

## *On Induction and Russell's Postulates*

Russell's later epistemology, dominated by his *Human Knowledge: Its Scope and Limits* (1948),<sup>1</sup> has received relatively little attention. Yet it contains plenty to interest a contemporary audience. There is a version of foundationalism that subtly avoids many difficulties standardly held to beset such a position. There is a naturalistic approach to various aspects of knowledge, a naturalism which both regards evolutionary facts as relevant to problems about induction, and also singles out the purely descriptive task of locating knowledge as a phenomenon within the natural order, a task that turns its back on traditional skeptical problems. There is a discussion of what would nowadays be called "Goodman's paradox," on the basis of which Russell draws the conclusion that a correct system of inductive reasoning could not be purely formal.<sup>2</sup> And there is more besides.

I want to look above all at Russell's "postulates of scientific inference." He held that unless these are known independently of experience, "science is moonshine" (p. 524). Yet he also held that postulates state contingent facts about the way the actual world happens to be. This may not strike us as a very comfortable position, but Russell offers solace in the form of a combination of modified foundationalism and a species of naturalism. The nature of this foundationalism, as presented in *Human Knowledge*, will set the background to the issues.

### 1. A Minimal Foundationalism

Foundationalism must entail that there is a distinction between (as I shall put it) *data*, which form the foundations of knowledge, and *hypotheses*, which form its superstructure and are justified, if at all, by, and ultimately only by, data. This much, Russell accepted. But this much has often been coupled with the following further doctrines, all of which Russell rejected:

- (a) The data are certain.
- (b) No proposition can contribute to the justification of a datum.
- (c) A datum has a distinctive content.

Russell defined a datum as:

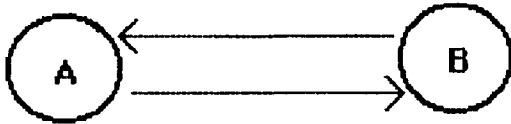
a proposition which has some degree of rational credibility on its own account, independently of any argument derived from other propositions. (p. 409; cf. p. 401)

This is consistent with the degree of rational credibility of a datum being less than maximal, and thus makes room for Russell's view that "a datum may be uncertain." He argues for this from such phenomena as faint perceptions, uncertain memories, and the dim awareness of logical connections: these may give us grounds, however inconclusive, for believing hypotheses. They serve as data, despite being uncertain.

The denial of (b) is closely connected with the denial of (a):

I may have a faint memory of dining with Mr So-and-So sometime last year, and may find that my diary for last year has an entry which corroborates my recollection. It follows that every one of my beliefs may be strengthened or weakened by being brought into relation with other beliefs. (p. 401)

In Russell's foundationalism, unlike some traditional versions, one cannot identify the data on a purely structural basis. In some traditional versions, if one sets out a person's body of knowledge as a flow diagram, with arrows indicating the flow of justification, his data would be identifiable as beliefs having no incoming, but at most outgoing, arrows. Consider a sub-structure like this:



In some traditional systems that permit such a substructure (and not all do), one could infer that neither *A* nor *B* is a datum. For Russell, one cannot infer anything about whether or not *A* and *B* are data. The diagram is consistent with either or both being data. (Unless some other arrows enter *A* or *B*, however, at least one must be a datum.) To display Russell's system in this fashion, one would have to attach numbers to the arrows to show how much credibility was being transmitted, and numbers to the propositions to indicate their total degree of credibility from all sources. A datum could then be defined as a proposition whose total degree of credibility exceeded the sum of the degrees associated with the inflowing arrows.

A traditional doctrine is that the source of the "intrinsic" credibility of a datum is the subject matter of the proposition. For example, the proposition concerns the subject's sense experience, so it is intrinsically credible for him. This is the sort of doctrine I intend to subsume under (c). Although he is not completely explicit, Russell seems to have rejected this. There are plenty of examples, like the one quoted about dining with Mr. So-and-So, in which it is obvious that it is not

some feature of the proposition itself that gives rise to its “intrinsic” credibility, but rather something about how the believer is related to it. One could believe “I dined last year with Mr So-and-So” in circumstances under which it has no intrinsic credibility at all. But under other circumstances the proposition can have some intrinsic credibility, for example, when it is the content of an apparent memory. This shows that having some special subject matter, for example, being about sense experience, is not necessary for being a datum.

Subject matter should also not be held to be sufficient for being a datum, as is established by examples of illusion or hallucination when the subject is aware that his experience is nonveridical. I have not found this point in *Human Knowledge*, but nor have I found anything inconsistent with it.

The first of these points, that a special subject matter is not necessary for being a datum, suggests that in Russell’s definition of a datum we should not understand the phrase “independently of any argument derived from other propositions” to entail “independently of any other fact about the person for whom it is a datum.” Rather, the “intrinsic” credibility of a person’s datum consists in its having some credibility not owed to other of his *beliefs*. Its credibility may well be owed to facts about him other than what he believes. For example, placed in full view of a walnut tree, the belief “There’s a walnut tree” may not owe any credibility to the perceiver’s other beliefs, though its credibility is dependent on such facts as that his senses are working normally.<sup>3</sup> This latter sort of dependence should not be allowed to rule the belief a nondatum.

This concession is important, for it renders highly implausible the iteration principle (If  $X$  knows that  $p$ , then  $X$  knows that  $X$  knows that  $p$ ), which has characterized traditional foundationalism. It is much easier for “There’s a walnut tree” to *be* intrinsically credible for you, thanks to the fact that your senses are working normally, than for you to *know* that it is intrinsically credible for you. In order that “There’s a walnut tree” should *be* intrinsically credible for you, there is no need for you to defeat Descartes’s demon; but, arguably, you do have to defeat the demon to know that your senses are working normally, and thus to *know* that “There’s a walnut tree” is intrinsically credible for you.

So once you allow, as there is evidence that Russell does, that the intrinsic credibility of a proposition can be affected by the believer’s circumstances, you should come to have serious qualms about the iteration principle. I have not found any explicit statement of such qualms in *Human Knowledge*. However, Russell had a quite different reason for having them, at least with respect to some kinds of knowledge. He stressed the importance of the kind of knowledge, not necessarily conceptual in character, that animal behavior may be held to manifest. The conditions for this kind of knowledge, summarized on page 450, clearly permit knowledge that the knower does not know he has, and so defeat the iteration principle.

The significance of the iteration principle is that it determines a project for

epistemology: Descartes's project. The Cartesian idea is that the knowing subject should be able to sift through his beliefs, distinguishing knowledge (or justified belief) from unsatisfactory belief. The project requires that the knower be able to identify his knowledge. That is, it requires the iteration principle. If one takes seriously the falsehood of the iteration principle, one will not suppose that one who genuinely possesses knowledge is thereby assured of success in Descartes's project. Russell's shift of perspective toward knowledge as an evolutionarily adaptive natural process is a move away from Descartes's project. Whether his emancipation from the Cartesian style of epistemology is complete is another matter.

Russell summarizes his foundationalism in the following passage:

The edifice of knowledge may be compared to a bridge resting on many piers, each of which not only supports the roadway but helps the other piers to stand firm owing to interconnecting girders. The piers are the analogues of the propositions having some intrinsic credibility, while the upper portions of the bridge are the analogues of what is only inferred. But although each pier may be strengthened by the other piers, it is the solid ground that supports the whole, and in like manner it is intrinsic credibility that supports the whole edifice of knowledge. (p. 413)

Only a datum can be a primary source of credibility. The credibility of any nondatum is ultimately owed only to data. The question that will be my concern in this essay is the one to which Russell devotes part VI of *Human Knowledge*: how do data transmit credibility to hypotheses? It is in answering this question that Russell introduces his "postulates of scientific inference," postulates that are contingent, not knowable through experience, yet that have to be known if science is to be known.

## 2. The Transmission of Credibility: Russell's Doctrines Concerning Principles of Evidence

Consider a hypothesis,  $H$ , which is made credible by some body of data,  $D$ . Let us call this relation between  $H$  and  $D$  the  $C$ -relation. Russell appears to hold the following theses:

1. Particular facts of the form  $C(H, D)$  hold in virtue of some general features of  $H$  and  $D$ . Hence there are general principles—I shall call them *principles of evidence*—determining the conditions for the obtaining of the  $C$ -relation.
2. Principles of evidence are true in virtue of contingent facts about the actual world. Russell refers to these facts as the *postulates of scientific inference*.
3. Principles of evidence must be known, if any hypotheses can be known.

4. Principles of evidence cannot be known from experience.
5. Principles of evidence are known.

In this section I simply present the evidence that these are Russell's views. In subsequent sections I evaluate some of them.

Evidence for (1) can be found in many places. His treatment of the principle of induction makes it plain that it has the kind of generality he thinks is appropriate, though he also thinks that, unless stated with some "hitherto undiscovered limitation" (p. 436), it will give the wrong results. As he says later:

Induction, we have seen, is not quite the universal proposition that we need to justify scientific inference. But we most certainly do need *some* universal proposition or propositions. (p. 524)<sup>4</sup>

This need is based on the fact, as I would put it, that it is the nature of the *C*-relation itself that requires characterization, if we are to understand and validate scientific inference.

Further evidence for (1) comes from his discussion of animal expectation. He envisages a dog's expectation of *B*, triggered by a perception of *A*. Suppose, further, that *A* is always, or nearly always, followed by *B*. Does this justify the dog's expectation? He argues that it is not enough.

Suppose that, although *A* is in fact always followed by *B*, this generalization only happens to be right, and most logically similar generalizations are wrong. In that case we must regard it as a stroke of luck for the dog that she has hit on a case in which a fallacious process, by chance, leads to a true result. (p. 446)

Notice here that where some would have looked to natural necessity in the generalization, Russell looks to whether data and hypothesis instantiate the *C*-relation. The quotation assumes that data and hypothesis could not be thus related if most logically similar generalizations are wrong.

The following quotations are evidence that Russell holds (2):

Any principle which will justify inference from the particular to the general must be a law of nature, i.e. a statement that the actual universe has a certain character which it would be possible for it not to have. . . .

Scientific inferences, if they are in general valid, must be so in virtue of some law or laws of nature, stating a synthetic property of the actual world, or several such properties. (pp. 354, 436)

Russell does not distinguish between synthetic and contingent.

One of the most explicit assertions of (3) is:

We need, among the premises of science, not only data derived from perception and memory, but also certain principles of synthetic inference. (p. 355)

He is prepared to count these principles of evidence among our data.

I am here including among data the principles used in any inferences that may be involved [in getting to hypotheses]. (p. 401)

Bearing in mind that a datum is a belief, this quotation again makes plain that it is not merely that principles of evidence have to be true, they also have to be known (or at least justifiably believed):

When an argument is stated as simply a possible, the connection asserted in every step has to be a datum. (p. 412)

However, there is one moment when he shows some hesitation:

It is more necessary [for the justification of inferences] that there should *be* laws than that they should be known. (p. 354)

Thesis (4) is a doctrine familiar from Hume, and also familiar from Russell's own earlier writings. A clear statement of it, along with a hint of an argument, is:

The truth of [the postulates] cannot be made even probable by any argument from experience, since such arguments, when they go beyond hitherto recorded experience, depend for their validity upon the very principles in question. (p. 436)

Russell is quite tentative in his affirmation of (5). For one thing, he thinks that the only way to prove it is by accepting (3) and rejecting skepticism: since we do have knowledge of hypotheses, we must have whatever other knowledge this knowledge requires. For another thing, when he comes to say what this knowledge is like, he finds it resembles animal expectation—a kind of nonconceptual knowledge. Thus we find:

Our knowledge of these principles—if it can be called “knowledge”—exists at first solely in the form of a propensity to inferences of the kind that they justify. It is by reflecting upon such inferences that we make the principles explicit. (p. 526)

Russell warns that he will depart from tradition in his use of the term “knowledge.” He more or less identifies knowing with having a reasonable degree of credibility in belief (p. 444–45), and I shall throughout consider only the latter.

In the following three sections, I shall discuss in detail the theses (2), (3), and (4).

### 3. The Contingency Thesis

A *principle of evidence*, as I shall use the phrase, is a conditional statement. Typically, the antecedent will set out conditions satisfiable by data, and the consequent will state that such data confer rational credibility on a hypothesis whose

content is related in a certain way to that of the data. In short, a canonical form would be: For any  $D$ , if they satisfy . . . , then  $D$  confer rational credibility on any  $H$  satisfying \_\_\_\_\_. We would have to allow for credibility to be a matter of degree, though this refinement will be needless for our purpose. Strictly speaking, both antecedent and consequent of a principle of evidence can be any proposition, whether a datum or a hypothesis. For Russell's view is that a credible hypothesis can add credibility to a datum, and it can certainly do this to another hypothesis. However, the basic case is one in which the antecedent specifies a condition on data, and the consequent specifies a condition on a hypothesis, and it will make the exposition easier to couch the principles in terms of the basic case. A further qualification is that a principle of evidence should allow for relativization to a person: if  $D$  are data *for*  $x$  and satisfy . . . , then if  $H$  satisfies \_\_\_\_\_ it is rationally credible *for*  $x$ . I shall not always make this relativization explicit.

It is important to be clear about exactly what such principles deliver. Is it conclusions of the form specified in the  $C$ -relation:  $H$  is credible relative to  $D$ ? Or is it an absolute assignment of credibility to  $H$ ? The point is that some  $H$  might be credible relative to one set of data but not relative to another, so if we had only conclusions of the form  $C(H, D)$  we still would not know what it is rational to believe, for we would not have been told what set of data it is best to use.<sup>5</sup> One way around part of this difficulty is to stipulate that the antecedent of a principle of evidence requires that the data be the total of one's data, and it is on these data that the condition is placed. We can then see the consequent as assigning (a degree of) credibility absolutely to a hypothesis. One would still need a justification for thinking that it is a good thing to try to expand one's total body of data (see note 5); and it would remain an open question, and one concerning which some initial skepticism is appropriate, whether any plausible principles of this form can be formulated. But I leave these problems to one side.

Russell presents some of his postulates explicitly as empirical generalizations, rather than as principles of evidence, for example, postulate I:

*Given any event  $A$ , it happens very frequently that, at any neighbouring time, there is at some neighbouring place an event very similar to  $A$ .* (p. 506)<sup>6</sup>

But he presents other postulates as principles of evidence themselves. Postulate II, for example, is (approximately)<sup>7</sup> a principle of evidence.

It is frequently possible to form a series of events such that, from one or two members of the series, something can be inferred as to all the other members. (p. 508)

Russell feels able to move freely between these two very different kinds of postulates in virtue of his thesis that a principle of evidence (properly speaking) is made true, if it is true, just by contingent features of the world, and so is itself contingent. This is what I call the *contingency thesis*. To avoid confusion, I shall use

Russell's term "postulates" to refer to the particular propositions he calls postulates, and will always regard the contingency thesis as stated with respect to principles of evidence, and not postulates. As we have seen, some of the postulates are obviously contingent. With respect to these, what is of interest is whether some corresponding principle of evidence is necessary or contingent. Thus, corresponding to postulate I would be a principle of evidence along the lines:

If it is a datum that an event  $\alpha$  occurred at a place  $p$ , time  $t$ , and that  $\alpha$  has a value  $v$  for some parameter  $\Phi$ , then the hypothesis that an event occurs near  $p$  and near  $t$  and with a value for  $\Phi$  near  $v$  is rationally credible.<sup>8</sup>

Russell argues against the view that principles of evidence are necessary, notably on pages 387 and 420 of *Human Knowledge*. I quote the latter passage in full:

If "probability" is taken as an indefinable, we are obliged to admit that the improbable may happen and that, therefore, a probability-proposition tells us nothing about the course of nature. If this view is adopted, the inductive principle may be valid, and yet every inference made in accordance with it *may* turn out to be false; this is improbable but not impossible. Consequently a world in which induction is true is empirically indistinguishable from one in which it is false. It follows that there can never be any evidence for or against the principle, and that it cannot help us to infer what will happen. If the principle is to serve its purpose, we must interpret "probable" as meaning "what in fact usually happens"; that is to say, we must interpret a probability as a frequency.

Russell distinguishes two senses of "probable." One makes "is highly probable" equivalent to "is highly credible," and it is this sense that is at issue in the passage quoted. The only principle of evidence mentioned in the passage is the inductive principle, which, as we have mentioned, Russell took to be invalid in its standard formulations. But it seems clear that the argument, if good at all, would be good against any principle of evidence. It trades on no special feature of the inductive principle.

The main thrust of the passage, adapted to the terminology I have introduced, seems to me to be this: if principles of evidence were necessary, then they would hold regardless of the actual course of events. But then there would be no connection between forming one's beliefs in accordance with good principles of evidence and being right. So there would be no *point* in forming one's beliefs in accordance with good principles of evidence. Being a *good* principle would be an empty honor.

There is a subsidiary argument to the effect that if the principles were necessary there could never be any evidence for or against them. Given that some necessary truths are knowable but not knowable a priori, it would not follow immediately from the necessity of principles of evidence that there is no evidence for them.<sup>9</sup> In any case it is odd that Russell should regard this as a defect in the



necessity thesis, since he is, as we have seen, at pains to point out that, on his version of the thesis that principles of evidence are contingent, there can be no evidence for the principles themselves. I postpone all discussion of the epistemic status of principles of evidence until section 4, and return now to the main argument for contingency: that it is needed to connect credibility to being right.

Russell, in the passage, merely asserts this need. But it has to be argued for. An opponent might deny it, and perhaps lend color to his denial by asking us to consider a universe in which the evil genius presents Descartes with a facsimile of his veridical experience, yet nothing exists other than these two spirits. The opponent might claim that while this makes all the difference to the *truth* of Descartes's beliefs, it makes no difference to their degree of credibility. Once one has accepted that a falsehood can be credible, it is tempting to say that credibility imposes no requirement of truth, however indirect. Let us examine the clash between these positions more closely.

An initial point is that it ought not to be controversial that *some* principles of evidence are contingent. For suppose we know a contingent truth of the form "Most  $\Phi$ s are  $\Psi$ s." Then (ceteris paribus) if it is a datum that  $\alpha$  is a  $\Phi$ , the hypothesis that  $\alpha$  is a  $\Psi$  is rationally credible. This is a principle of evidence, but it is contingent. Russell took this as the model for all principles of evidence. The controversial issue is not whether there are some principles of evidence having this status, but whether all do.

It may be objected that the real principle of evidence at work in the preceding example is one that assimilates "Most  $\Phi$ s are  $\Psi$ s" to the data. It is then unclear whether the "real" principle is contingent. Certainly, it is not contingent upon most  $\Phi$ s being  $\Psi$ s.<sup>10</sup> The most this could show, however, is that contingent principles like the one given in the preceding paragraph are derivable from necessary principles together with contingent facts about the course of nature. The point fails to show that there are no contingent principles.

The contingency thesis, then, is best formulated as the thesis that *all* principles of evidence are contingent. The main argument at which Russell hints in the passage quoted is directed at the necessity of any principles of evidence. If any were necessary, one might form one's beliefs in accordance with them, yet be wrong more often than not. Is this true? Would its truth damage the view that there are necessary principles of evidence? One can analyze Russell's argument as having two parts, corresponding to these two questions.

(i) *Necessary principles of evidence could award credibility to no end of false conclusions.* There is a sense in which it is true, as Russell says in the passage quoted, that if a principle of evidence is necessarily true, it "tells us nothing about the course of nature." The sense is that a necessary proposition does not entail any contingent proposition, so there is nothing to be inferred from it about the special nature of our world, the special features of the actual course of nature. But does it follow from this that it is possible for every inference made in accor-

dance with a necessary principle to have a false conclusion, while having true premises? It might seem that it does not. For are not decent principles of inference self-correcting? Will not the very content of the principle ensure that the falsity of its conclusions disables it for future use? So as soon as the principle starts yielding false conclusions, will it not cease to be applicable, in virtue of the total data no longer being of the sort that can transmit credibility to related hypotheses?

Here is a simple example of a form of principle that allows all conclusions to which it assigns credibility to be false: for all  $F$ ,  $G$ , if the data entail that more of the observed  $F$ s are  $G$  than non- $G$ , then any hypothesis is rationally credible if of the form: the next (hitherto unobserved)  $F$  to be observed is  $G$ .<sup>11</sup> Suppose that the sum of your current data entails that more of the observed  $\Phi$ s are  $\Psi$  than non- $\Psi$ . Let us also suppose that these are *only* data, and in particular that the principle of evidence has not been used to transmit any credibility to them. But now you start using the principle and you infer that the unobserved  $\Phi$ ,  $\alpha$ , is  $\Psi$ , the unobserved  $\Phi$ ,  $\beta$ , is  $\Psi$ , and so on. Plainly, it could be that all of these conclusions are false.<sup>12</sup> As their falsehood becomes a datum, there will come a time when you will not have data that can be used to extract, by modus ponens from the *current instance* of the principle, an attribution of credibility to "the next  $\Phi$  is  $\Psi$ ." In this modest sense the principle is self-correcting. But at this stage,<sup>13</sup> you will have the data needed to apply another instance of the principle, in which non- $\Psi$  replaces  $\Psi$ : if the data entail that more of the observed  $\Phi$ s are non- $\Psi$  than  $\Psi$ , then any hypothesis is rationally credible if of the form: the next (hitherto unobserved)  $\Phi$  to be observed is non- $\Psi$ . Every application of this fresh instance of the principle might yield a false conclusion: the next run of observed  $\Phi$ s might be  $\Psi$ s. What Russell is asking us to find paradoxical is that, if the principle is necessary, such a series of false predictions does not impugn its correctness.

The basis of Russell's case is that data do not entail hypotheses, not even when they lend them credibility. This does not lead at once to the conclusion that a principle of evidence could yield none but false hypotheses. Perhaps the same principle could at one time confer credibility upon  $H$ , given data  $D$ , and also later confer credibility on not- $H$ , given subsequent augmentation of  $D$ . Then it obviously could not be that all the hypotheses upon which the principle confers credibility, in the history of a single world, are false. But there is no necessity that the hypotheses delivered by a growing body of data should be thus related. They could all be false together. Russell wants us to say that the realization of this possibility would be a refutation of the principle: the truth of a principle ought to guarantee that the possibility is not realized, and so should tell us something about the course of events in the actual world.

I think we must accept the possibility. But must we draw Russell's conclusion from it?

(ii) *Does it matter that (i) is true?* I think not. But consider the opposite view. A principle of evidence is supposed to confer rational credibility. But what is ra-

tional about believing something unless there is at least a good chance that it is true? One does not expect *every* credible proposition to be true. But one has a right to expect that if one follows the strategy of forming one's beliefs in accordance with correct principles of evidence, one should end up with more truths than falsehoods. If a principle could be true while, from now on, conferring credibility only upon falsehoods, one would be denied this right. It is not that one is looking for some absolute guarantee that some principle of evidence that one takes to be true is really true. Nor is one looking for a guarantee of the truth of any inductive belief. The point is, rather, that if one *does* follow a true principle, that ought to ensure that one believes a *reasonable proportion* of truths. To deny this connection is to prevent inquiry from being a search after truth. It becomes merely a search for "rational" belief: a goal that, shorn of its link with truth, is of no value.

It is trivial that a proposition that is awarded credibility by a correct principle of evidence on the strength of some data, *D*, is probable, relative to *D*, in the sense of being credible, relative to *D*. What the preceding argument claims is that it must also be probable in a frequency sense, echoing Russell's claim already quoted.<sup>14</sup> The frequency in question is a decent ratio (say exceeding 1/2) between true and false beliefs among those formed in accordance with the principle of evidence. It would certainly be very pleasant to be able to show that this frequency in fact obtains for the principles of evidence we use.

To undermine this line of thought, to show that a principle can be true even if there is no such frequency, consider the fact that principles of evidence can but reflect the nature of our cognitive powers. If we were omniscient we would need no such principles. The facts themselves would be, so to speak, laid out before us. We would not need to grope our way toward them from the evidence. If we found rather little correlation between written records of past events and our apparent memories of them, we could not use the principles of evidence we do use. Principles of evidence, if correct, must reflect the best we can do. But, unless we make a sharp turn toward idealism, we cannot *stipulate* that the best we can do will lead to a preponderance of truth. Perhaps truth is beyond us. So it is wrong to say that, *by definition*, a correct principle of evidence will award rational credibility to more truths than falsehoods.

This does not entail that an empty, because no longer truth-connected, ideal of rationality must replace that of truth. We cannot aim higher than to do the best we can. *Best* is the best we can do in the search for truth. But to search is not necessarily to find.

What my argument purports to show is that a realist will not find anything especially disconcerting in the possibility, established under (i), of a correct principle of evidence leading us badly astray. For a realist, this is just the way we relate to truth. The argument does not, of course, purport to establish realism. But, in the context of *Human Knowledge*, realism can be taken for granted. Part of the

overall aim of the book is to reject the method of construction, characteristic of Russell's idealism in, for example, "The Relation of Sense-Data to Physics."<sup>15</sup> In its place is the method of inference, involving the intelligibility of objects lying beyond acquaintance, and of which our knowledge cannot be certain.

If I am right, Russell's realism ought to have undermined his argument for the contingency thesis. How can we explain this tension? Part of the explanation lies in his failure to give due attention to the distinction between saying how things must be if our scientific beliefs are to be, by and large, justified, and saying how things must be if our scientific beliefs are to be, by and large, true. The reason for this assimilation, in turn, is that he had enumerative induction before his mind as the main model for a principle of evidence. Faced with the problem of what regularities should be extrapolated, he sought an answer in terms of the *content* of the regularities, in terms, for example, of whether they instantiated postulate I.<sup>16</sup> And so the natural conclusion was that the inferences are justified only if regularities of the relevant kind obtain. And, if they do, it is not merely that scientific beliefs will be, by and large, justified; they will also be, by and large, true. Had Russell had before his mind a quite different kind of principle of evidence, for example, one that attributes credibility to a hypothesis in virtue of its explaining the data, it seems unlikely that his thinking would have followed this route.

There is also a deeper explanation, which I elaborate in section 6, of why Russell should have thought that the possibility of conferring credibility on none but false hypotheses matters: Russell *does* have to confront the skepticism to which realism is inevitably subject, but he confronts it in a different form.

The upshot is that the claim that some principles of evidence are necessary survives Russell's attack, but that is not to say that it is true.

#### 4. Must Principles of Evidence Be Known?

If it were in general correct that, if one's reasoning is to lead from knowledge to knowledge (or credibility to credibility), one needs to know any principles of inference involved in one's reasoning, as well as the premises to which the principles apply, then one would do well to include the principles among one's premises: they would be among the things knowledge of which is required for knowledge of the conclusion. So should one who argues from  $P$  and if  $P$  then  $Q$  to  $Q$  include modus ponens among his premises? Suppose modus ponens is formulated: for all propositions  $A$ ,  $B$ , and  $C$ , if  $A$  is true, and so is if  $A$  then  $B$ , then  $B$  is also true. As Lewis Carroll showed,<sup>17</sup> adding this premise does not obviate the need for an application of modus ponens. So the propounder of the original argument gains nothing by including it, explicitly or implicitly, among his premises. This establishes either that one cannot include all one needs to know, in order to know a conclusion, among one's premises, or that one does not need to know the principles of inference one uses. The unattractiveness of the first disjunct is an argument for the second.

It is even clearer that it is wrong to hold that a principle of evidence needs to be known, in order to confer credibility. A principle linking  $D$  and  $H$  entails that one who believes  $D$  is rational to believe  $H$ . If the principle does not include itself among  $D$  (and we certainly assume that principles of evidence do not normally do this), then the credibility of  $H$  is independent of knowledge of the principle. So it seems as if Russell has simply made a mistake in saying that the principles of evidence must be known, if the hypotheses to which they relate are to be rationally credible.

In one who believed in the iteration principle, this mistake would be fairly easy to explain. The principle, applied to credibility, makes it impossible for  $H$  to be credible for someone without it also being credible for him that  $H$  is credible. It is hard to see how we are to have a decent basis for the stronger (iterated) claim without the principle that confers the first credibility being itself credible.<sup>18</sup> But Russell, I have suggested, eschews the iteration principle. So how can his mistake be explained?

The answer lies in Russell's very low key conception of knowledge, a conception that in effect robs his claim that the principles of evidence must be known of any significance—and thus, indeed, saves him from a mistake. The conditions for the kind of knowledge that Russell says we have of principles of evidence are set out on page 450 (the last page of part VI, chapter 1). In effect, the upshot is that you know a principle if it is true, you reach hypotheses that it licenses, and you do so as a causal consequence of the impact on you of the data that it specifies. This does not entail knowledge, or rational belief, or even belief, as the terms are normally used. I do not say this in criticism of Russell. Rather, abstracting from Russell's special conception of knowledge, in *Human Knowledge*, it would be less misleading to say that he held that we do *not* have to know the principles of evidence.

To leave it at that would be to miss something important. As Russell clearly states at the very beginning of part VI (p. 439), the principles are involved in two philosophical tasks: the descriptive task, of saying what the principles are that adequately describe what is generally accepted as good inductive reasoning; and the justificatory task, of saying whether this reasoning is *really* good. It is the latter task, rather than the former, which creates *for the philosophical inquirer* the need to know the principles of evidence. While a person who reasons in accordance with a true principle ends up with a rationally credible conclusion, we want to know whether the principles we implicitly invoke, the ones specified in discharge of the descriptive task, are true. There is no nonskeptical answer to that question, it would seem, except one that shows that we know the principles, and know them, moreover, in the ordinary sense of the term. Showing this would, in Russell's framework, be the way to provide a solution to the traditional problem of justifying induction.

### 5. Can Principles of Evidence Be Known from Experience?

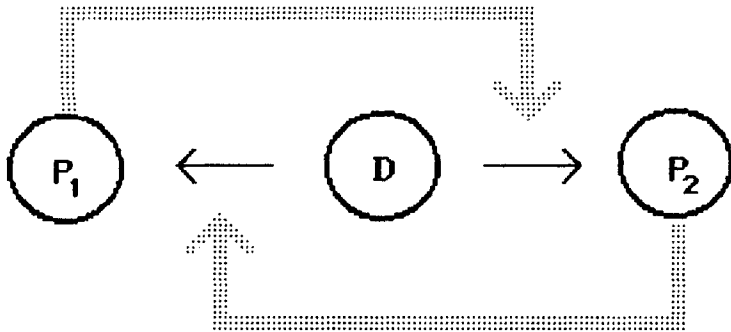
In the most naive view, it is observed cases of  $\Phi\alpha$  and  $\Psi\alpha$  that support generalizations of the form "For all  $x$ , if  $\Phi x$  then  $\Psi x$ ." But if the consequent of a principle of evidence has the form that we have supposed – " $H$  is rationally credible for  $x$ " – it is not clear what sort of evidence would support it because it is not clear what it would be to "observe" that we had a true instance of the consequent. What observations are supposed to establish that  $H$  was, or was not, credible for  $x$ ?

Russell's view is that a principle would be supported, if it could be shown that most of the hypotheses upon which it confers rational credibility are true. For Russell, the problem is that showing this itself requires a principle of evidence, so we seem caught in a circle.

Before looking at this supposed circle, it is worth pointing out that a presupposition has already been made concerning the proper analysis of what it is for a principle of evidence to be correct. It must have the property I shall call that of being *truth yielding*, where a principle is truth yielding if and only if most of the hypotheses upon which it confers credibility are true. The circle for Russell consists in the fact that we cannot move from the datum that a principle has been truth yielding up to now to the hypothesis that it is in general truth yielding without invoking *another* principle of evidence.

We ought not to accept without question this analysis of what it is for a principle of evidence to be true. If the argument of section 3 is correct, truth yieldingness is not a necessary condition of the truth of a principle of evidence. Moreover, it is not obvious that it is sufficient. Might not a principle of evidence, like a person, fortuitously light upon truth more often than falsehood, without thereby delivering rational credibility? Russell says that what is needed to prevent this fortuitous correctness is that the principle will confer credibility on a *class* of hypotheses.<sup>19</sup> But if the class is a "manufactured" (grue-like) class, there is still the possibility of fortuitousness. Russell remained too much a Humean even to consider a notion of natural law involving necessity.

Let us return to Russell's alleged circle. He cannot mean that no principle of evidence could be justified empirically. Some data,  $D$ , and a principle,  $P_1$ , might confer credibility on another principle,  $P_2$ . What he must mean is that they cannot *all* owe all their support to being established empirically. If they are to owe all their support to being established empirically, they cannot be data, by definition, since a datum has some credibility on its own account. So one has a picture of a collection of principle-free data, and one asks oneself: how can I get anywhere? The answer, clearly, is that one can make no acceptable progress without moving to a hypothesis that is  $C$ -related to the data. One cannot infer from this alone that it is impossible for all the principles of evidence to receive empirical support. For example, consider the following structure:



The circles represent propositions or sets thereof. The thin black arrows connect the premises of some inductive reasoning to the conclusion. The shaded arrows are principles entailing that such premises lend rational credibility to such conclusions. In virtue of principle  $P_1$ , data  $D$  support  $P_2$  (or support the proposition that  $P_1$  is truth yielding). Every principle is supported empirically, yet there is no *straightforward* circle: no principle is the one in virtue of which the data that support it do so.

To introduce a charge of circularity, we have to introduce a foundationalist notion: that of a *total* justification. In a total justification, there will be a justification for every proposition mentioned, unless that proposition is a datum; and every principle of inference must be mentioned. The possibility of a total justification, in this sense, appears to be a consequence of the view that data alone are the ultimate source of credibility. There can be no total justification of all the principles of inference, unless either some principle is a datum or some principle occurs in its own justification. In the preceding simple structure, the justification of  $P_2$  by  $D$  and  $P_1$  is not a datum; for totality, one has to extend the justification to include a justification of  $P_1$ , and this reintroduces  $P_2$ . This circularity defeats the aim of showing that every proposition ultimately owes its credibility to, and only to, data.

If this argument is accepted, and it is granted that some principle of evidence is credible, then some principle is a datum. There are theoretically two possibilities. Some principle could be a sensory datum, like an apparent memory or a perception; but this seems absurd. Alternatively, some principle is known a priori. This is the alternative that Russell adopts.

I suspect that the most controversial aspect of this part of Russell's position is his reliance on the notion of total justification. We are often told that *in practice* justifications are not, and need not be, total, that justification is relative to context, and that the "philosophical" notion of total justification is too radically unlike our ordinary notion to derive its title to coherence from the ordinary one. I shall not present any such criticisms in detail. But I will draw attention to three points, which may arm Russell against this kind of criticism. The first is that we must

bear in mind that Russell's overall objective is not to argue for skepticism but rather correctly to identify the epistemological status of principles of evidence. The second is that he is not in the position of one who insists on justification while being prepared to take nothing for granted. On the contrary, he is rather liberal about what he will take for granted: all the data. The third is that he is not claiming that we need total justifications in practice. However, he rightly does not let practical concerns inhibit his philosophical curiosity. Practical concerns no doubt do not often require the explicit articulation of principles of evidence. But if we could succeed in articulating them, we would certainly be right to ask what their epistemic status is.

## 6. The Challenge of Skepticism

Russell's official discussion of skepticism in *Human Knowledge* is brief:

Skepticism, while logically impeccable, is psychologically impossible. (p. 9)

But the next sentence makes it plain that the skepticism Russell has in mind is what one might call a *first-order atheistic* skepticism: for example, one involving "the denial of physical events experienced by no one." This skepticism is atheistic in that it involves *denying* some commonly held beliefs, rather than merely *doubting* them. It is first-order in that the beliefs in question do not say anything about knowledge, credibility, or justification. Russell is no doubt right to dismiss such a skepticism in a couple of sentences. But the real enemy is a skepticism of a different kind: a *second-order agnostic* skepticism, as I shall call it. Russell does not acknowledge this skepticism, but I believe that it exerts an influence on his thought.

This influential skepticism is agnostic in that it adopts the attitude of doubt rather than denial, and second-order in that the propositions it doubts have the form: it is credible that  $p$  (where  $p$  is first-order). This is the serious form of skepticism about induction. Such skeptics believe as stoutly as the rest of us that bread will nourish and the sun rise. No doubt it would be psychologically impossible for them to rid themselves of these beliefs. What they doubt, however, is whether any such beliefs—any hypotheses, in the sense of this essay—are really worthy of credence; equivalently, whether there are any true principles of evidence. It seems psychologically possible genuinely to have this doubt. These skeptics cannot be dismissed in Russell's laconic way.

If there are necessary principles of evidence, their truth might be evident to us a priori. This is certainly what Mackie hopes with respect to his indifference principle, and what might reasonably be hoped for the principle that what best explains one's data is credible: *if* these are true at all, it would be a reasonable ambition to make their truth manifest a priori. But we have learned from Russell's attack on necessary principles that this will not quell every form of skepticism. To the extent that one of skeptical leanings will accept a necessary principle of



evidence, one will so interpret it that it remains an open question whether there is any real value in believing what is credible. For, the skeptic will complain, it is an open question whether having credible beliefs will guarantee even a preponderance of true beliefs. This form of skepticism, I said in section 3, was one we would expect to be associated with a realist metaphysics, with the view that how the world is can totally outrun our powers of discovery.

Let us compare this with the form of skepticism that lurks close to the surface of *Human Knowledge*. This consists simply in doubting whether the principles of evidence that we in fact use, principles identified in accomplishing what I have called the descriptive task, are true; equivalently, in doubting whether any hypothesis is credible. But what does being credible amount to here? By the contingency thesis, credibility will be connected to truth: having only credible beliefs will ensure having a preponderance of true ones. This skeptic has to accept that *if* there were any credible beliefs, his skepticism would be at an end, at least with respect to those beliefs.

It is apparent that what is in effect the same skepticism can emerge in two different forms. Suppose we have a truth-yielding conception of credibility, as could issue, and in Russell's system does issue, from contingent principles of evidence: a conception, that is, according to which believing a preponderance of truths is constitutive of having none but credible beliefs. No skepticism can arise about whether the beliefs upon which any such true principles confer "credibility" deserve to be believed. But there is room for skepticism about whether any such principles are true. Now suppose we have a conception of credibility that is not, constitutively, truth yielding, a conception that, Russell has shown, is forced on us if we have necessary principles of evidence. Then there may be no room for skepticism about whether such principles are true, but there is room for skepticism about whether the "credibility" they deliver is anything worth taking seriously. The skeptic will say it is not to be taken seriously, if it is not truth yielding.<sup>20</sup>

The underlying skepticism is essentially the same in each case, so we can choose in which form we will confront it. Russell made credibility truth yielding and is faced with the skeptic who doubts whether any principles of evidence are true. There is no explicit confrontation, since Russell thinks, wrongly, that he has already dismissed skepticism. But Russell makes it plain that the question of whether we know the postulates is problematic.

There is the difficulty that there is, *prima facie*, little reason to suppose these principles [of evidence] true, and still less to suppose them *known* to be true. (p. 439)

We have seen that he hopes to overcome the difficulty by using a very modest conception of knowledge, one that, as I have said, does not even involve belief, and makes the knowledge in question of a kind with animal expectation. While

it is an important fact that there is a continuity between animal expectation and more explicit and conceptual predictions, it is not one that can carry any weight against the skeptic. No doubt there is a connection between a way of forming expectations being evolutionarily adaptive, and it being one that yields a preponderance of truths. Skeptics can accept this. What they will doubt is whether ways of forming expectations or principles of evidence, which have been truth yielding up to now, will continue to be so. Perhaps they must be if we are to survive. But then the doubt is whether we will.

In section 3, I said that the skepticism that addresses necessary principles of evidence is only what one should expect within a realist perspective. In this section I have said that it is really the same skepticism that addresses contingent principles of evidence. The claims taken together entail that this latter skepticism, too, is no less than what a realist should expect. And this is, I think, the correct conclusion. If the course of nature is independent of our knowledge, or possibility of knowledge, then there is a possibility, which the skeptic will argue to be an epistemic one, that principles that have been truth yielding up to now will cease to be so.

The equivalence of these forms of skepticism I think establishes something else: that there is no substantial issue over whether or not the contingency thesis is correct. Necessary principles will not yield all we need because of skepticism about whether they are truth yielding. Contingent principles will not yield all we need because of skepticism about whether they are true. It does not matter whether we formulate principles of evidence as necessary, if true at all, and then confront the challenge of skepticism in the first form; or whether we formulate principles of evidence as contingent, if true at all, and then confront the challenge of skepticism in the second form.

In the formulation Russell prefers, he sees that meeting the skeptical challenge head-on involves showing that we have knowledge of contingent principles of evidence that is not derived from experience. He also sees that it is no easier to meet the challenge in its other form, as addressed to necessary principles of evidence. We can, indeed, criticize him for thinking that this possibility of skepticism refutes the necessity of principles of evidence, since it tells as much against the contingency thesis. And it has to be admitted that Russell does not succeed, or even seriously engage, in showing that we have the knowledge we need to refute the skeptic. But success in this project is something that, I suggest, no realist has achieved.<sup>21</sup>

#### Notes

1. All page references in what follows are to this book, unless otherwise indicated. American readers are warned that the pagination of this British edition differs from that of American editions.

2. Page 422 (part IV, chapter 7). Russell had drawn attention to this problem of induction, much later made famous by Nelson Goodman in his *Fact, Fiction and Forecast*, 2nd ed. (Indianapolis: Bobbs-Merrill, 1965), as early as 1912: "On the Notion of Cause," *Proceedings of the Aristotelian*

*Society*, 13 (1912/13), reprinted in Russell's *Mysticism and Logic* (1918, paperback edition, 1963), at pp. 147–49. I interpret Russell's postulates of scientific inference as an attempt to overcome the problem. They do not succeed, for they invoke an unrestricted notion of similarity—one that does not exclude the similarity possessed by two *grue* things, one of which is green, the other blue. But this problem is not my concern here.

3. At least, this could be argued. The opposing case is that the belief would still be credible, though false, if the subject were the unwitting victim of an illusion.

4. Compare also: "If we are justified in believing that all men are mortal, that must be because, as a general principle, certain kinds of particular facts are evidence of general laws" (p. 354).

5. A. J. Ayer used this point in an attempt to discredit necessary principles of evidence: "The Concept of Probability as a Logical Relation," in Stephan Körner (ed.), *Observation and Interpretation* (New York: Dover Press, 1957). The strongest form of the objection is this: why strive officiously to increase the quantity and variety of one's data? The natural answer is: because this will make it more likely that one will attain the truth. Ayer's claim that this answer is not available to one who holds that principles of evidence are necessary (that there are necessary 'probability'-relations) parallels Russell's attack on the contingency thesis, discussed in this section.

6. The ontology of *Human Knowledge* is founded on events. This postulate is Russell's transposition into his favored ontology of the traditional doctrine that there exist substances.

7. More exactly, it is a *form* of a principle of evidence. Further work should provide an intrinsic specification of a kind, *K*, of series, and a type, *T*, of property that can be inferred, which would make the following principle of evidence (in the strict sense) true: "If the total data entail that one or two things belong to a *K*-series and have a *T*-type property,  $\Phi$ , and nothing in the data counts against the hypothesis that all members of the series have  $\Phi$ , then this hypothesis is rationally credible." (The qualification "nothing in the data counts against the hypothesis . . ." might seem to introduce a circularity. However, there need not be anything vicious here. A relational predicate "counts against" could be defined in terms of some set of principles of evidence. It *could* be quite noncircular [depending on the content of the other principles] whether or not any principles in a given set tell against a hypothesis. There must certainly be a question, not to be pursued here, whether any such set of principles would be plausible.)

8. Henceforth I shall usually abstract from complications to do with totality of data. See note 7 above.

9. There can be empirical evidence (e.g., authority) even for an a priori necessary truth. Examples of necessary truths that are not knowable a priori are given by Saul Kripke, "Naming and Necessity," in G. Harman and D. Davidson (eds.), *Semantics of Natural Language* (Dordrecht: Reidel, 1972).

10. For example, J. L. Mackie suggests that a similar principle is "logical or quasi-logical," and so, presumably, necessary. See his "A Defence of Induction," in G. F. Macdonald (ed.), *Perception and Identity: Essays Presented to A. J. Ayer* (London: Macmillan, 1979), pp. 113–28, at p. 118–19.

11. Whatever the inadequacies of such principle, it is of a kind with Russell's postulates. Plausible candidates for necessary principles of evidence would include Mackie's indifference principle (see note 10) and the principle that a hypothesis can acquire credibility by explaining the data.

It is surprising that Russell, in *Human Knowledge*, paid so little attention, indeed no attention at all, to the concept of explanation. This bypasses an interesting possibility: perhaps the fundamental kind of nondeductive reasoning is inference to the best explanation. Then the restriction on the principle of induction for which Russell was looking might be this: extrapolation of regularities is justified only when it follows from an explanation of them. This idea was briefly discussed in the original version of this essay, as presented at the Conference on the Philosophy of Bertrand Russell (Minnesota, 1982). It has been excellently presented and defended by John Foster: "Induction, Explanation and Natural Necessity," *Proceedings of the Aristotelian Society*, 82 (1982/83), pp. 87–101.

12. Strictly speaking, the conclusion of an application of a principle of evidence has the form: it is rationally credible that *H*. However, I shall in this discussion speak of *H* itself as the conclusion. It is plainly the falsehood of the propositions upon which credibility is conferred that Russell takes to be objectionable.

13. More exactly, after one more non- $\Psi \Phi$  has been observed.

14. From p. 420. See p. 207, this essay.

15. *Scientia*, 4 (1914); reprinted in *Mysticism and Logic* (1917).

16. "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" (p. 506).

17. "What the Tortoise Said to Achilles," *Mind* 4, (1895), pp. 278-80.

18. Let us use " $K_x$ " as the epistemic operator (knowledge or credibility, as preferred), with the  $x$  subscript representing a relativization to a person. Suppose  $P$  is: for all  $x$ , if  $D$  is  $x$ 's total data,  $K_x H$ . Suppose  $D$  are  $\alpha$ 's total data. Then  $K_\alpha H$ . Iteration requires that if  $K_\alpha H$ , then  $K_\alpha K_\alpha H$ . This cuts both ways: if you know that  $K_\alpha H$  you can use it to establish  $K_\alpha K_\alpha H$ ; if  $K_\alpha K_\alpha H$  appears doubtful, it casts doubt on  $K_\alpha H$ . If  $K_\alpha P$ , one could use this plus  $K_\alpha$  ( $D$  are  $\alpha$ 's total data) to establish  $K_\alpha K_\alpha H$ .

19. Page 446 (quoted in section 2). Cf. p. 387: "If I say that an inductive argument makes a conclusion probable, I mean that it is one of a class of arguments most of which have conclusions that are true."

20. P. F. Strawson, in *Introduction to Logical Theory* (London: Methuen, 1953), part II, chapter 9, failed to give this point due weight. It may be conceptually true that it is rational to reason in accordance with accepted standards of inductive argument. But this does not knock out skeptics who ask why one ought to be rational. They will accept the value of rationality only if it can be shown to make true belief more likely (in a frequency sense) than false belief.

21. I am grateful to the editors of this volume for their comments on an earlier draft.