Reichenbach, Russell and Scientific Realism[*]

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Abstract: This paper considers how to best relate the competing accounts of scientific knowledge that Russell and Reichenbach proposed in the 1930s and 1940s. At the heart of their disagreements are two different accounts of how to best combine a theory of knowledge with scientific realism. Reichenbach argued that a broadly empiricist epistemology should be based on decisions. These decisions or "posits" informed Reichenbach's defense of induction and a corresponding conception of what knowledge required. Russell maintained that a scientific realist must abandon empiricism in favor of knowledge of some non-demonstrative principles with a non-empirical basis. After identifying the best versions of realism offered by Reichenbach and Russell, the paper concludes with a brief discussion of the limitations of these two approaches.

I. Introduction

How should we relate the accounts of scientific knowledge that Russell and Reichenbach proposed in the 1930s and 1940s? On the one hand, Russell and Reichenbach agreed on many key philosophical issues. As Russell put it in a letter to Reichenbach from Sept. 1940, "Those of our way of thinking have arrived at the possibility of considering philosophical questions as one considers questions in science, so that opinions are no longer merely an expression of personal temperament."[1] On the other hand, Russell and Reichenbach disagreed on the compatibility of empiricism and scientific realism. In his classic 1938 book *Experience and Prediction*

[*] *Synthese*, forthcoming in a special issue "All Things Reichenbach", F. Padovani & E. Curiel (eds.).

[1] Russell to Reichenbach, Sept. 6, 1940 (Bertrand Russell Archives, 53895 RA3 17K).

Reichenbach argued that the proper approach to epistemology supported a novel "probability theory" of meaning, and that this theory afforded the right kind of empiricist justification for many of our scientific beliefs. Right around this time, Russell developed his own distinctive approach to epistemology as part of a defense of scientific realism. However, for Russell, scientific realism mandated a rejection of empiricism. This argument is prominent in 1927's *Outline of Philosophy*, but its character continued to change up through Russell's 1948 book *Human Knowledge*. In this paper I show how Russell's reading of Reichenbach played a decisive role in his epistemology. Reichenbach took Russell's arguments quite seriously in the 1940s, and offered two significant responses. I argue that comparing the scientific realisms of Reichenbach and Russell allows us to see the limitations of each position, and points the way to a more viable, third strategy.

II. Historical overview

The compatibility of empiricism and scientific realism is a perennial issue for the philosophy of science. Many prominent debates in the philosophy of science turn at least in part on this issue. For example, Mill's attack on Whewell in the *System of Logic* anticipates many aspects of the later exchange between Planck and Schlick.[2] A scientific realist often seems forced to appeal to a debatable epistemic principle in order to secure knowledge of entities that are not directly experienced. A common empiricist critique is that this epistemic principle is illegitimate, and that we should recast our scientific knowledge in more experiential terms.

[2] See, e.g., Godden 2014 and Neuber 2018, ch. 2. I am grateful to an anonymous referee for emphasizing the importance of this history for this paper.

Ongoing work on Reichenbach and the Berlin Group have complicated any simple identification of Reichenbach with either traditional empiricism or the logical positivism advanced by Schlick and some other members of the Vienna Circle. As Milkov has helpfully put it, the Berlin Group should figure "as an equal partner with the Vienna Circle in promulgating, around 1930, the scientific philosophy in the German-speaking world" (Milkov 2013, 4). Milkov also emphasizes the important role that Russell played in inspiring much of the work of the Berlin Group. First, it is clear that "The Berlin Group's philosophical hero was not Wittgenstein but Bertrand Russell" (Milkov 2013, 19). Milkov here cites, among other evidence, two essays that Reichenbach published on Russell in 1928 and 1929.[3] The 1929 almanac entry suggests that Reichenbach saw his own work as the proper continuation of Russell's reformation of philosophy. Russell is singled out as "the worthiest representative of … a philosophy of the positive sciences" (Reichenbach 1978, I, 298). Reichenbach draws on Russell's 1927 *Outline of Philosophy* to praise both the style and substance of Russell's philosophical writings. While Russell argues in support of a metaphysics that countenances only particulars composed of "neutral 'stuff'," Reichenbach emphasizes "an essentially novel feature of his philosophy that is perhaps more important than the underlying positivistic thesis itself" (Reichenbach 1978, I, 301). This is that "epistemological problems are only to be solved by using mathematical-logical methods" (Reichenbach 1978, I, 301). As we will see, for Reichenbach, the proper application of these methods yields a "probability" theory of meaning, along with a novel defense of induction and the epistemology of blind posits.

[3] See Reichenbach 1967 and Reichenbach 1978, I, 298-303 for English translations.

Russell was 19 years older than Reichenbach, and had already worked out various philosophical positions by the time Reichenbach completed his dissertation in 1915. However, as Milkov puts it, "Russell … was a complex philosopher whose views were subject to radical shifts and whose thought cut across the interests of both the Vienna Circle and the Berlin Group" (Milkov 2013, 19). As we will see, one crucial shift in Russell's views on induction can be traced to his engagement with Reichenbach. For in 1944 Russell came up with what he viewed as a decisive objection to Reichenbach's defense of induction. This informed Russell's 1948 book *Human Knowledge: Its Scope and Limits*, and the character of its non-empiricist version of scientific realism. Russell's criticism of Reichenbach in this book prompted a long letter from Reichenbach in 1949. This letter was important enough for Reichenbach's literary executors M. Reichenbach and R. Cohen to include it in Reichenbach's *Selected Writings*.

Prior to this exchange, Russell and Reichenbach had a number of contacts, but by far the most important was Russell's visit to UCLA in 1939-1940 while Reichenbach was a professor there. In the preface to his 1940 book *Inquiry into Meaning and Truth* Russell notes that the manuscript for the book was the basis for seminars at the University of Chicago in 1938-1939 and at UCLA in 1939-1940. He goes on to note that "The discussions at the two seminars did much to widen my conception of the problems involved and to diminish the emphasis which I originally placed on the linguistic aspects of the subject. I have to express a collective obligation to those, both Professors and pupils, who, by detailed friendly criticism, helped (I hope) in the avoidance of errors and fallacies" (Russell 1940, 7). Only Carnap, Charles Morris and Norman Dalkey are mentioned by name here. But there is every reason to think that Russell discussed the manuscript extensively with Reichenbach during his time at UCLA. For example, in the letter

from Sept. 1940 noted earlier, Russell adds "I wish that during the year at UCLA I had had more time and leisure of mind for discussion with you, as I always found it fruitful."

In her moving memoir of Hans Reichenbach, Maria Reichenbach offers an anecdote that, if genuine, would further bolster the case for the importance of Reichenbach for Russell's philosophical development. Maria Reichenbach writes:

> In 1940 Bertrand Russell was visiting Flint Professor at UCLA and shared Hans' office. They became good friends and Hans took excellent photographs of Russell. He also tried to use photography for the purpose of convincing Russell to give up his two-dimensional, phenomenalist descriptions of the world and of converting him to a realist and physicalist description. Hans took stereo-photographs of various things and then let Russell look at them through a viewer, waiting expectantly for the 'aha moment', when the two pictures would merge into a three-dimensional one and Russell's facial expression would indicate this fact. In spite of these attempts, I think Hans was not very successful in changing Russell's mind (Reichenbach 1978, I, 79).[4]

It is hard to know what to make of this anecdote, though, as by this time Russell did not accept "two-dimensional, phenomenalist descriptions of the world." Since at least 1927 Russell had explicitly endorsed a metaphysics of the world composed of events, including events that are not "percepts". One could speculate that the debate concerned the contents of our perceptions: Russell maintained that our perceptions were ultimately of such events standing in

[4] See also Dalkey's description of H. Reichenbach's "stereo pictures" and the summer of 1940 spent with Russell's family, with visits by H. Reichenbach (Reichenbach 1978, I, 50).

relations, while Reichenbach argued that our perceptions are best interpreted as involving ordinary objects like chairs and tables. On this point, at least, Russell did not change his mind.[5]

Milkov appears to have this anecdote in mind when he proposes another interpretation of how Reichenbach influenced Russell in 1940. Milkov contrasts external and internal philosophy of science. An external approach emphasizes questions about logic and language that are not explicitly discussed by scientists, while an internal approach is more focused on scientific practice, e.g. how knowledge is acquired. Russell of course considered both kinds of questions, with the 1940 book emphasizing semantic issues and the 1948 book considering what scientific practice tells us about the world and our knowledge. Milkov says that "Likely enough it was Reichenbach who awoke Russell from his philosophy-of-language slumbers, for the two shared an office when Russell was at the University of California at [Los Angeles] in 1940" (Milkov 2013, 28). This is a puzzling proposal, and Milkov does not provide any additional discussion to develop it. Prior to 1940, in works such as *Analysis of Matter* and *Outline of Philosophy*, Russell had considered issues that are essentially the same as what he discussed in the later *Human Knowledge* book. In addition, as we will see, Russell's discussion of Reichenbach in the 1948 book are quite negative. So even though Russell took Reichenbach very seriously and formed his own views through a sympathetic engagement with Reichenbach, there is no evidence that Russell changed his philosophical interests in 1940. Still, Milkov's proposal should prompt a more detailed examination of their philosophical relationship.

[5] Cf. Schott's recollection that Reichenbach "used such images to demonstrate that visual perception is not just a matter of retinal images" (Reichenbach 1978, I, 53).

Another recent discussion of Russell and Reichenbach by Shaffer essentially proposes that Russell and Reichenbach remained wedded to their basic assumptions throughout their interactions, and so neither had any significant influence on the other. As Shaffer puts his position, "the comparison of Reichenbach's and Russell's solutions [to the problem of induction] calls attention to the opposition between extensional and intensional *metaphysical* presuppositions in the context of their particular attempts to solve the problem of induction" (Shaffer 2019, 164).[6] Reichenbach is a metaphysical extensionalist because he does not appeal to properties or laws connecting properties in his approach to induction. Russell, by contrast, does invoke properties and laws throughout his discussion of induction. Shaffer helpfully discusses the objection that Russell makes to Reichenbach in the 1948 book *Human Knowledge*. We will return to this in more detail below, but Shaffer is right to note that Russell concludes from this objection that "The problem of induction … demands intensional treatment" (Russell 1948, 414, given at Shaffer 2019, 173). In his 1949 letter in reply to Russell, Reichenbach insists that "Induction does not require an intensional logic" (Reichenbach 1973, I, 410, given at Shaffer 2019, 177). So there is good textual evidence that the disagreement about induction can be traced to a disagreement about metaphysics.

My main disagreement with Shaffer's reconstruction is that Shaffer assumes that Russell's attitude towards induction is unchanged from the 1912 book *Problems of Philosophy* through the 1948 book *Human Knowledge*. In 1912 Russell endorsed an unrestricted principle of induction that was known through our acquaintance with universals. According to Shaffer, this justification of induction clashed with the "austere metaphysical atomism and empiricism"

[6] I am grateful to an anonymous referee for drawing this paper to my attention. See also Shaffer 2017.

that Russell endorsed in 1918. However, the 1948 treatment is said to be essentially the same as the 1912 proposal: "The more robust and sophisticated approach to the problem of induction that was suggested by Russell in his 1912 is developed … more fully and extended in his 1948 book *Human Knowledge*" (Shaffer 2019, 172). By contrast, I will argue that Russell abandoned an unrestricted principle of induction in 1944 through his examination of Reichenbach's work. After this change, Russell did not argue that any principles related to scientific knowledge were known through acquaintance with universals. Instead, he opted for a new sort of externalist justification that Russell supposed was incompatible with empiricism. So, I will argue that Shaffer has failed to appreciate the significant change in Russell's views or their connection to Reichenbach.

III. Realism through posits

Reichenbach begins *Experience and Prediction* with the influential contrast between the context of discovery and context of justification. The context of discovery relates to psychological aspects of how a scientist formulated a claim, while the context of justification considers the possibility of a rational reconstruction of an argument for that claim (Reichenbach 1938, 6-7). Reichenbach goes on to argue that the rational reconstruction of any scientific claim will reveal a series of "volitional decisions" (Reichenbach 1938, 9). These decisions are to be critically evaluated in terms of the aims of science, which are in turn conventionally adopted. In fact, Reichenbach argues, one aim for science that enjoys "almost universal assent" is that "science is to furnish methods for foreseeing the future as well as possible" (Reichenbach 1938, 15). If this aim is maintained, epistemology can identify a number of "entailed decisions," where the entailment is said to be broadly logical in nature (Reichenbach 1938, 13-14). So once

values are settled, broadly logical considerations tell in favor of some beliefs over others. This is how Reichenbach's epistemology avoids collapsing into conventionalism.

The first kind of entailed decision that Reichenbach examines is the proper conception of meaning that flows from the aim of predicting the future. Reichenbach argues that any approach to meaning in terms of truth is inconsistent with the choice to aim at the prediction of the future. This argument focuses on the contrast between directly and indirectly verifiable propositions as it had been developed by verificationists. One flavor of verificationism considers observations that are physically possible, while another flavor allows a broader class of logically possible observations. However direct verification is clarified, the remaining indirectly verifiable propositions are assigned a meaning based on their reducibility to some directly verifiable propositions. These approaches all tie meaning to truth because they suppose that verification consists in establishing the truth of some proposition. Reichenbach then argues that there is no way to associate the indirect propositions that are actually used in science with some finite basis of direct propositions. In general, "the inference from premises to the indirect sentence is not a tautological transformation but a probability inference" (Reichenbach 1938, 53). Once the logical character of these inferences is revealed, the choice is clear: "either to renounce indirect sentences and consider them as meaningless or to renounce absolute verifiability as the criterion of meaning" (Reichenbach 1938, 53). "Absolute verifiability" here is the assumption that every meaningful proposition is true or false.

Once a truth-based theory of meaning is given up, the appropriate alternative is to adopt a probability theory of meaning. Reichenbach implements this using his notion of "weight", which is a number attached to a proposition: "a proposition has meaning if it is

possible to determine a weight, i.e. a degree of probability, for the proposition" and "two sentences have the same meaning if they obtain the same weight, or degree of probability, by every possible observation" (Reichenbach 1938, 54). The need for a probability theory of meaning to make sense of prediction is famously illustrated in sections 13-14 with Reichenbach's cubical world. In an allegory worthy of Plato, Reichenbach imagines "the whole of mankind … imprisoned in a cube, the walls of which are made of sheets of white cloth, translucent as the screen of a cinema but not permeable by direct light rays" (Reichenbach 1938, 115-116). Outside this cube, there are two light sources. One points directly down, while another points in the horizontal direction from right to left. The light pointed in the horizontal direction is reflected by two mirrors so that the left side of the cube is illuminated (Reichenbach 1938, 117). Scientists then observe what we are told are in fact birds as they fly around in the space above this cube. The light from above creates shadows of the birds on the top of the cube, while the light from the right creates, via the mirrors, shadows on the left side of the cube. Reichenbach imagines a "Copernicus" (Reichenbach 1938, 116) who notices the correlations between the positions of the shadows on the two sides of the cube, and proposes "that these two shades are nothing but effects caused by one individual thing situated outside the cube within free space" (Reichenbach 1938, 118). Such a theory affords predictions about the future.

If we suppose that humanity is confined to the cube, then there are no possible observations that would establish the truth of the birds theory as opposed to a more restricted theory that talked only of the shadows and their movements. However, "Judged from the facts observed the hypothesis appears highly probable" (Reichenbach 1938, 120). This is because "It

seems highly improbable that the strange coincidences observed for one pair of dots are an effect of pure chance" (Reichenbach 1938, 120). More generally, once the probability theory of meaning has been adopted, it becomes possible for claims concerning unobservable entities to accrue a high probability. Here, in a nutshell, is Reichenbach's argument for scientific realism. While the point is first introduced for the imagined cube world, the argument is extended later to the unobservable entities of the scientific realist. In section 25, Reichenbach introduces "illata" or "inferred things" as a label for entities that are neither observable nor abstract. Epistemically, "The relation of the illata to the concreta is a projection in the sense indicated in section 13" (Reichenbach 1938, 212). A projection relation obtains whenever some class A of entities is connected to another class B of entities in a way that permits the systematic probabilistic coordination of propositions concerning A and B, but where no reduction is available (Reichenbach 1938, 110). So, just as the birds are a projection from the shadows, so too are atoms a projection from observable things. In both cases, the appropriate standing probability relations, along with the right observations, confer a high probability on the proposition that such illata exist.

A scientist should believe an atomic theory and use it to formulate predictions concerning the future just in case the other propositions that she believes confer a high probability on the propositions of that atomic theory. The key for Reichenbach is to figure out how these probabilities or weights are best assigned. In the last chapter of *Experience and Prediction* Reichenbach resolves this puzzle with his well-known justification of induction. For someone who has adopted the aim of prediction, the best means of achieving this aim is to adopt what Reichenbach calls the principle of induction. This principle takes as input an

observed relative frequency for some series of events. If each event in the series of n events has property A or ~A, then h^n is the ratio of A-events to (A + ~A)-events. Reichenbach's principle then says that "For any future prolongation … the relative frequency will remain within a small interval around h^n" (Reichenbach 1938, 340). Adopting this principle is justified because the aim of prediction will succeed, if it can succeed at all, by use of this principle. Reichenbach here considers two possible situations. Suppose first that the series of events of interest has a limiting relative frequency. In that case the use of Reichenbach's principle, when combined with ongoing observations of members of the series, will guarantee an eventual match between the scientist's h^n and the actual limiting relative frequency. But if there is no limiting relative frequency to this series, then there is no way for the scientist to fulfill their aim of prediction. So, while there is no a priori guarantee that prediction will succeed with this principle, it is the "best wager" (Reichenbach 1938, 352) given the situation.

For any new domain of investigation, the scientist will initially be unable to assign a high probability to any proposition concerning future events. A small number of observations opens up the possibility of applying the principle of induction to this new domain. The choice to make this application is what Reichenbach calls a "blind posit" (Reichenbach 1938, 353). In making a blind posit, we make a wager concerning what some future events will look like. There is no alternative to making blind posits for Reichenbach. As science matures, some initially blind posits can be stabilized to the point that they become "appraised posits" (Reichenbach 1938, 352). But this refinement itself turns on new blind posits. So at the root of our system of scientific knowledge, Reichenbach sees the need for ongoing decisions. These decisions are

justified as the best means to achieve our aims, most notably the aim of "foreseeing the future as well as possible" (Reichenbach 1938, 15).

One illustration that Reichenbach provides for the indispensability of blind posits involves what he calls cross-induction. Suppose that we expose each available substance to increasingly high temperatures. For copper we find a series of observations ~M, ~M, M, M, … where for our first two temperatures, the copper did not melt, but for every higher temperature, the copper did melt. Other temperature series are then obtained for other substances such as iron, e.g., ~M, ~M, ~M, M, M, … At some point we encounter a substance like carbon that resists our attempts to melt it: the observed temperature series for carbon is ~M, ~M, ~M, … (Reichenbach 1938, 365). Here Reichenbach recognizes a potential puzzle for his principle of induction. If we apply the principle to a new sample of copper, the observed temperature series supports the conclusion that there is a high probability that copper will melt at some sufficiently high temperature. A similar conclusion follows for iron and all the other substances we have observed, except for carbon. In the case of carbon, the principle of induction leads to a high probability that a new sample of carbon will *not* melt at any temperature. The puzzle arises when we notice that the propositions at issue here seem to support a further "second order" induction: for nearly all observed substances, that substance will melt at a sufficiently high temperature. The principle of induction thus implies a high probability for the claim that a new sample of carbon *will* melt at a sufficiently high temperature. How can we reconcile these conflicting applications of the principle of induction?

Reichenbach's solution to this puzzle is to invoke blind posits of a higher order. In fact, scientists suppose that every substance will melt at a sufficiently high temperature. This reflects

the choice to resolve the conflict between the first order inductions and the second order induction in favor of the second order induction. In doing so, the first order induction involving carbon is corrected, and transformed from an initially blind posit, into a holistically appraised posit. More generally, "Every blind posit may be transformed into a posit with appraised weight, but the transformation introduces new blind posits" (Reichenbach 1938, 367). This is why "the system of knowledge, as a whole, is a blind posit" (Reichenbach 1938, 401). Ultimately, the defense of scientific knowledge requires dropping "one deep-rooted prejudice: … that the system of knowledge is to be a system of true propositions" (Reichenbach 1938, 404). Our knowledge instead concerns only "the road toward our best wagers" (Reichenbach 1938, 404).[7]

IV. Truth and probability

As noted in section II, Reichenbach took Russell's work very seriously, both before he wrote *Experience and Prediction* and after. By 1930 Russell had staked out an approach to induction and scientific knowledge that is quite different from Reichenbach's approach. The basic argument is given in 1927's *Outline of Philosophy*. The chapter "The Validity of Inference" first argues that "the whole structure of science, as well as the world of common sense, demands the use of induction and analogy if it is to be believed" (Russell 1927, 215). This is because science and common sense involve beliefs that go far beyond the events that we experience. This practice requires something like what Russell calls the principle of induction: if there are two kinds of events, A and B, and if many known instances of A have been "quickly

[7] See Sober 2011, Psillos 2011a, Neuber 2018, ch. 4 and their references for other discussions of Reichenbach's realism and his approach to induction.

followed" by known instances of B, with no exceptions, then "a sufficient number of instances of this sequence, or instances of suitable kinds, will make it increasingly probable that A is always followed by B, and in time the probability can be made to approach certainty without limit provided the right kind and number of instances can be found" (Russell 1927, 216). Knowledge in science and common sense, then, is said to rest on knowledge of this principle of induction.

Russell goes on to consider the "logical" approach to induction championed by Keynes. After noting a refinement by Nicod and a review of Nicod by Braithwaite, Russell says "A man who reads these three will know most of what is known about induction" (Russell 1927, 217). However, neither Keynes' proposal nor Nicod's refinement is accepted by Russell. Keynes says that if a "postulate of limited variety" holds, then one can establish the principle of induction. The postulate requires that each observed thing is only finitely complex. As a result, repeated observations can over time increase the probability of a pattern obtaining quite generally. Russell endorses Nicod's objection that this postulate is too weak. What is required instead is that "there is a finite number n such that there is a finite probability that the number of independent qualities of our object is less than n" (Russell 1927, 220). However, for this postulate to support the principle of induction, the postulate must have some a priori justification. Russell offers no analysis of where this justification could come from.

Russell ends his brief discussion by contrasting Keynes' view that probability is a "fundamental logical category" with the frequentist approach to probability (Russell 1927, 220-221). Russell expresses some hopes for frequentism as it "would bring probability into much closer touch with what actually occurs" (Russell 1927, 221). Unfortunately, "the difficulties of

the frequency theory are considerable" (Russell 1927, 221). Russell concludes, without argument, that "on either view the principle of limitation of variety will be equally necessary to give validity to the inferences by induction and analogy upon which science and daily life depend" (Russell 1927, 221).

In his published writings up until 1948, Russell insisted that his non-empirical principle of induction was known, but that the source of this knowledge was not known. He suggested two potential sources, though. One source was the prima facie justification possessed by the beliefs that we have when we start philosophizing. As Russell puts it in *Outline of Philosophy*,

> In actual fact, we start by feeling certainty about all sorts of things, and we surrender this feeling only where some definite argument has convinced us that it is liable to lead to error. When we find any class of primitive certainties which never leads to error, we retain our convictions in regard to this class (Russell 1927, 133).

The principle of induction or an associated postulate of limited variety could then be one of these primitive certainties or a means of systematizing other, primitive certainties. If this basis passed the tests that Russell has in mind, then it could be said to be justified. This notion of justification makes frequent appearances in Russell's discussions of difficult principles prior to 1927. For example, Russell famously considered a "regressive" justification of problematic logical axioms that emphasized their capacity to systematize other logical and mathematical beliefs (Patton 2017). One ongoing worry for this kind of proposal is that it is not clear how what we do in "actual fact" translates into a justification.

Another potential source that Russell considered for justifying his principle of induction was the universals that we become aware of through experience. This is the strategy suggested

by the 1936 paper “The Limits of Empiricism”. The most important part of this article for our purposes concerns Russell’s discussion of how we are to make sense of the verbal expression of knowledge that arises directly from sensory experience. For example, Russell says “there is a cat”. What must happen for this utterance to accurately reflect his most basic sensory knowledge? Russell identifies three elements: “(1) a sensible fact expressed, perhaps inaccurately, by the words “there is a cat”; (2) that I say “there is a cat”; (3) that I say “there is a cat” because a cat (or a sensible appearance resembling that of a cat) is there” (Russell 1996, 317). Russell objects that the empiricist cannot make sense of the third element: “the word “because” seems to take me beyond what an empiricist ought to know” (Russell 1996, 317). A non-empiricist can insist that “I can perceive *some* relation having an intimate connection with that of cause and effect” (Russell 1996, 318). Once this relation is grasped through perception, this grasp can afford knowledge of some general propositions involving that relation. More generally, once we consider how words acquire their meaning, we see that grasping the meanings of some words affords non-empirical knowledge of the character of the world.

Russell seems dissatisfied with both options for justifying his principle of induction. In the *Inquiry into Meaning and Truth* (1940) he rehearses parts of his argument against empiricism, but does not even hint at a positive solution for the non-empiricist. The chapter on “Truth and Verification” sets out the dilemma very clearly. Many ordinary and scientific inferences involve assuming that when events of type A are followed by events of type B, there are intermediate events that connect them. This continuity principle involves events that are not actually experienced. More generally, there is a need to supplement what we experience with “some non-demonstrative form of inference” (Russell 1940, 380). Russell here rejects the

proposal that the harmony of the resulting system is sufficient for its justification, as he claims that a more restrictive empiricist system could be just as harmonious (Russell 1940, 382). However, nobody actually adopts this kind of restrictive empiricism. So, "if we are to retain beliefs that we all regard as valid, we must allow principles of inference which are neither demonstrative nor derivable from experience" (Russell 1940, 383). As Russell puts the point even more simply in his 1944 reply to Reichenbach, "I do not see any way out of a dogmatic assertion that we know the inductive principle, or some equivalent; the only alternative is to throw over almost everything that is regarded as knowledge by science and common sense" (Schilpp 1944, 683; Russell 1997, 20).

Given this basic disagreement between Reichenbach and Russell on induction, it is worth noting that Russell took a surprisingly conciliatory attitude towards Reichenbach in *Inquiry into Meaning and Truth*. Russell begins his chapter on "Warranted Assertibility" by saying that "I shall … not controvert Professor Reichenbach's views, since I believe that, by a small modification, they can be rendered consistent with my own" (Russell 1940, 400-401). Russell is willing to admit that we are never certain of any proposition that we express in words. This was a core consideration that Reichenbach offered in favor of his probability theory of meaning. But Russell briefly replies that some notion of truth is required to make sense of probability: "It seems to me that " 'p' is probable" is strictly equivalent to " 'p is true' is probable," and that when we say " 'p' is probable," we need some probability that this statement is *true*" (Russell 1940, 400). Elsewhere in the book Russell argues for the law of excluded middle, which Russell takes to entail that every meaningful proposition is either true or false (Russell 1940, 382). It seems, then, that Russell is suggesting that Reichenbach could

simply retain a notion of truth alongside his probabilities. Then, in addition to a weight, each meaningful proposition could be assigned a truth value. This truth value would often be unknowable, but for Russell this is a price we must pay to make sense of our knowledge.

Reichenbach's contribution to Russell's Schilpp volume, "Bertrand Russell's Logic", represents his most thorough discussion of Russell's epistemology. Reichenbach begins by noting that "he agrees very much with most of the fundamental views of Bertrand Russell" (Schilpp 1944, 23). Most of the article concerns logic and mathematics, where it does seem that Russell and Reichenbach tend to agree. However, one part of the article concerns "the need for inductive methods" (Schilpp 1944, 47) where several basic disagreements are clear.

As we have already seen, Reichenbach aims to replace a truth theory of meaning with a probability theory of meaning, while Russell is content to supplement a truth theory of meaning with a second notion of probability. Reichenbach here targets two issues: Russell's general argument that truth is indispensable, and Russell's treatment of basic statements in *Inquiry into Meaning and Truth*. Reichenbach first considers Russell's defense of "tertium non datur" or the law of excluded middle. Reichenbach argues that such a logical law is merely conventional, and that an alternative convention allows three or more values, as with his own preferred probability theory of meaning. For Reichenbach, "The notion of truth used in actual knowledge is so defined that it is related to what actually can be done. We have methods to find out the truth, and if no such methods existed it would be no use to speak of true propositions" (Schilpp 1944, 42). Reichenbach goes on to note that Russell insists on the law of excluded middle even when there are no conceivable methods available for attaching truth or falsity. In this case "I do not see what this principle can mean other than a convention" (Schilpp 1944, 44). Furthermore,

it is a convention that serves no useful purpose in helping us to realize our aims, e.g. in prediction.

Reichenbach goes on to argue that Russell's preoccupation with truth led him astray when it comes to the most basic statements of knowledge. Russell focuses his attention on the use of statements to express the knowledge provided by experience, as with the above case of "there is a cat." While Reichenbach agrees with Russell on the need for an observational basis, he is puzzled about why Russell opts for experiences as the basis rather than ordinary concrete things. Perhaps Russell's "attempt to reduce the content of immediate observations to sense-data springs from the desire to find a basis of knowledge which is absolutely certain" (Schilpp 1944, 51). For Reichenbach, there is no such basis as the system of knowledge connects statements about observations to one another using probability relations. But "if by the use of inductive methods basic statements will lead to a prediction of future observations, then observations conversely will also make the original statements more or less certain" (Schilpp 1944, 51). There is then no way to single out a class of statements that acquire a special sort of certainty through their relationship to an individual's experiences.[8]

In his brief reply Russell repeats his claim that the law of excluded middle and the class of "unverifiable truths" "is necessary for the interpretation of beliefs which none of us, if we are sincere, are prepared to abandon" (Schilpp 1944, 682). Russell then turns to Reichenbach's solution to the problem of induction through the treatment of the system of knowledge in terms of posits. He claims that "I do not see what difference is made by regarding knowledge as a "tool;" if it is to be a good "tool for predicting the future," the future must be such as it

[8] This is perhaps a continuation of the debate about perception from 1940 that was noted in section II.

predicts. If not, it is no better than astrology" (Schilpp 1944, 683). Russell then adds the "dogmatic assertation" that we have quoted above: we do know the inductive principle or some equivalent. We must insist on this knowledge, or else abandon most of our beliefs. Fundamentally, then, Russell and Reichenbach treated knowledge very differently. Reichenbach allowed for knowledge in the case of blind posits as long as those posits could be shown to be the best available. Russell maintained instead that knowledge required a justified belief in the truth, and that justification must bear on this truth.

V. Induction and knowledge

Although Russell's reply to Reichenbach suggested that Russell was unmoved by Reichenbach's worries, Russell did conclude his reply by admitting the need to identify "some principle of which induction can be justified" (Schilpp 1944, 684). Soon after writing his replies to the Schilpp volume, Russell changed his approach to induction in a dramatic way. This shift is recorded in an unpublished manuscript "Non-Deductive Inference", which Russell's editors tentatively date to 1945. This change in Russell's approach to induction is central to his 1948 book *Human Knowledge: Its Scope and Limits*. Armed with this new insight, Russell found a new objection to deploy against any purported defense of a principle of induction, including Reichenbach's pragmatic defense.

One reconstruction of Russell's route to his objection is that he first sought to apply a frequentist approach to probability, in line with his stated hope in *Outline of Philosophy*. The objection then is that a frequentist interpretation of the probability of the inductive principle winds up assigning it a very low probability. This result is inescapable if we determine the probability of a general claim by looking at the instances of the claim, and if we propose an

unrestricted inductive principle. Recall that in *Outline*, Russell had defended the principle that when observed As are followed by observed Bs in enough cases, it will be highly probable that the next A will also be followed by a B. In the 1945 manuscript, "Non-Deductive Inference" Russell first summarizes the views of Keynes, Nicod and Reichenbach concerning a principle of induction. In the next paragraph, Russell adds "the principle of induction by simple enumeration … can … be shown to be false" (Russell 1997, 122). Russell goes on to summarize his objection to an unrestricted principle of induction:

> In an inductive inference, we are given that two classes, A and B, have n members in common, and we are not given that they have any members not in common. Let a_1, a_2, … a_n be the n members of A that have been found to be members of B, and let a_{n+1} be the next member of A that we encounter. Then so far as the data are concerned, B may be any class of which a_1, a_2, … a_n are members. It is clear that unless n exceeds half the things in the universe, most of the classes of which a_1, a_2, … a_n are members will not contain a_{n+1}. Unless, therefore, the classes A and B are subject to some limitation, it is unlikely that a_{n+1} will be a member of B (Russell 1997, 122).

The argument applies directly to the principle of induction that Russell had defended in *Outline of Philosophy*. For suppose that events a_1, a_2, … a_n of type A have been observed to be quickly followed by other events of b_1, b_2, … b_n of type B. Then it should be highly probable that event a_{n+1} of type A will be followed by an event of type B. But B can be artificially defined to guarantee that event a_{n+1} of type A will not be followed by an event of type B. For example, B could be defined as the class consisting of just the events b_1, b_2, … b_n. More generally, for any common feature of b_1, b_2, … b_n, there is a definition of a class B that an unrestricted principle of

induction would apply to. But we can know in advance that many of the resulting predictions will be false. If we adopt the frequentist interpretation of probability, and consider all the potential instances of the use of such a principle, then the principle itself comes out with a very low probability: most of the available B classes will fail to conform to the principle as they will not include events that follow members of A.

In retrospect, Russell's worries strike us as very similar to Goodman's new riddle of induction. It seems that Russell and Goodman developed these worries independently of one another. Goodman dates his worries to the summer of 1944 (Goodman 1983, xvii). In a 1959 note, Russell also dates his insight to 1944: "induction used without common sense leads more often to false conclusions than to true ones" (Russell 1997, 138). In Goodman's case, he first directed his concerns at Carnap's and Hempel's "logical" approaches to confirmation (Goodman 1946), while Russell's target is Reichenbach's frequentism and the pragmatic justification of induction.

Russell's argument against an unrestricted principle of induction is developed in *Human Knowledge* (Russell 1948, 404) and quickly used to undermine Reichenbach. Reichenbach had offered his principle as the best available posit for the purposes of prediction. But Reichenbach's "posit can be shown to be false":

> Suppose a_1, a_2, … a_n are members of α which have been observed and have been found to belong to a certain class β. Suppose that a_{n+1} is the next α to be observed. If it is a β, substitute for β the class consisting of β without a_{n+1}. For this class the induction breaks down. 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 be not *any* classes, but classes having certain properties or relations (Russell 1948, 413-414).

Russell concluded on the basis of this argument that our scientific knowledge does not rest on some general principle of induction. Instead, it must arise from a small number of postulates that make reference to specific properties and relations. This in turn forced Russell to finally consider how such substantial postulates could be known.

Much of the early chapters of *Human Knowledge* reviews arguments that Russell had been developing since the 1920s and that were central to 1940's *Inquiry into Meaning and Truth*: Russell uses the results of physics and psychology to support a neutral monist metaphysics of events, some of which we are familiar with in sensory experience. Russell's theory of linguistic meaning continues to be developed in terms of a correspondence theory of truth, where an unrestricted law of excluded middle goes along with a commitment to the truth or falsity of claims that cannot be verified or refuted.

The main innovation of *Human Knowledge* is Russell's examination of probability. As we have seen, Russell was a long-standing follower of Keynes on the nature of probability, but at times also expressed enthusiasm for frequentist interpretations. Part five of the book marks Russell's most extended discussion of these issues. In explicit opposition to Reichenbach, Russell argues that two notions of probability are needed to make sense of scientific knowledge. The first notion is a finitist frequentist interpretation. For example, Russell takes "the chance of heads is a half" (Russell 1948, 370) to involve a tacit reference to a class of coin tosses. An admissible class of tosses will have a finite number of actual tosses, and the chance will be fixed by the ratio between the number of heads and the total number of tosses.

Russell maintains that the best version of the frequentist view of chance is finitist: all chances will involve finite classes of events. Russell interprets Reichenbach to be offering something else based on Reichenbach's use of limiting relative frequencies. As Russell puts it, according to Reichenbach, to say that the chance of heads is half is the "prophecy" that "if I continue long enough, the proportion of heads will come, in time, to be permanently very near ½; in fact, it will come to differ from ½ by less than any fraction however small" (Russell 1948, 370). Russell takes this be a kind of unknowable prediction concerning the future. This is why, even though Russell agrees with Reichenbach on how to evaluate claims about chances, Russell notes that during his discussion he is "consistently disagreeing [with Reichenbach] as to the *definition* of probability" (Russell 1948, 372). Reichenbach's treatment of chances in terms of limiting frequencies makes such claims "hypothetical and forever unascertainable" (Russell 1948, 372). By contrast, finite frequencies are testable and knowable.

A second major dimension of disagreement is Russell's argument for a "certainty logic" that is more fundamental than a "probability logic" of frequencies (Russell 1948, 372). Here Russell defends a notion of credibility or rational degree of belief that is a kind of successor to Keynes' logical notion of probability: "Perfect rationality consists not in believing what is true but in attaching to every proposition a degree of belief corresponding to its degree of credibility" (Russell 1948, 397). So credibility is a normative standard against which our actual degrees of belief can fall short. In many cases, Russell allows that "in the typical cases of mathematical probability, it is equal to degree of credibility" (Russell 1948, 385). For example, the statement that the next card drawn from a well-shuffled deck will be the ace of spades is assigned a mathematical probability of 1/52 using standard frequentist calculations. And the

degree of credibility in this statement is also 1/52. But for Russell the key link between frequency and credibility is a background assumption: “we have to know, or assume, that each case is equally credible” (Russell 1948, 385).

Russell thus argues that the use of frequencies to assess the rationality of degrees of belief depends on a more basic assignment of rational degrees of belief. This regress bottoms out in what Russell calls “data” and their credibility: “I define a “datum” as a proposition which has some degree of rational credibility on its own account, independently of any argument derived from other propositions” (Russell 1948, 392). Many data pertain to the contents of an individual’s experience. Such data are not certain, but begin with a high credibility (Russell 1948, 395).

Russell’s argument for rational degrees of belief informs his treatment of induction. Human knowledge requires an appropriate kind of responsiveness to what is initially credible. That is, human knowledge requires an appropriate holistic integration of data. However, Russell now finally admits that this kind of integration is not sufficient for genuine knowledge. An agent can do their best to weigh what is initially credible, and systematize it in line with these rational degrees of belief, and still wind up without knowledge. For such a rational agent to acquire genuine knowledge, they must carry out their reasoning in the right kind of environment. That is, in *Human Knowledge*, Russell proposes to justify some restricted body of non-demonstrative inferences based on an externalist notion of justification.

Russell prepares the reader for this revised conception of knowledge in a number of ways throughout the book. Russell’s preface, for example, warns that words like “belief” and “knowledge” are “vague and imprecise” and so “it is inevitable that everything said in the

earlier stages of our inquiry should be unsatisfactory from the point of view that we hope to arrive at in the end" (Russell 1948, v). Russell also places great emphasis on how we judge an animal to know: ""Knowledge", in particular, must not be defined in a manner which assumes an impassable gulf between ourselves and our ancestors who had not the advantage of language" (Russell 1948, 421).

Based in part on his argument against an unrestricted principle of induction, Russell uses holistic considerations based on credibility to isolate five postulates. These postulates employ a restricted range of predicates tied to events, their spatio-temporal relations, their causal connections and some of their experiential qualities. For example, "Given any event A, it happens very frequently that, at any neighboring time, there is at some neighboring place an event very similar to A" (Russell 1948, 488). These postulates are chosen based on their capacity to rationalize the inferences that humans tend to make as they fill out their beliefs about the unobserved past and future. In addition, Russell uses his analysis of what he takes to be our best psychology and physics to zero in on the events at the basis of his metaphysics.

The last chapter of Russell's book, called "The Limits of Empiricism", makes clear that this kind of internal reasoning will only generate knowledge if humans are in the right environment. Worries about an unrestricted principle of induction are again central: "we most certainly do need *some* universal proposition or propositions, whether the five canons suggested in an earlier chapter or something different. And whatever these principles of inference may be, they certainly cannot be logically deduced from facts of experience. Either, therefore, we know something independently of experience, or science is moonshine" (Russell 1948, 505). So the anti-empiricism of the 1936 paper "The Limits of Empiricism" remains. But

the suggested solution is now quite different. Where Russell had earlier suggested some kind of knowledge of general principles through an acquaintance with relations found in experience, the non-empirical source of knowledge is now wholly external to experience:

> Owing to the world being such as it is, certain occurrences are sometimes, in fact, evidence for certain others; and owing to animals being adapted to their environment, occurrences which are, in fact, evidence of others tend to arouse expectation of those others. By reflecting on this process and refining it, we arrive at the canons of inductive inference. These canons are valid if the world has certain characteristics which we all believe it to have (Russell 1948, 496, given by Grayling 2003, 471).

To put the position vividly, and in line with more recent discussions of externalism, two agents with internally identical mental states could carry out their reflections and arrive at the very same canons of inductive inference in the best possible way, using their data, or what seems credible to them. But if the first agent is embedded in an environment that accords with these canons, and the second agent is not, then only the first agent will genuinely know. For some time, Russell had been committed to the meaningfulness of unverifiable truths. Now he embraced the conclusion that "S knows that p" is not something that S is in a position to conclusively verify. S might reason perfectly rationally, and assign a high rational degree of belief to their claims to know, and yet lack knowledge.[9]

In 1949 Reichenbach wrote a long letter to Russell where he attempted to resolve their differences on probability and knowledge. Reichenbach notes that he has been studying *Human*

[9] See Johnsen 1979, Grayling 2003, Stevens 2011 and their references for other accounts of Russell's epistemology in *Human Knowledge*.

Knowledge with his students in a graduate seminar, and that Russell's "criticism is always to the point and witty" (Reichenbach 1978, II, 405).[10] However, "your abandonment of empiricism is unnecessary" (Reichenbach 1978, II, 405) and based on a misunderstanding of Reichenbach's views. The key misunderstanding is tied to the justification of Reichenbach's blind posits. As we have seen, Reichenbach admits that every probability assignment is based ultimately on a decision. For Russell, by contrast, each probability assignment is tied to a prior distribution of credibility, which is linked to an agent's rational degrees of belief. Reichenbach identifies "the decisive point in which you misunderstand my theory of induction" to be that "there are other reasons to make assertions than reasons based on belief" (Reichenbach 1978, II, 406-407). In fact, "blind posits are justified as a means to an end, and that no kind of belief in their truth is required" (Reichenbach 1978, II, 407). More aggressively, Reichenbach rejects Russell's notion of "credibility" as a holdover from the discredited philosophies of the past: "The idea that there is such a thing as a 'rational belief' is the root of all evil in the theory of knowledge and is nothing but a remnant from rationalistic philosophies" (Reichenbach 1978, II, 407). We must make the leap to recognize the indispensable role of decisions in all systems of genuine knowledge. Once this shift is made, there is no problem with admitting that our knowledge rests on decisions with a purely pragmatic justification.

Reichenbach's letter also contains a spirited defense of his principle of induction. This defense deploys a contrast between primitive and advanced knowledge. When we begin our investigations of nature, we must deploy various blind posits to assign probabilities based on very limited samples. However, as our investigations develop, it becomes possible to transform

[10] See also Salmon's recollection in Reichenbach 1978, I, 73.

many blind posits into appraised posits. One such method is the concatenation of inductions that we reviewed above, and that Reichenbach again mentions in his letter (Reichenbach 1978, II, 410). As we have seen, there is always a residual need for blind posits of higher order, but Reichenbach argues that this does not have the disastrous consequences that Russell claims.

Reichenbach begins by complaining that "Strangely enough, you say … that the inductive posit can be shown to be false" (Reichenbach 1978, II, 409). For Reichenbach, a posit is not something that can be true or false. A posit is a decision that can be more or less justified based on its pragmatic consequences. When it comes to Reichenbach's principle of induction, he defends it against Russell's worry by insisting that it nevertheless remains pragmatically justified:

> Induction merely supplies an asymptotic rule which eventually must come true if there is a limit of the frequency. And this is true of your example, too. Suppose you define your class B as the class of all things except the elements of the sequence beyond n. Then all elements up to n are in B, and the inductive conclusion would be: the next element is in B. Further observation would show this conclusion to be false. Continuing this procedure, you would soon find that the relative number of elements in B gets smaller and smaller, and the inductive conclusion would furnish for an element B a probability that converges to 0. So the asymptotic rule works correctly (Reichenbach 1978, II, 409-410).

Reichenbach argues, then, that for any classes A and B of the sort Russell invokes, no matter how artificially defined, his inductive principle will eventually lead an agent to settle on the appropriate probability. This is the sense in which the principle is justified. It cannot be justified

as a means to rationally believe that the next A observed will also be a B. This is the mistaken kind of justification that rationalistic philosophers have sought for in vain. Russell is thus criticized for this outdated dogma.[11]

VI. Taking stock

Russell and Reichenbach agree that our scientific knowledge includes the existence of unobservable entities and that we should not reform scientific practice to eliminate these commitments. They are both scientific realists. However, Russell anticipates a defense of scientific realism that employs some basically credible starting point that is then systematized to arrive at a collection of propositions that is internally coherent. This holistic process serves to prune the original structure and pick out how the world must be for that collection of propositions to be true. Russell then adds an externalist element: if the world is in fact that way, then our purported knowledge is genuine knowledge, but there is nothing that we can do to improve our epistemic situation beyond continuing to investigate the world.

Reichenbach agrees with Russell on this basic methodological point: our knowledge enables predictions concerning the future, but any prediction is fallible, and the best way to proceed is to continue to update our knowledge as we gather more evidence. This agreement masks the more "decisive" disagreement concerning what knowledge consists in. For Reichenbach, all knowledge rests on commitments that are not even truth-apt. This conception of knowledge is typically associated with the rejection of scientific realism. Most influentially, van Fraassen sometimes ties his anti-realism to his empiricist "stance", where a stance is an

[11] See also Reichenbach 1949b. Russell wrote a brief letter in response to Reichenbach, dated 1949/4/22. He concedes one "mathematical error" in an objection to Reichenbach that we have not discussed, but adds that "most of the other points seem more capable of being argued" (Bertrand Russell Archives 53900 RA3 17K).

attitude towards inquiry that can be voluntarily adopted. More recently, some scientific realists have admitted that scientific realism also rests on a distinctively realist stance or framework (Psillos 2011b, Chakravartty 2017). So the disagreement between Russell and Reichenbach in the 1940s has potential significance today for ongoing debates about the proper relationship between empiricism and scientific realism. To determine this significance we can first note some broad areas of agreement between Russell and Reichenbach. Both advance individualism about knowledge: what is known is known by individual subjects, and so that is the proper focus for philosophical investigation. Second, scientific knowledge is homogeneous: there is one account of how scientific knowledge is possible that will apply to all domains of investigation. From this common starting point Reichenbach makes the case for ineliminable decisions in the form of blind posits. Russell responds by supposing that some beliefs are basically credible and that one's environment can provide additional justification even if one is not aware of these aspects of the environment.

Many philosophers now reject Reichenbach's pragmatic defense of induction for reasons that are broadly similar to Russell's worries. If we require that evidence bear on truth, then it is hard to see how Reichenbach's inductive principle is appropriately tied to evidence. For Russell, the availability of artificially constructed classes shows that no unrestricted principle of induction can be sustained. This convinced Russell to change his approach, and focus on more restricted principles of non-demonstrative inference. Reichenbach is correct to say in his response to Russell that nothing about Russell's worries shows that Reichenbach's inductive posit is false. As we have seen, the decision to apply a weight as a blind posit is not truth-apt. However, Reichenbach seems to have missed the implications of Russell's artificial

classes. The point can be brought out in a concrete way using Reichenbach's own example of the observed temperature series that we discussed above. We can imagine an agent X who collects the observations as Reichenbach describes them. Such an agent faces a conflict between first and second order inductions, which can be resolved as Reichenbach imagines. Agent X thus concludes that every substance melts.

However, a second agent Y can describe the very same observations using a different vocabulary: a substance *smelts* just in case the substance melts for some temperature below 2000 degrees Celsius or the substance does not melt for any temperature. Then if all observations so far pertain to temperatures below 2000 degrees Celsius, the series of observations for copper, iron and carbon support the conclusion that every substance smelts. If agent Y adopted Reichenbach's inductive principle, then they would be led to assign a high probability to the claim that the next observed substance will smelt. This leads to predictions that are flatly contradictory to the predictions of agent X whenever observations are taken at temperatures above 2000 degrees Celsius.

Reichenbach's response to this conflict is that it will be resolved once observations are recorded at this temperature. However, there is nothing that agent X or Y can do before these observations that will indicate how the conflict can be resolved. Russell adds to this worry that both agent X and agent Y are in a position to see that most uses of the inductive principle will go wrong. So it seems that an agent using Reichenbach's principle is placed in an incoherent position: they resolve to make a prediction and act on it using a blind posit, but they also recognize that their prediction is unlikely to succeed. Reichenbach appeals to advanced

knowledge as a corrective on this, but if one's starting point is sufficiently far from the mark, no advanced knowledge will save the situation.

It is hard to see, then, how anything like Reichenbach's blind posits can inform a version of scientific realism that maintains that we know something about unobservables.[12] Of course, Russell's defense of scientific realism has its own problems. Two parts of this defense seem especially weak. First, it is just not clear that the holistic process of systematizing our original data will converge on a metaphysics whose only commitments are Russell's events or percepts. Here Russell anticipates the current mania for restricting one's ontology to what is metaphysically fundamental. But, to echo Reichenbach, it may be that the belief in a single category of metaphysically fundamental stuff is one more "remnant from rationalistic philosophies." A scientific realist could agree with Russell's basic point that our philosophizing must operate on some domain of initially credible data, and yet argue that the resulting metaphysics is highly differentiated and pluralistic, with various domains related in ways that go beyond one domain grounding another.

The other weak point of Russell's approach is his treatment of credibility. Russell is right to argue that our philosophical investigations must operate on what we initially believe, and that these beliefs come in various strengths that can be associated with degrees of belief. However, it is not clear how we can transform these subjective degrees of belief into the quasi-logical measures that Russell requires. A subjective Bayesian can recognize the rationality of a coherent set of degrees of belief. Russell holds out for much more than this, though. Each basic

[12] See also Eberhart & Glymour 2011. They note that Reichenbach offered an explicit response to Goodman (1947) in the revised English edition of his *Theory of Probability* (1949a, 448-450), but complain that this response "makes no sense" (2011, 384). Reichenbach's published reply to Goodman is very similar to his response to Russell.

belief comes with its own credibility assignment that is intrinsic and that establishes the extent to which it is rational to maintain this belief. Reichenbach seems right that this is a mysterious element of Russell's approach that is best discarded. What would be left would be the more familiar Bayesian notion of a prior probability. It informs our reflections, but is not ascribed any normative significance beyond its role in assessing the coherence of our degrees of belief.

If we try to avoid these two problematic aspects of Russell's defense of scientific realism, then we wind up with a much more limited and piecemeal scientific realism. There are various principles of non-demonstrative inference, but they are tailored for each domain of investigation. Their justification can consist in the two kinds of elements that Russell imagined. First, a principle is subjectively justified based on its capacity to systematize and validate the reasoning that appears plausible to the agents who have investigated that domain. Second, this subjective justification must be supplemented by an externalist justification. Agents can know the conclusions that result from this principle only if their investigations relate to how the world really is. The real work of such a defense of scientific realism is thus in identifying the principles that accord with our purported knowledge and assembling the evidence that these principles have the right knowledge-generating characteristics.

From a common starting point of individualism about knowledge and the assumption that scientific knowledge is homogeneous, both Russell and Reichenbach failed to build a plausible defense of scientific realism. We can use their work and its limitations to motivate the view that scientific knowledge works differently in different domains. If this is right, it also seems worth examining the other common starting point, namely individualism about knowledge. For if different areas of science generate knowledge in different ways, then it is

unlikely that any individual will be in a good position to acquire knowledge by themselves as they consider different domains. There may be an ineliminable social aspect to scientific knowledge tied not only to testimony between scientists but also based on the role of an inherited tradition in enabling individual knowledge. So it is too much to hope that clarifying the philosophical relationship between Russell and Reichenbach will settle how best to defend scientific realism. But in mapping out their relationship in a more comprehensive and accurate way, we can at least begin to see what defenses have been proposed and where they come up short.

Acknowledgements

An earlier version of this paper was presented at the Workshop on the History of Analytic Philosophy, Université Clermont-Auvergne, Clermont-Ferrand, France, May 2019. I am grateful to the audience for their helpful suggestions, especially Sébastien Gandon and Henri Galinon. I would also like to thank Alexander Klein for his assistance in obtaining the correspondence between Russell and Reichenbach. Two anonymous referees offered very helpful suggestions for how to improve this paper.

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