at all and this is the gist of his point in the 1949 letter to Russell. As we saw, Russell (1948) had contended that Reichenbach's view entailed a problematic infinite regress of levels of probabilities and that, as a result, grounding induction required adopting an intensional metaphysics. But, near the very beginning of that letter Reichenbach claims that Russell misunderstands his view and that in even a reasonably brief meeting, "You would then see that your abandonment of empiricism is unnecessary and that you need not resort to an "extra-logical principle not based on experience" (Reichenbach 1949d: 405). Reichenbach dedicates the remainder of the letter to clarifying his view, answering Russell's worries about the regress of probability levels and ultimately adopting the view that "there are other reasons to make assertions than reasons based on belief" (Russell 1949d: 407). Specifically, the assertion of the existence of the limits of relative frequencies that grounds the inductive rule is a pragmatically grounded posit that stems the alleged regress. On this basis Reichenbach dodges Russell's worries by adopting a more inclusive methodology and retaining an extensionalist and empiricist metaphysics. So, as suggested in the introductory remarks to this paper, paying careful attention to the metaphysical views involved in both Russell's and Reichenbach's views and manifest in their brief exchange makes clear what was (and still is) at stake in this matter.

7. Induction Redux: Extensionalism, Intensionalism and Hyper-intensionalism

So, what does this brief but important historical exchange tell us about the contemporary situation with respect to the problem of justifying induction? The point made here is that this methodological and epistemological debate about induction between Russell and Reichenbach shows us, among other things, that we cannot easily disentangled methodological matters from the associated metaphysical issues. In the narrow context of this particular debate what we can learn from the Russell/Reichenbach exchange is that the sort of justification that can be given for induction depends deeply on austerity of one's metaphysics and that richer metaphysical resources yield richer methodological resources. Specifically, the commitment to purely extensional metaphysics precludes the successful epistemic justification of induction and suggests the more radical move that induction only admits of pragmatic justification, but helping one's self to more inflationary metaphysical views opens up the door to epistemological justifications of induction. However, given the centrality of induction in human reasoning and the increasingly wide-spread recognition that offering a substantial, successful and non-pragmatic justification of induction requires conceding both purely extensional and merely intensional metaphysics in favor of yet more inflationary hyper-intensional metaphysics, it is at least provisionally clear that there is something to be said for inflationary metaphysics as it applies here and in the broader discipline of philosophy.[27] In effect, all of this suggests that the severe methodological and epistemological costs associated with the most austere extensionalist metaphysical views may be simply too costly when it comes to the matter of induction and the same sort of lesson may apply to a variety of philosophical issues involving the inter-play of methodology and metaphysics.[28]

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[27] Cresswell 1975 Spohn 2005 and Ortner and Leitgeb 2009, Kelly 2014 and Nolan 2014.

[28] See, for example, Shaffer 2015 and Shaffer 2017b.

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