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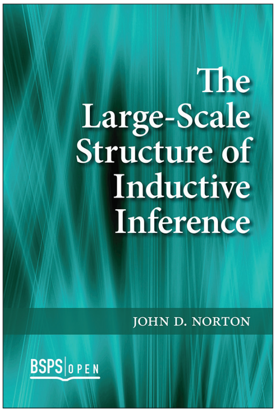

THE LARGE-SCALE STRUCTURE OF INDUCTIVE INFERENCE

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The Problem of Induction

1. Synopsis

Since the problem of induction is so widely known, I expect that many readers will want a simple summary of the main claims instead of the more usual orienting introduction. This synopsis is for those readers.[1]

1.1 The Traditional Problem

I take the problem of induction here to be a specific difficulty in any logic of inductive inference, where "inference" is understood to be a mind- and belief-independent relation of logical support for propositions. Logics prone to the problem are based on universal rules of induction. Traditionally, the rule is enumerative induction: we are authorized to infer from the proposition that some cases bear a property to the proposition that all cases do. Other rules might be abductive: we are authorized to infer to the best explanation or the supposition that relations of inductive support are numerical and conform to the probability calculus.

The problem resides in a short and sharp demonstration that no inductive rule can be justified. The demonstration uses either a circularity or a regress. The rule of enumerative induction itself is justified by some version of that rule: enumerative induction has worked, so we should expect it to continue to work. Hence, its justification is circular. If we consider other rules of inductive inference, then we encounter a similar circularity, if the rule is used to justify itself. Alternatively, the rule might be justified by applying a second rule, and that second rule is justified by a third, and so on in an

1 My thanks to James Norton and Anil Gupta for helpful remarks and reactions.

infinite regress. The regress is fanciful since taking just one or two steps is strained and unrealistic.

Probabilistic accounts of inductive support and analyses of the problem do not escape the fundamental difficulty. In turn, there must be some justification for a logic whose basic rule is that inductive relations of support are probabilistic. Chapter 10 (especially Section 10) of *The Material Theory of Induction* (Norton 2021b) argues that all of the standard justifications for this basic rule are circular.

1.2 The Material Dissolution

The material theory of induction dissolves the problem by denying one of its premises. The problem of induction depends essentially on the presupposition that inductive inference is governed by universal rules. The material theory of induction asserts that there are no universal rules of inductive inference. Inductive inferences are warranted by local facts, not rules. With this understanding, the problem of induction can no longer be set up. It is dissolved.

1.3 Attempts to Recreate It in the Material Theory

A common rejoinder to this dissolution is the proposal that there is an analogous problem for the material theory of induction. It derives from the circumstance that background facts can only warrant an inductive inference if they are true. Thus, they should also be warranted, and the inferences that warranted them must also be warranted. Somehow, lurking in this circumstance, there is supposed to be a regress or circularity as devastating as the original problem of induction. Following are three versions of the problem supposed.

Regress end: Each inductive inference requires a warranting fact of greater generality than the conclusion. The resulting succession of warranting inductive inferences requires a sequence of warranting facts of increasing generality that admits no benign termination.

Regress start: Inductive inference cannot get started: any inductive inference that attempts to go beyond some small, given set of particular propositions requires an unavailable warranting fact of greater generality outside the given set.

Circularity: These successive warranting inferences will eventually form circles of large or small extent. They are supposed to be as harmful as those of the original circularity in the problem of induction.

1.4 Why the Attempts Fail

In earlier chapters, I described the large-scale structure of relations of inductive support in science afforded by the material theory of induction. Briefly, in that theory, inductive inferences are warranted by facts that in turn are supported inductively, those inductive inferences are warranted by further facts, and so on. What results is the massively entangled structure of inductive support relations of a mature science. This structure does not respect any hierarchy of generality. Relations of support routinely cross over one another. It follows that tracing back successively the facts that support some nominated inference leads to a journey through the propositions of the science. There are many forks in the journey's path since its extent grows rapidly and soon might embrace much of science. There is no inexorable and unsustainable ascent to warranting propositions of ever greater generality. Hence, the supposition of *regress end* fails.

This massively entangled structure can be created by hypothesizing provisionally propositions needed to warrant some initial inductive inference. The provisional character of the hypotheses must be discharged by further investigations that provide inductive support for them. This mechanism makes warranting hypotheses of greater generality available when the inductive project of some science is initiated. Hence, the supposition of *regress start* fails.

There are circularities both large and small in this massively entangled structure of relations of inductive support. However, as I argued in Chapter 3, we cannot automatically assume that the mere presence of circularities is harmful. There are benign circularities throughout science. One must establish by positive argumentation that the circularities here are harmful. These harms arise in two ways: as a contradiction of a vicious circularity or as an underdetermination. If either one arises, then it is eliminated by routine adjustments in the science. In place of self-defeating circularities, all that we find in the entangled structure is how one result in a mature science is supported by others, those by others still, and so on. The exercise merely recapitulates, over and over, ordinary relations of inductive support in mature sciences. Hence, the supposition of *circularity* fails.

1.5 Local versus Distributed Justifications of Inductive Inference

The problem of justifying inductive inference has a different character according to whether inductive inference is conceived as warranted by universal rules or by material facts.

If inductive inferences are warranted by universal rules, then the project of justifying them is reduced to that of justifying those few universal rules. All attention is devoted to a small sector of the sciences in which the inductive power is localized. We learn from the endurance of the traditional problem of induction that this localized version of the problem is intractable.

If inductive inferences are warranted by facts, then the justification for inductive inferences is not localized. It is distributed over all of the sciences. In a mature science, the justification for some chosen inductive inference lies in the applicable warranting facts. It is an unproblematic application of the material theory to a particular case. This is true for *every* inductive inference in the mature science, and *that is all* there is to the justification for induction, understood materially. The totality of the justification for inductive inference lies in the accumulation of many such unproblematic justifications and thus itself is unproblematic. When the justification is so distributed, the difficulty is reversed. Efforts to set up the problem of induction fail repeatedly.

2. Introduction

The synopsis above is merely a sketch of the analysis to be developed in greater detail in this chapter. My hope is that readers who might be unsatisfied by its brevity will be satisfied by the lengthier analysis below. Its first step is a more precise statement of the original problem. Although the problem of induction is widely recognized, I have no confidence that we all address the same problem. Before a claim of a dissolution of the problem of induction can be sustained, the problem itself must be clearly delineated. That delineation is the task that I will undertake in Sections 3–10 of this chapter. The task is largely historical, and readers who are confident that they know the history might want to skip ahead to Section 10.

Since inductive inference traditionally has been regarded as generally troublesome, in Section 3 I will seek to sweep away some preliminary distractions that might be taken mistakenly to be the problem of induction. In recalling a collection of what I call "inductive anxieties," the section identifies what the problem of induction is not. It is not, for example, the problem that enumerative induction is capricious. An inference from some *A*s are *B* to all *A*s are *B* sometimes can be sustained by only a few cases of *A*s that are *B*, or it can fail to be sustained even by many cases.

In Section 4, I review David Hume's own presentation of the celebrated argument. It was a masterpiece of philosophical writing, still justly admired today. His argument was narrower than the version that modern authors have taken from his analysis: Hume limited all inductive inferences to causal inferences. And it was broader since he posed the problem largely in psychological terms. He characterized inferences as mental processes, as the "operation of thought." In his celebrated fork, Hume divided all such operations as concerning relations of ideas or matters of fact. Neither could justify inductive inferences about the future, he urged. The first cannot since we can imagine it failing. The second cannot since it requires that we presume in advance the very thing to be justified, that the future will resemble the past.

In Sections 5 and 6, I review the early reception of Hume's analysis. After an initial response, notably from Immanuel Kant and even possibly Thomas Bayes, the analysis faded and merited only passing mention in nineteenth-century discussions of induction. The term "the problem of induction" did not univocally have its modern meaning. Rather, it was a marker for more general inductive anxieties. For Mill, it denoted the capriciousness of inductive inference. In Section 7, I review the twentieth-century revival of Hume's problem, first in the writing of Bertrand Russell and then, with greater focus, in that of Hans Reichenbach and his student Wesley Salmon. They advocated a "circularity" version, reminiscent of Hume's own. Briefly, inductive inference, now understood in Salmon's formulation as any form of ampliative inference, cannot be justified by deduction, since then it would not be inductive, and it cannot be justified inductively, for that would be circular. In Section 8, I recall the "regress" version, delineated most thoroughly by Karl Popper. Instead of the circularity of a rule of inductive inference justifying itself, Popper imagined a rule of inductive inference being justified by another rule, and that by another rule, and so on in an unsustainable infinite regress.

In Section 9, I report that both Russell and Salmon insisted that their modern version of the problem of induction drops the psychological clothing that Hume gave it. The problem is purely one of inductive logic, which pertains to relations with propositions, independent of our thoughts and beliefs. Although modern epistemologists run together logical inference and mental operations, I was pleased to find that this rarely caused confusion. The exception is noted in Section 10, where I review failed attempts to argue that an externalist epistemology of beliefs can solve the problem of induction.

The material dissolution of the problem of induction is presented again in Section 11. In Sections 12 and 13, I respond to the concern that the harmful regresses and circularities of the problem of induction reappear in the tangle of relations of inductive support for the material theory of induction. I argue in Section 12 that the regress of the problem of induction is already fanciful and dubious in its first steps, whereas that of the material theory is merely the recapitulation of ordinary relations of inductive support in familiar science. I argue in Section 13 that the circularities of the problem of induction are harmful since they leave its rules of induction indeterminate. Drawing from the analyses of Chapters 3 and 4, I argue that the circularities of the material theory do not create analogous problems of indeterminacy.

Elliott Sober and Samir Okasha have given responses to the problem of induction that are close to this material dissolution. I review their work briefly in Section 14. Since I claim that there is no problem for the material theory in justifying inductive inference, in Section 15 I give a short summary of the character of the positive justification.

Finally, the present material dissolution of the problem of induction appeared in its earliest form in my first paper on the material theory of induction (Norton 2003). It has attracted some small though continuing critical attention. This attention has been stimulating and led to refinements of the material dissolution. In Section 16, I review the critical reception of the material dissolution in the literature and show how the refinements respond to and answer the negative criticism. Section 17 is a short conclusion.

3. What the Modern Problem of Induction Is Not: Inductive Anxiety

The very idea of inductive inference has been a long-standing target of hesitation and vilification. The dissolution of the problem of induction advocated here is not designed to address all hesitations about induction. To preclude confusion, in this section I report two of these other hesitations. One is simply the observation that inductive inference is not deductive inference and thus must admit the possibility of failure. The second is that a particular form of induction, enumerative induction, is capricious. Sometimes it works well.

Sometimes it does not, and then it encourages ill-advised hastiness. Beyond these two identifiable hesitations, for many, induction is surrounded by an unfocused but nonetheless menacing miasma. In it, induction simply is a problem. I will call the totality of these hesitations "inductive anxiety."

The first hesitation already has clear expression in the ancient tradition of skepticism. As part of his broadly spread critique of all forms of justification, the skeptic Sextus Empiricus himself gave a terse statement that still serves us well today:

> It is easy, I think, to reject the method of induction. For since by way of it they want to make universals convincing on the basis of particulars, they will do this by surveying either all the particulars or some of them. But if some, the induction will be infirm, it being possible that some of the particulars omitted in the induction should be contrary to the universal; and if all, they will labour at an impossible task, since the particulars are infinite and indeterminate. Thus in either case it results, I think, that induction totters. (Annas and Barnes 2000, 123)

Earlier in his text, Sextus Empiricus gives a colorful illustration of how induction totters: "Since most animals move their lower jaw but the crocodile alone moves its upper jaw, the proposition 'Every animal moves its lower jaw' is not true" (120).

We need not linger over this first hesitation. It is constitutive of (ampliative) inductive inference that it can sometimes fail. That fact does not impugn its utility as long as the inferences are secure enough that their failures are tolerably rare. To abandon inductive inference entirely would destroy science, all of whose major results are supported inductively.[2]

For the second hesitation, Mill, in his monumental *System of Logic*, recounts several inductive inferences, some of which proceed securely from a few particulars, whereas others are never judged secure. They lead to a synoptic lament of the capriciousness of induction:

2 Popper's ([1959] 2002) attempt to account for scientific practice solely with deductive inference fails. Salmon (1981) has shown that close adherence to Popper's strictures precludes science from making predictions.

> Why is a single instance, in some cases, sufficient for a complete induction, while in others, myriads of concurring instances, without a single exception known or presumed, go such a very little way toward establishing a universal proposition? Whoever can answer this question knows more of the philosophy of logic than the wisest of the ancients, and has solved the problem of induction. (Mill 1882, 228)

In arguing for the cautious inductive ascent of his preferred method, Francis Bacon provided a celebrated riposte, which seems to be a combination of both of the hesitations listed above:

> The induction which proceeds by simple enumeration is puerile, leads to uncertain conclusions, and is exposed to danger from one contradictory instance, deciding generally from too small a number of facts, and those only the most obvious. (Bacon [1620] 1902, 83)

This second hesitation also need not detain us. Many accounts of inductive inference have taken up the task of accounting for why enumerative induction works when it does and why it fails when it does. This was explicitly the task of Harman's (1965) paper in which the term "inference to the best explanation" was introduced. My material account of inductive inference in Chapter 1 of *The Material Theory of Induction* (Norton 2021b) identifies the warrant for this form of inductive inference in background facts. Generalizations are warranted or not according to whether these background facts are favorable or not. No doubt a Bayesian will find some combination of prior probabilities and likelihoods to fit the expected behavior of even the most capricious of inductive generalizations.

For further details of the troubled history of enumerative induction and a compilation of striking counterexamples mentioned in the traditional literature, see Norton (2010).

4. Hume's Critique

Hume's celebrated critique of inductive inference elevated these traditional anxieties about induction from answerable concerns to what became the model of a recalcitrant philosophical problem in the twentieth century. His critique needs some refinement before we recover the modern version of the problem of induction. Two refinements are notable.

First, Hume restricted all ampliative, nondemonstrative inferences to those mediated by relations of cause and effect:

> All reasonings concerning matter of fact seem to be founded on the relation of *Cause and Effect*. By means of that relation alone we can go beyond the evidence of our memory and senses. (1777a, 26; Hume's emphasis)

This restriction needs to be loosened.

Second, Hume did not separate cleanly two things that should be kept separate. First are thoughts, beliefs, and mental processes, such as is properly the subject of a theory of mental action. They are distinct from logical relations among propositions, such as is the subject of an abstract logic, formulated independently of thoughts and beliefs. For example, Hume's fork, the celebrated distinction of "Relations of Ideas" and "Matters of Fact," is introduced in terms of mental processes. The first "Relations of Ideas" are discoverable, Hume insists (1777a, 25), "by the mere operation of thought, without dependence on what is anywhere existent in the universe." This possibility is contrasted with a "Matter of Fact" whose contrary (negation) is possible. That is, "it can never imply a contradiction, and is conceived by the mind with the same facility and distinctness, as if ever so conformable to reality" (25). Elsewhere, however, his language could easily be mistaken by the unwary as conforming to an analysis of purely logical relations among propositions. On the supposition that present regularities might fail in the future, he asks "what logic, what process of argument secures you against this supposition?" (38). I will urge below that the distinctive Humean problem of induction resides in the inductive logic and can be formulated only indirectly in terms of mental processes.

With these complications noted, we can follow Hume's development of the problem.[3] First, Hume affirms that demonstrative reasoning cannot give us knowledge of these relations of cause and effect:

3 Comparable arguments can also be found more tersely in Hume's earlier *Treatise* (1739, 89–90).

> I shall venture to affirm, as a general proposition, which admits of no exception, that the knowledge of this relation is not, in any instance, attained by reasonings *a priori*, but arises entirely from experience, when we find that any particular objects are constantly conjoined with each other. (1777a, 27)

His argument is based on the immediately following claim:

> Let an object be presented to a man of ever so strong natural reason and abilities; if that object be entirely new to him, he will not be able, by the most accurate examination of its sensible qualities, to discover any of its causes or effects. (27)

The claim is illustrated by examples (27–28) that, Hume asserts, outstrip demonstrative reasoning. He imagines Adam, presumably new to the world and innocent of experiences of it. Adam cannot infer that water suffocates from its fluidity and transparency or that fire consumes from its heat and warmth. Someone innocent of natural philosophy could not infer that polished marble blocks will adhere tightly, that gunpowder is explosive, that lodestones attract, and more. An example earlier in the text, we shall see, reappears later in the text:

> *That the sun will not rise to-morrow* is no less intelligible a proposition, and implies no more contradiction than the affirmation, *that it will rise*. (25–26; Hume's emphasis)

Hume then looks for other possibilities for arriving at knowledge of cause and effect. There is only one candidate, "moral reasoning," for he recalls his fork:

> All reasonings may be divided into two kinds, namely, demonstrative reasoning, or that concerning relations of ideas, and moral reasoning, or that concerning matter of fact and existence. (1777a, 35)

Yet, he continues, moral reasoning cannot provide a firm basis for such knowledge. He justifies this failure by identifying a circularity within efforts to use moral reasoning for this purpose:

> We have said that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition that the future will be conformable to the past. To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question. (35–36)

Since this is the celebrated circularity upon which the modern problem is based, we can pause for another trenchant statement of it:

> It is impossible, therefore, that any arguments from experience can prove this resemblance of the past to the future; since all these arguments are founded on the supposition of that resemblance. (38)

5. The Reception

While Hume fretted that his earlier *Treatise* (1739) fell "dead-born from the press" (1777b, 8), there was still some fairly immediate and noteworthy reaction. It had a profound impact on Kant ([1783] 1909, 7), who famously credited Hume for "interrupt[ing] my dogmatic slumber." Hume's contemporary, Thomas Reid, mounted efforts to refute Hume's skepticism.[4] It is even plausible that his skepticism was one of the motivations for Thomas Bayes' analysis of inverse probabilities. Zabell (1989, 292) notes that the timing of the initiation of Bayes' research on inverse probabilities coincided with Hume's publication in 1748 of his *Enquiry*. After Bayes' death, his result was published and annotated by Richard Price (Bayes 1763). Zabell (1989, 294) and Earman (2002, Section 1) note that much in Price's annotations indicates a response to Hume, even though Hume is not mentioned by name. For example, Price writes (in Bayes 1763, 371–72) that

4 See Landesman and Meeks (2003, Chapter 29).

> Common sense is indeed sufficient to shew us that, from the observation of what has in former instances been the consequence of a certain cause or action, one may make a judgment what is likely to be the consequence of it another time. . . .

Price also considers “the case of a person just brought forth into this . . . world” (409) (reminiscent of Hume’s mention of Adam) who makes successive observations of the sunrise and forms odds of its return. The example is one that Hume had used but to skeptical ends.

6. The Nineteenth-Century Hiatus

In the nineteenth century, any recognition that Hume might have received for identifying the problem of induction faded. He was instead generally tolerated as a troublesome skeptic concerning topics such as causation and miracles. His analysis was not lauded then, unlike today, as the revered *locus classicus* for the modern problem of induction. In that century, the phrase “the problem of induction” appeared frequently. However, its focus was diffuse, and it appeared mostly to designate some version of the “inductive anxieties” sketched in Section 3.

Whatever role Hume’s critique might have had in the initiation of Bayes’ work on inverse probabilities, there is little trace of it in subsequent work. Laplace’s development of the rule of succession in his *Essay* (1814), sketched here in Chapter 1, used Hume’s example of successive sunrises but made no mention of Hume. The *Essay* includes an entire chapter (1902, Chapter 17) on induction and similar ampliative inferences. It recounts some history of such inferences, including mentions of the English writers Newton and Bacon but not the Scottish writer Hume.

Perhaps it is unsurprising that logic texts of the nineteenth century make scant mention of Hume’s critique. Their charter is to delineate the structure of the logics, not to rehearse skeptical assaults against them. Kirwan’s (1807, 231) early logic treatise does cite Hume but to dispute his assertion that chance is the absence of a cause. Munro’s (1850, 233–340) *Manual of Logic* decrees that induction is material and thus “extralogical” insofar as the induction is not complete. That means that its premises fail to include all instances of the generalization, so the inference is not deductive. Whately’s *Elements of Logic* (1856) includes a lengthy chapter on induction (Book 4, Chapter 1) and struggles with many hesitations but never clearly articulates Hume's argument or mentions Hume in the context of induction. Creighton's *An Introductory Logic* (1898) has a section entitled "The Problem of Induction" (Chapter 13, Section 47). However, the term "problem" is less the identification of a difficulty than the setting of a task: how are we to pass from chaotic experience to scientific knowledge?

As late as Schiller's (1912) discussion of formal logic, the phrase "the problem of induction" did not have its modern meaning. The work has a chapter entitled "The Problem of Induction" (Chapter 17). The problem identified is the difficulty of determining the truth of premises used in deductive syllogisms. Hume's concern appears only briefly some eight pages into the meandering chapter as the unanswered question "how do we know that the future will resemble the past?" (239).

One might have expected more from W. Stanley Jevons, notable for his nineteenth-century writing on scientific methodology. His two logic texts (1888, 1902) make no mention of Hume or any problem of induction, although both discuss induction extensively. His major work of methodology, *Principles of Science* (1874), similarly covers induction extensively and advocates for a Bayesian inverse approach. It too has no mention of Hume or any trace of the possibility that Bayes himself might have been motivated by Hume's challenge.

John Stuart Mill might have been the preeminent writer of his age on scientific methodology. We saw in Section 3 that he labeled the capriciousness of inductive inference as "the problem of induction" and declared hyperbolically that to solve it is to "know more of the philosophy of logic than the wisest of the ancients."

The third book of the six forming his *System of Logic* is devoted to induction. In it, Mill presents his methods, whose content remained a core of presentations of scientific methodology into the mid-twentieth century. Buried in this third book among its twenty-five chapters is Chapter XXI. It addresses what, in effect, is Hume's circularity argument. Its subsidiary treatment indicates that Mill regarded the problem as a minor nuisance, a philosopher's sophistry, that can be dispatched forthwith by his sharp wit. Mill notes (1882, 398) that his inductive methods depend on the law of causality, that every event has an invariable, antecedent cause. We are assured of this law by processes of induction that join those cases in which causation is not yet apparent with those in which it is. The inevitable circularity appears:

> If, then, the processes which bring these cases within the same category with the rest, require that we should assume the universality of the very law which they do not at first sight appear to exemplify, is not this a *petitio principii*? Can we prove a proposition, by an argument which takes it for granted? And if not so proved, on what evidence does it rest? (398)

In stating this Humean circularity, Mill makes no mention of Hume. It is not for lack of knowledge of his work, for Hume's controversial analysis of miracles is discussed at length elsewhere in Mill's *System of Logic*. That Hume's analysis had indirect or even direct influence on Mill, however, is suggested by his distinctively Humean choice of examples:

> It would be absurd to say, that the generalizations arrived at by mankind in the outset of their experience, such as these — food nourishes, fire burns, water drowns — were unworthy of reliance.[5] (401)

Mill's dismissal of the circularity fares as poorly as any that underestimates its gravity. His dismissal allows that we first arrive at the law of causality by a fragile, simple, enumerative induction but that our inductive methods are subsequently reinforced by applying the law to itself so that a certainty results:

> The law of cause and effect, being thus certain, is capable of imparting its certainty to all other inductive propositions which can be deduced from it. . . . And hence we are justified in the seeming inconsistency, of holding induction by simple enumeration to be good for proving this general truth, the foundation of scientific induction, and yet refusing to rely on it for any of the narrower inductions. (403)

Mill has staked here the entirety of his inductive enterprise on the certainty of the law of cause and effect, which in his writing amounts to a principle of determinism. The irony, of course, is that this certainty was about to be falsified by the discovery of quantum theory in the 1920s.

5 That bread nourishes is an example that Hume uses repeatedly in his *Enquiry* (1777a, 28 ff.).

In any case, authors contemporary to Mill were not so easily bluffed. Lachelier devoted Section 2 of his 1871 doctoral dissertation, *Du fondement de l'induction*, to Mill's argument. No matter how artful Mill's analysis, Lachelier concluded that a purely empiricist view like Mill's cannot derive conclusions for the future from the knowledge of the past (1907, 25; translated from Ballard 1960, 13):

> If we see nature as nothing more than a series of impressions without reason and without connection, we can indeed record, or rather undergo, these impressions at the moment they are produced, but we cannot predict them nor even conceive of their production in the future.

Lachelier's own ideas inclined toward a Kantian, rationalistic idealism, so Lachelier regarded this empiricist failure merely as motivation for his preferred approach. Although Hume's circularity would have provided powerful further direction, Lachelier mentions it but immediately abandons analysis of it (Lachelier 1907, 17; Ballard 1960, 9):

> The principle of induction itself, then, must be the product of an induction . . . (we leave aside the circle suspected to be in this reasoning).

Similarly, the British idealist F.H. Bradley had little interest in induction and any problems that Hume might have found in it. In his *Principles of Logic* (1883, 342), the treatment of inductive inference is deeply buried in the text and passed over dismissively: "[Mill's methods of inductive logic] will not work unless they are supplied with universals. They presuppose in short as their own condition the result they profess alone to produce." Bradley concludes that "we may set down Inductive Logic as a *fiasco*." Although this conclusion is reminiscent of Hume's circularity, Hume is not credited with any insight and is not mentioned by name anywhere in the 534 pages of the text.

Perhaps prominent recognition of Hume's argument has slipped past this sampling of nineteenth-century writing. If his critique had prominence in the nineteenth century, then we would expect it to register in survey writing. In light of this expectation, it is revealing that Thomson's (1887) philosophical dictionary has an entry for "The Problem of Inductive Logic," but it simply defines the problem as the capriciousness of inductive inference by giving the quotation from Mill above in Section 3. This while elsewhere in the dictionary Hume appears copiously as something of a disreputable gadfly. Hume's skeptical nihilism, Thomson reports, "gave . . . offence so serious to the British public" (xxx).

Still more remarkable is that the introduction of twenty-five pages to the 1894 edition of *Enquiry*, written by Lewis Amherst Selby-Bigge in 1893, makes no mention of Hume's charge of circularity concerning inductive inference. Rather, what attracts the editor's attention concerns causation (xv). It is Hume's affirmation "that there is nothing at the bottom of causation except a mental habit of transition or expectation, or, in other words, a 'natural relation.'" Selby-Bigge then turns to other concerns and reports similar skeptical remarks by Hume on the relation of resemblance (xvi).

7. Twentieth-Century Revival: The Circularity Formulation

With the start of the twentieth century, "the problem of induction" was a phrase used variously to represent a variety of inductive anxieties or even just as a caption to introduce a wide-ranging discussion of induction.[6] The phrase did not indicate the short, sharp problem posed by Hume that any justification for a rule of induction must be inductive and thus circular.

Matters soon changed. Russell's *Problems of Philosophy* (1912) gave terse and readily accessible accounts of a series of philosophical problems. The chapter "On Induction" developed a clear and compelling version of Hume's original problem. Although Hume is not mentioned by name, the chapter's Humean inspirations are clear by its use of familiar Humean examples. The running example asks what justifies our belief that the Sun will rise tomorrow. Russell asks, for example,

> Do *any* number of cases of a law being fulfilled in the past afford evidence that it will be fulfilled in the future? If not, it becomes plain that we have no ground whatever for expecting

6 Ernst Cassirer (1910) has a long chapter entitled "On the Problem of Induction" ("Zum Problem der Induktion"). The phrase "the problem of induction" seems to designate no sharply defined difficulty for induction, such as that posed by Hume. Rather, it serves as a general heading under which Cassirer can develop complaints about empiricism and defend Kantian perspectives on induction.

> the sun to rise to-morrow, or for expecting the bread we shall eat at our next meal not to poison us, or for any of the other scarcely conscious expectations that control our daily lives. (96; Russell's emphasis)

The inevitable circularity emerges. Russell develops and refines the circularity until it becomes one of justification for what he calls the "principle of induction" (103). It is expressed in several cautious clauses. Its overall import, however, is that past association of things of sorts A and B make probable that this association will continue. Justification for this principle itself inevitably falls victim to Hume's circularity. The chapter concludes darkly:

> The inductive principle, however, is equally incapable of being *proved* by an appeal to experience. Experience might conceivably confirm the inductive principle as regards the cases that have been already examined; but as regards unexamined cases, it is the inductive principle alone that can justify any inference from what has been examined to what has not been examined. All arguments which, on the basis of experience, argue as to the future or the unexperienced parts of the past or present, assume the inductive principle; hence we can never use experience to prove the inductive principle without begging the question. Thus we must either accept the inductive principle on the ground of its intrinsic evidence, or forgo all justification of our expectations about the future. (106; Russell's emphasis)

Hans Reichenbach proved to be a more tenacious and exacting proponent of the cogency of Hume's critique. In his contribution to the first issue of the new journal *Erkenntnis*, Reichenbach argued on Humean grounds that there can be no justification for probabilistic forms of inductive inference. It is just that we have no choice but to use them:

> There is no other justification for our belief in logic than to point to the fact that we cannot think at all otherwise. We can however give the analogous [justification] for the laws of probability: we cannot do anything else at all other than to believe in the laws of probability. (1930, 187)

The point is soon given an even stronger form:

> It is exactly the same with probabilistic logic [as with deductive logic]; we cannot justify it, but we can affirm that we just cannot think of any alternative. (188)

Reichenbach concluded that

> Our reply, then, to the problem of validity does not consist in an answer to Hume's question. Rather, the attempt to find a logical foundation for probabilistic assertions seeks an impossible goal, comparable to the squaring of the circle. (188)

The idea that we have no choice but to think probabilistically in inductive terms now seems to be unreflective and unimaginative.[7] Perhaps Reichenbach recognized the weakness of this idea, for he shortly replaced the "no choice but" defense of the use of probabilistic induction with a stronger and now celebrated pragmatic argument. In Section 38, "The Problem of Induction," of his *Experience and Prediction* (1938), Reichenbach formulated a "principle of induction" (340). Loosely speaking, it tells us to expect that the observed frequency of some property in a sequence of events will persist at this value, approximately, within error bounds, as the sequence proceeds. Hume, Reichenbach continued, had mounted a most significant challenge to the principle. He summarized it as follows:

> 1. We have no logical demonstration for the validity of inductive inference.
>
> 2. There is no demonstration a posteriori for the inductive inference; any such demonstration would presuppose the very principle which it is to demonstrate.
>
> These two pillars of Hume's criticism of the principle of induction have stood unshaken for two centuries, and I think they will stand as long as there is a scientific philosophy. (342)

7 That seems so especially to me after having written several chapters in *The Material Theory of Induction* (Norton 2021b) that explore calculi of inductive inference that are alternatives to the probability calculus.

Reichenbach then roundly chastised the philosophers and logicians of the nineteenth century for their failure to recognize the gravity of Hume's challenge:

> It is astonishing to see how clear-minded logicians, like John Stuart Mill, or Whewell, or Boole, or Venn, in writing about the problem of induction, disregarded the bearing of Hume's objections; they did not realize that any logic of science remains a failure so long as we have no theory of induction which is not exposed to Hume's criticism. (342)

Reichenbach's *The Theory of Probability* (1949) gave a similar formulation derived from Hume's original. In Section 91, "The Justification of Induction," citing Hume's *Enquiry*, Reichenbach asks Hume's question. What grounds the inference that the same causes will still be followed by the same effects in the future? Following Hume, Reichenbach divides the negative answer into two parts: there can be no deductive justification and no inductive justification:

> 1. The conclusion of the inductive inference cannot be inferred *a priori*, that is, it does not follow with logical necessity from the premises; or, in modern terminology, it is not tautologically implied by the premises. Hume based this result on the fact that we can at least imagine that the same causes will have another effect tomorrow than they had yesterday, though we do not believe it. What is logically impossible cannot be imagined — this psychological criterion was employed by Hume for the establishment of his first thesis.
>
> 2. The conclusion of the inductive inference cannot be inferred *a posteriori*, that is, by an argument from experience. Though it is true that the inductive inference has been successful in past experience, we cannot infer that it will be successful in future experience. The very inference would be an inductive inference, and the argument thus would be circular. Its validity presupposes the principle that it claims to prove. (470)

Reichenbach proceeded in both works to his well-known answer to Hume's problem: we are justified in using induction pragmatically. Although we have no guarantee that it will work, if anything can work, then it will work.

Wesley Salmon, one of Reichenbach's most successful students, continued the Reichenbachian analysis. His *Foundations of Scientific Inference* (1967) gave, in my view, the most incisive development of Hume's objection.[8] Salmon's version of Hume's objection is slightly more general than Reichenbach's version; it proceeds to a systematic and gently ruthless refutation of each escape proposed in the then present literature; it then concludes with Reichenbach's pragmatic answer.

The inductive inferences of earlier formulations of Hume's problem are replaced by Salmon with the considerably more general notion of "ampliative" inference. Such an inference is defined negatively by Salmon (1967, 8) merely as an inference that is not demonstrative —

> . . . an ampliative inference, then, has a conclusion with content not present either explicitly or implicitly in the premises.

Loose as this definition is, Salmon has no difficulty recreating Hume's charge of circularity against it:

> Consider, then, any ampliative inference whatever. . . . We cannot show deductively that this inference will have a true conclusion given true premises. If we could, we would have proved that the conclusion must be true if the premises are. That would make it necessarily truth-preserving, hence, demonstrative. This, in turn, would mean that it was nonampliative, contrary to our hypothesis. Thus, if an ampliative inference could be justified deductively it would not be ampliative. It follows that ampliative inference cannot be justified deductively.
>
> At the same time, we cannot justify any sort of ampliative inference inductively. To do so would require the use of some sort of nondemonstrative inference. But the question at

8 Wes Salmon was highly respected and the kindest senior colleague in my junior years on the faculty of the University of Pittsburgh. I regret that time robbed me of the opportunity to show him my analysis, for his approval would have meant the world to me. Then again, his disapproval would have been devastating.

issue is the justification of nondemonstrative inference, so the procedure would be question begging. Before we can properly employ a nondemonstrative inference in a justifying argument, we must already have justified that nondemonstrative inference.

Hume's position can be summarized succinctly: We cannot justify any kind of ampliative inference. If it could be justified deductively it would not be ampliative. It cannot be justified nondemonstratively because that would be viciously circular. (11)

8. Twentieth-Century Expansion: The Regress Formulation

Explicit notions of induction, when Hume wrote, were limited to some version of generalization. The simplest was the long-standing form, enumerative induction: from some *A*s are *B*, we infer that all are. Bacon's method of tables provided a more sophisticated, if still limited, version of inductive practice. Nonetheless, writing after Bacon, Hume was comfortable reducing all inductive inferences to one simple form: the same causes will continue to have the same effects. With similarly limited conceptions of inductive inference, Russell and Reichenbach[9] worked with comparably simple conceptions of inductive inference, as codified in their respective "principles of induction" sketched above. The simplicity of these conceptions makes it possible for Hume's critique to be expressed in terms of a circularity. There is one simple notion of inductive inference, and the only way to justify it inductively is to apply that notion to itself.

As the twentieth century unfolded, this simple conception of inductive inference ceased to be viable, if ever it was. It became all too clear that there are many forms of ampliative inference in addition to the few considered by Hume, Russell, and Reichenbach. By the start of the twenty-first century, the variety was so great that I found it a challenge to write a survey of accounts

9 I have excluded Salmon's analysis from the list since his analysis is not limited to the narrow conceptions of inductive inference of Russell and Reichenbach. His ampliative inferences include all nondemonstrative inferences. However, his formulation of the problem as one of circularity omits the possibility of an infinite regress.

of inductive inference that would capture and usefully systematize them. My best effort is Norton (2005).

With many such accounts available, the circularity of Russell's and Reichenbach's analyses ceased to be sufficiently expansive. What if their principles of induction are just not justified by applying the principles to themselves? What if they are justified by some *other* form of ampliative inference? Harman's (1965) revival of abductive inference as "inference to the best explanation" was offered explicitly as providing a warrant for enumerative induction. Justifying one form of inductive inference inductively by another does not settle the matter. Now we must ask what inductively justifies this second form, Harman's schema of inference to the best explanation, and when another form of inductive inference is invoked we must ask what justifies that further form.

The resulting succession of justifications for inductive inference schemas either leads back to a schema already used, in which case we have a circularity, or triggers an infinite regress. This last possibility is the "regress" form of the problem of induction.

The earliest clear articulation that I have found of this regress form of the problem of induction comes in Karl Popper's *Logik der Forschung* (1935), translated as *Logic of Scientific Discovery* ([1959] 2002). Popper formulates the problem of induction as the problem of justifying a principle of induction, the fact that authorizes inductive inferences. He dismisses the possibility that such a principle could be analytic or a tautology: that is, a purely logical truth. Rather, it is a proposition whose truth is known from experience by induction. This immediately leads to the infinite regress:

> To justify it [the first principle of induction], we should have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus the attempt to base the principle of induction on experience breaks down, since it must lead to an infinite regress. (5)[10]

10 Popper's *Logik der Forschung* is noted for its decisive rejection of inductive inference. His deeply skeptical view of induction was not so novel in 1935. We have seen that Reichenbach's *Erkenntnis* paper in 1930 abandoned the project of justifying induction on Humean grounds. Popper cites Reichenbach's paper, mentions Reichenbach's endorsement of probabilistic

This is a terse but serviceable formulation of the regress version of the problem. A more developed version can be found in what Popper (2009, preface) describes as drafts and preparatory writings of 1930–33 for *Logik der Forschung*. They were first published in German in 1979 and then in English translation in 2009. In the translation (Book 1, Chapter 3), we find that Popper prefers the regress form of the problem of induction because the circularity form would be open to the objection that the mere assertion of the circularity involves self-reference, which Russell had shown to raise the possibility of vicious circularity. Popper continues:

> The concept of "infinite regression" is not open to these objections, but otherwise it accomplishes the same task, namely that of demonstrating the existence of an impermissible operation.

Popper continues the chapter, slowly developing the infinite regress and eventually providing this summary:

> In this way, a hierarchy of types emerges:
>
> *Natural laws* (these may be understood as statements about singular empirical statements, and as of a higher type than the latter). The induction of a natural law requires a
>
> *First-order principle of induction*, which as a statement about natural laws is of a higher type than the latter; the induction of a first-order principle of induction, in turn, requires a
>
> *Second-order principle of induction*, which as a statement about first-order principles of induction is, in turn, of a higher type than the latter; and so on.
>
> Every universal empirical statement requires a principle of induction of a higher type than the *inductum*, if it is to possess any *a posteriori* validity value at all (either true or false) as an inductum.
>
> Therein consists the infinite regression. (Popper's emphasis)

inferences, but does not note Reichenbach's deep skepticism about justifying probabilistic inferences (5–6).

9. Logic of Induction, Not Epistemology of Belief

We saw above that Hume's formulation of his critique of induction mixed logical and psychological notions. Hume identified deductive necessities as those discoverable by "the mere operation of thought," and contingencies are characterized as freely conceivable by the mind. As a result, his account leaves open whether the problem that he identified arises in inductive logic or in the psychological processes of belief formation. The first context, inductive and deductive logic, is independent of human thoughts and beliefs. It consists of propositions and inferences that arise as relations among propositions. The second context resides within the operation of the mind. Its relata are not propositions but beliefs, and reasoning[11] is a mental process that carries us from some beliefs to the formation of other beliefs.

The modern version of the problem of induction, the version that I wish to address, resides within the first context, the logic of induction, and not within the second context, the epistemology of belief. The problem is formulated in terms of rules governing inductive inferences and what happens when these rules are applied to themselves or to other rules. They are defined within the context of logic. These rules and the resulting problem of induction appear only indirectly in the epistemology of beliefs, after the problem has been formulated in the logical context. It arises in this second context in the specific case in which a reasoner uses these rules to direct reasoning from a belief in some propositions to a belief in others.

It is not possible, as far as I can see, to define the problem of induction within the epistemology of belief without first formulating it in the logical context. There is no problem of induction if a reasoner merely passes from one belief to another. The problem arises only when that passage is authorized by some rule of inductive inference, and we then ask what justifies that rule.

That the problem of induction is best formulated within the logical context is explicitly part of the twentieth-century revival of the problem. Russell makes the point:

11 It is common to describe this mental process as "inference" in the epistemological literature. Here I restrict the term "inference" to the first context, in which it denotes mind- and thought-independent relations over propositions. (This strictly logical operation is often called "implication" in the epistemological literature.)

> Now in dealing with this question we must, to begin with, make an important distinction, without which we should soon become involved in hopeless confusions. Experience has shown us that, hitherto, the frequent repetition of some uniform succession or coexistence has been a *cause* of our expecting the same succession or coexistence on the next occasion. (1912, 96–98; Russell's emphasis)

He continues with some examples. They include the well-known but dark chicken remark.[12] Russell concludes that

> We have therefore to distinguish the fact that past uniformities *cause* expectations as to the future, from the question whether there is any reasonable ground for giving weight to such expectations after the question of their validity has been raised. (98; Russell's emphasis)

Salmon (1967, 6) is similarly explicit. The problem, he stresses, is "a *logical* problem" (Salmon's emphasis). "It is the problem of understanding the logical relationship between evidence and conclusion in logically correct inferences." He then concludes thus:

> The fact that people do or do not use a certain type of inference is irrelevant to its justifiability. Whether people have confidence in the correctness of a certain type of inference has nothing to do with whether such confidence is justified. If we should adopt a logically incorrect method for inferring one fact from others, these facts would not actually constitute evidence for the conclusion we have drawn. The problem of induction is the problem of explicating the very concept of *inductive evidence*. (Salmon's emphasis)

12 "The man who has fed the chicken every day throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful to the chicken" (98).

10. Epistemology Does Not Solve the Problem of Induction

In principle, misidentifying the problem of induction as deriving from the epistemology of belief could be troublesome. After a review of the literature on epistemology that was not especially diligent, my impression is that the danger has not been realized. Although the literature has made no special efforts to separate the two contexts, the failure seems not to have been troublesome. In internalist epistemologies, what justifies a belief is cognitively accessible to the reasoner. When a belief is justified by inductive inference, the reasoner knows it and knows that a rule of inductive inference was used. Thus, the problem that Hume identified can be spelled out in appropriate logical terms. In externalist epistemology, cognizers have no access to what justifies some beliefs. If they include the justifications for reasoning that corresponds to inductive inferences, then Hume's problem cannot be set up. We are unaware by stipulation of what justifies our reasoning and how it effects the justification. It follows that we cannot know whether these external justifications can be applied to themselves or even what it is for these external justifications to be applied to themselves.

There is only one case that I found of a clear confusion of logical and epistemological issues. In a widely known paper,[13] van Cleve (1984) sought to give an externalist solution to the problem of induction. It is evident from the start that the project cannot succeed. The challenge is to provide an explicit justification for inductive inference. Such a thing cannot be supplied by an epistemology in which the means of justification, by definition, are inaccessible to us.[14]

Van Cleve is undeterred. In the briefest sketch, he identifies two related inductive inference schemas:

> *x*% of the *A*'s I have examined were *B*'s.
> Hence, *x*% of *all A*'s are *B*'s.

13 I learned of this paper through correspondence with Job de Grefte.

14 For a critique of the capacity of externalist epistemologies to answer a broad range of skeptical challenges, see Fumerton (1995, Chapter 6). He notes (163, 171) that philosophers do not have the neurophysiological expertise to assess the efficacy of externalist justifications: "If I had wanted to go mucking around in the brain trying to figure out the causal mechanisms that hook up various stimuli with belief, I would have gone into neurophysiology."

and

> Most of the *A*'s I have examined were *B*'s.
> Hence, The majority of *all A*'s are *B*'s. (1984, 555–56; van Cleve's emphasis)

Somehow, through an external process inaccessible to us, we know that these are good inference schemas, and we know how to restrict application of these rules so that grue-like problems are avoided. This schema is then applied to our history of inductive reasoning to form what van Cleve calls "Argument A":

> Most of the inductive inferences I have drawn in the past from true premises have had true conclusions.
> Hence, The majority of *all* inductive inferences with true premises have true conclusions. (557; van Cleve's emphasis)

With the conclusion of Argument A, we have arrived at some form of justification for inductive inference.

This analysis cannot withstand scrutiny. There are two problems. The first problem is that the analysis is entirely too optimistic about the accuracy of our spontaneous human attempts at inductive reasoning. We human reasoners are naturally rather poor at it. Our natural inclinations are toward inductive fallacies.[15] If we could find some way to quantify the "majority of all inductive inferences" in the premise of Argument A, then we would likely find that the premise is false. That we are disposed to infer in some specific way, without any explicit justification for that disposition, is a poor justification for the correctness of the argument form implemented.

Indeed, a strong motivation for modern scientific methodology lies in the need to correct our natural inclination toward inductive fallacies. We see patterns where there is none. We too easily scan some collection of numerical data and come to the wrong conclusion. Too many of us judge a chance remission of some ailment as caused by whatever dubious therapy happened to be tried at that moment. Too many find an occasional cold day a basis for denying our warming climate. These misapprehensions are corrected by explicit statistical analysis. Similarly, we are too easily misled by anecdotal reports to

15 How is it that we survive? Our natural inductive inclinations are toward safety and the exaggeration of threats, not toward accuracy. There is ample redundancy in our interactions with the world, so that our many errors are individually correctible and mostly not fatal.

believe in the efficacy of some faulty treatment. The impulse to believe must be reined in by requiring controlled studies.

One can well imagine that an externalist justification is viable for narrowly specific beliefs, such as "Jones believes he has just seen a mountain-goat," to use Goldman's (1979, 10) example. However, it is much harder to see how such external mechanisms could reliably implant within us the sorts of universal logical schema sought by inductive logicians. Rather, we should expect most of the schemas spontaneously occurring to us to be incorrect. We will need explicit methods, such as those of science, to separate the few good ones from the many bad ones. Since these internal methods decide which schemas we should accept, any real advantage that externalist epistemologies could provide is lost.

The second problem is more serious. If externalism can solve the problem of induction, then we should expect the analysis to display a justified inductive inference schema. A principal consequence of the material theory of induction is that this end is unachievable if the goal is a universal schema of the type offered by van Cleve (1984). Inevitably, the particular schemas displayed by van Cleve, to put it charitably, are incomplete. That most of the *A*s that I have examined are *B*s is quite insufficient to authorize the conclusion that the vastly greater majority of *all A*s are *B*s.

Van Cleve simply avers that "I shall assume that we know how to restrict the predicates involved in these inferences so as to avoid Goodman's paradox about the grue emeralds" (1984, 556). That brash display of wishful thinking only begins to address the troubles that van Cleve has to suppose away. Even without the trickery of grue-ified predicates, inductive inference schemas, such as van Cleve displays, most commonly fail unless the *A*s and the *B*s are chosen very selectively under the guidance of background facts specific to the domain. This is the extended lesson drawn in Chapter 1 of *The Material Theory of Induction* (Norton 2021b). Even then additional facts might be needed. For example, depending on the case, we might need some assurance that the *A*s at issue have been sampled appropriately. That requires further background assumptions, such as the specification of a random sampling protocol.

These two concerns leave little of van Cleve's (1984) analysis intact. With the inductive inference schemas so incompletely specified, we have no assurance that they can be applied to our history of inductive reasoning to recover the core Argument A. And there is little motivation to do so since the premise of Argument A is likely false.

Papineau (1992, Section 2) gives a briefer analysis, similar to that of van Cleve (1984). We carry out an induction, premised on the successes of our past history inductions, to conclude that inductions lead to true conclusions. The correctness of this larger induction is based on the supposed reliability of induction as an inference scheme. It is sufficient here to note that the criticism above of van Cleve's analysis applies equally to it.

De Grefte (2020) has more modest ambitions. He disavows van Cleve's (1984) attempt to justify inductive inference. Rather, he argues that there is no problem of induction for a reliabilist externalist epistemologist:

> My present aim is only to establish that a reliabilist would not be troubled by the problem of induction. And that follows from the fact that reliabilists maintain that reliability is sufficient for justification, and that inductive inference may be reliable even if it is impossible to provide an argument for its inductive validity. We thus do not need to make the controversial assumption that inductive inference is, in fact, reliable. (102)

Here I agree with de Grefte: the modern problem of induction does not arise in a context in which there are no rules of inductive inference. However, he is wrong to conclude from this ". . . that externalist epistemologies are generally able to dissolve the problem of induction" (100). The problem of induction is a problem of inductive logic. It is not solved or dissolved by pointing out that the problem does not arise in another context.

There is a related problem that leaves reliabilist externalist epistemologists in a worse position than inductive logicians. That some epistemic process has been reliable in the past is no guarantee that it will continue to be reliable. Since these processes are invisible to externalists, they cannot even identify the processes justifying beliefs and thus have no means of controlling and assessing them.

11. The Material Dissolution of the Problem of Induction

The material theory of induction dissolves the problem of induction. The reason is simple and has already been given in the synopsis at the start of this chapter: the problem of induction is formulated in terms of universally applicable rules or schemas for inductive inference. There are no such rules or schemas in the material theory. It follows that the problem of induction cannot be set up. That is, there is no problem of induction within the material theory.

The analysis could stop with that. However, a common but mistaken reaction to this dissolution is that it is too easy. Surely a recalcitrant problem like the problem of induction cannot be dispatched so simply. In other failed solutions to the problem, the core difficulty remains but is somehow sufficiently disguised that it is no longer immediately apparent. Any claim of a solution to the problem of induction is then taken as an invitation to dig deeper to expose the trick and defeat the solution. It is the default reaction of philosophers to any claim of a solution to the problem of induction. This understandable intuition, mistaken in this case, directs us to seek a comparably troublesome regress or circularity in the justifications for inductive inferences within the material theory. There are both — regresses and circularities — within the material theory of induction. However, they are benign, unlike their counterparts in theories of induction with universally applicable schemas. Demonstrating this is the goal of the next two sections.

12. Regresses

Consider first the regresses within the material theory of induction. Each inductive inference is warranted by background facts in the applicable domain. If they are to provide a warrant, then they must be facts — that is, truths — so we expect that in turn they are supported by further inductive inferences. And these inductive inferences in turn require further facts to warrant them. And so on. What results is a regress of facts of some sort. However, it is a benign regress that merely recapitulates the mundane relations of inductive support that arise routinely within the sciences. It is unlike those troubling universally applicable inductive inference schemas of the problem of induction.

12.1. In the Traditional Problem

To see this, we begin with the troublesome case. For universally applicable inductive inference schemas, the traditional starting point of the regress is some version of enumerative induction. The regress is already troublesome at the outset, for, as we just saw, schemas of enumerative induction are incomplete. If applied mechanically, then they lead mostly to false conclusions. All too often, when we have some *A*s that are *B*, it is *not* the case that all *A*s are *B*. Hence, the first step of the regress, using another rule of inductive inference to justify the schema, has been set an impossible task. Still, we might follow Harman's (1965) lead and seek to use inference to the best explanation to vindicate enumerative induction. The effect is merely to add another layer of trouble. As argued at some length in Chapters 8 and 9 of *The Material Theory of Induction* (Norton 2021b), the schema of inference to the best explanation itself is incomplete. We have no agreement in the literature on what counts as an explanation, let alone just how to judge which is the best explanation. Indeed, I have argued, a distinctive notion of explanation seems to play no role in the standard examples of inference to the best explanation in science.

The regress cannot stop, however. We press on. How, as a general matter, are we to justify inference to the best explanation? Often explanations require simplifications that intentionally introduce idealizing falsehoods. Explanation and truth need not coincide. Nonetheless, perhaps we can find a third rule to justify this second rule. Might we suppose that the general use of this argument form has passed some sort of severe test so that it is justified by the rule of severe testing? Has the rule of inference to the best explanation been tested severely enough to justify its universal use?

Finally, might we tap instead into the unbridled optimism of Bayesians that their system can account for everything? Might there be a Bayesian vindication of inference to the best explanation, even if we remain unsure of just what an explanation is? Or might a Bayesian vindication succeed for any of the other rules that we might seek to justify in the regress? Whatever the prospects of success here for Bayesian vindications, we have still only postponed the difficulty. We must now ask what justifies the Bayesian system? In Chapters 10 and 11 of *The Material Theory of Induction* (Norton 2021b), I argued that all of the many attempts to justify probabilism are circular. This does halt the regress but at the cost of circularity.

We have explored only a few steps of the regress, and our store of distinct, universally applicable schemas of inductive inference is depleted. The prospect of sustaining an *infinite* sequence of such steps is not just distant but also obviously impossible. Our inferences have become as brittle as glass. We must feign some grasp of the application of inductive inference schemas in all generality, and then pretend to grasp clearly just what it is to apply still further inductive inference schema to them, and then more to them. We rightly judge this infinite regress of rules applied to rules and to rules as fanciful and unsustainable.

12.2. In the Material Theory

The regress of factual warrants in the material theory of induction is different. Where the regress of rules applied to rules is incomplete, speculative, and dubious, the regress of factual warrants is distinctive precisely because of its lack of distinction. It is simply the recapitulation of the grounds given in a mature science for its results. In Chapter 2, I described the nonhierarchical, massively entangled relations of inductive support within a mature science and argued that the totality of these relations is self-supporting. Another example can remind us of just how routine the regress of factual warrants is in a mature science. In Chapter 2, I used the illustration of the impossibility of a perpetual motion machine in the case of the EmDrive.

Consider now the general proposition that a perpetual motion machine of any kind is impossible. Our certainty of its impossibility is warranted by the fact of the conservation of energy. We can now begin the regress of warrants. What supports our confidence in the conservation of energy? I indicated in Chapter 2 that the totality of support for this fact is so immense that it extends well beyond what can be specified here. However, it is sufficient to say a little more to make the key point.

The conservation of energy — then commonly known as the "conservation of forces" — was one of the proud triumphs of mid-nineteenth-century physics. The result derived from the joint achievements of many, including James Joule, Julius Mayer, and Hermann Helmholtz. It was established through the accumulation of many smaller results, for the conservation of energy applies to all physical transformations. What needed to be shown was that, in each physical transformation, where a capacity was lost in some component, it was restored in another, and the restoration was such that a quantitative measure of the capacity was preserved.

When the result was still a scientific novelty, Helmholtz gave a popular lecture in Karlsruhe, sometime in the winter of 1862–63, summarizing its basis. Helmholtz (1885) proceeded methodically through the various transformation processes that contribute to the general result.

> *Simple mechanical processes*, such as bodies moving under gravity. They include the motion of pendula powering clocks, the falling weights that powered such clocks, mills powered by falling water, and the operation of diverse lever and pulley systems.
>
> *Processes involving and powered by elastic bodies*. They include springs and crossbows, along with bodies moved by the expansive powers of heated gases, such as those produced in a gun barrel by exploding gunpowder or the steam within a steam engine.
>
> *The many transformations of heat*. They include its transmission among solids, liquids, and gases and by radiative processes; its latency in phase transitions such as the melting of ice; and its production and absorption in chemical processes. Of great historical importance was the novel recognition that heat and motive power are intertransformable. Motive power can be converted to heat by friction, and heat can be converted to motive power in a heat engine.
>
> *Chemical transformations*. They include all manner of heat-generating, combustion reactions and fermentation reactions that produce pressurized gases.
>
> *Electrical processes*. They include the creation of combustible gases by electrolysis, the use of chemicals to generate an electrical current in the cells of a battery, and the interconvertibility of motive power into electrical currents in electric motors and dynamos. These electric currents can then produce chemical changes or, in resistances, create heat.

As Helmholtz worked his way, step by step, through all of these processes, the same result was recovered over and over: "Thus, whenever the capacity for work of one natural force is destroyed, it is transformed into another kind of activity" (359).

We see here the first steps of the regress of inductive support. Each result claimed by Helmholtz required further support. To recover them, he could indicate a long history of experimental work preceding his work in each of the sciences touched on by his inventory of processes. The best known of them in this context was the experimental work of Joule. He painstakingly measured the exact conversion between heat and motive power, the mechanical equivalent of heat. His was just one of many experiments touching all of the sciences. They include Regnault's painstaking measurements of the physical properties of steam and Faraday's many researches into electrochemistry. Following this path, the regress takes us on a tour of earlier nineteenth-century experimental work in the physical sciences. These next steps of Helmholtz's regress are not limited to experimental work. They also engage with established physical sciences. The conservation results pertaining to the motions of bodies under gravity could be drawn directly from well-established Newtonian mechanics and the conservation of heat itself from results in calorimetry and from what could be preserved of the caloric theory of heat.

Helmholtz's lecture gives an early portrait of the regress of inductive support shortly after the initial recognition of the conservation of energy. The regress continued for decades with ever growing strength. Each item listed in Helmholtz's inventory identified a distinct science: conservative mechanics, the mechanics of fluids, thermodynamics, chemistry, and electrical theory. As each developed, it affirmed the conservation of energy within the processes peculiar to its domain. Might we fear that the mysteries of electricity, magnetism, and radiation harbor a violation of the law of energy conservation? With the perfection of Maxwell's electrodynamics as the century progressed, the conservation of energy was issued as a simple theorem, a deductive consequence of his equations. There were also interactions among the sciences. The joint sciences of electrochemistry and thermochemistry emerged, for example. In each, the conservation of energy was maintained. Overall, the conservation of energy proves to be affirmed multiply in each of the sciences and in many experiments. Its affirmation in one area then provides support for its affirmation in another and conversely.

The law found new strength with the arrival of novel physics in the twentieth century. With Einstein's $E = mc^2$, energy and mass are identified. The law of conservation of mass had figured prominently in Lavoisier's establishment of the oxygen theory of combustion and his tabulation of elements. The law of conservation of mass is now merged with the conservation of energy. Evidential support for one is also evidential support for the other. As relativistic mechanics developed, a similar merger of conservation laws appeared. In the four-dimensional account of special relativity developed by Hermann Minkowski, the laws of conservation of energy and of momentum proved to be a manifestation of a single law of conservation of energy-momentum. The conservation of momentum supports the conservation of energy and conversely. The standard Hilbert space formulation of quantum theory emerged in the late 1920s and early 1930s. It gave energy conservation a special role. Physical systems were routinely represented by conservative Hamiltonian operators whose action on quantum states generates their time evolution. The resulting temporal dynamics then automatically conserves the energy of a system with determinate energy. The success of quantum dynamics depended on the conservation of energy and conversely.

This recounting of the evidential support for energy conservation and the necessary failure of all perpetual motion machines is likely not a moment of great excitement for the reader. It reads like a dull recitation of an introductory chapter in a dreary science text. That, of course, is precisely the point. When we ask what justifies a fact warranting some inference in a mature science, we begin a regress that recounts relations of inductive support upon relations of inductive support. We rapidly find that tracing these relations takes us on a tour of much science, and we find the relations entangled in many mutually reinforcing interactions that give rigidity and strength to the structure. At each moment in our tour, we encounter a piece of ordinary, unremarkable science. What we do not find is what we found in the regress of universal schemas of induction: an accumulation of incompletenesses that terminates in dubious speculation after only a few steps of regress.

The justificatory regress of universal schemas of inductive inference is almost immediately ruinous and presents a severe challenge to any account of such schemas. The regress of inductive support in the material theory of induction is merely the recapitulation of mundane science. It just recalls how science is done.

13. Circularities

To recall a theme stressed repeatedly in this volume, the massive tangle of relations of inductive support in a mature science includes circularities, both large and small in compass. We have just seen several of them. Einstein's $E = mc^2$ merged the conservation of energy and the conservation of mass. Through the mediation of this fact of merger, it now follows that the earlier establishment of the conservation of mass in chemistry provides support for the conservation of energy in physics, and the earlier establishment of the conservation of energy in physics provides support for the conservation of mass in chemistry. Similar relations of mutual support arise for the laws of conservation of energy and conservation of momentum, through the fact that these laws are expressed as a single law of conservation of energy-momentum in the four-dimensional formulation of special relativity.

I argued at some length in Chapter 3 that the mere presence of a circularity in some system is not an automatic condemnation of the system. Many circularities, like the ones just noted, are common in unobjectionable science. Rather, if we are to assert that a circularity is troublesome, then we have a positive obligation to demonstrate that the specific circularity is so. The chapter provided two means for doing this. The most serious case is a vicious circularity. In it, the circular relations lead to a contradiction. The less serious case is a circularity that leaves the structure indeterminate. If that indeterminacy is not transient but ineliminable, then the common resolution is to judge the structures involved as not factual. That is, they can be set conventionally, much as we are free to set our units of measurement.

In the circularity forms of the problem of induction, we seek to use a universal schema of inductive inference to justify itself. This circularity is immediately troublesome, for it forms a tight circle that leaves the schema indeterminate. It is easy to show that there are too many dubious universal schemas of inductive inference that are self-justifying. The trouble is that self-justification is too permissive. Salmon's (1967, 12) preliminary example is of a psychic who makes predictions by gazing into a crystal ball:

> When we question his claim he says, "Wait a moment; I will find out whether the method of crystal gazing is the best method for making predictions." He looks into his crystal ball and announces that future cases of crystal gazing will yield

> predictive success. . . . "By the way, I note by gazing into my crystal ball that the scientific method is now in for a very bad run of luck."

Another of Salmon's examples is a counterinductive rule that is self-justifying. It mimics the familiar attempt to allow inductive inference to be self-justifying. Salmon defines an inductive rule R_3:

> To argue from
>
> *Most instances of A's examined in a wide variety of conditions have been non-B*
>
> to (probably)
>
> *The next A to be encountered will be B*. (15; Salmon's emphasis)

Salmon then takes as a premise that most applications of rule R_3 (*A*) have been unsuccessful (not-*B*). Rule R_3 then assures us that it will be successful on its next application. More formally, Salmon writes that

> R_3 has usually been unsuccessful in the past.
>
> *Hence (probably)*:
>
> R_3 will be successful in the next instance. (15; Salmon's emphasis)

Douven (2017, Section 3.2) provides an amusing variant of Salmon's counter-inductive rule:

> For suppose that some scientific community relied not on abduction but on a rule that we may dub "Inference to the Worst Explanation" (IWE), a rule that sanctions inferring to the worst explanation of the available data. We may safely assume that the use of this rule mostly would lead to the adoption of very unsuccessful theories. Nevertheless, the said community might justify its use of IWE by dint of the following reasoning: "Scientific theories tend to be hugely unsuccessful. These theories were arrived at by application of IWE. That IWE is a

> reliable rule of inference — that is, a rule of inference mostly leading from true premises to true conclusions — is surely the worst explanation of the fact that our theories are so unsuccessful. Hence, by application of IWE, we may conclude that IWE is a reliable rule of inference."

I stressed above that we have a positive obligation to demonstrate that circularity is troublesome. Salmon's and Douven's analyses show just this trouble for the self-justifying schema.

That there is no such demonstration of troublesome circularity in the material theory of induction I argued in Chapters 3 and 4. Contradictions can arise provisionally in the tangle of mutual relations of inductive support of a developing theory. They are merely an indication that we have an error somewhere in our structure. They are routine and provide a helpful guide to finding the error and its subsequent elimination. Indeterminacies can also arise. If they are ineliminable, then we have good reason to conclude that what is left indeterminate is not factual but something that can be set by convention, for we have found something beyond the reach of evidence. Finally, if the indeterminacies admit multiple theories but remain within the reach of evidence, then we find that the resulting competition among those theories is unstable. An advantage for one strengthens it at the expense of the others. Under this instability and the accumulation of further evidence, inductive support is driven to favor just one of the competing theories.

The circularities that arise when universal schemas of inductive inference seek to justify themselves are self-defeating. The circularities of inductive support that arise in the material theory of induction are merely symptoms of a massively interconnected network of relations of inductive support. They are part of what gives strength and rigidity to the evidential support of mature sciences.

14. Sober and Okasha

It would be a surprise if a response to Hume's problem this simple had been entirely overlooked in the literature. As far as I know, there are two older versions of this escape. Neither is complete since each omits at least one key piece, but they have enough for me to characterize them as close to the material dissolution.

Sober (1988) notes that Humean skepticism about our knowledge of the future is equally a problem for historical sciences, such as evolutionary biology, for they try to discern the past from evidence in the present. In these inferences, invocations of simplicity can play a prominent role. Sober, however, understands them materially:

> Whenever observations are said to support a hypothesis, or are said to support one hypothesis better than another, there must be an empirical background theory that mediates this connection. It is important to see that this principle does not evaporate when a scientist cites simplicity as the ground for preferring one hypothesis over another in the light of the data. *Appeal to simplicity is a surrogate for stating an empirical background theory.* (64; Sober's emphasis)

Sober then applies this material understanding of induction to Hume's problem. According to the problem, as Sober recalls it, inductive inference depends on an inductive principle that cannot be justified by reason alone. In place of this failure, he finds a regress:

> What we do find in any articulated inductive argument is a set of empirical assumptions that allow observations to have an evidential bearing on competing hypotheses. These background assumptions may themselves be scrutinized, and further observations and background theory may be offered in their support. When asked to say why we take past observations to support the belief that the sun will rise tomorrow, we answer by citing our well-confirmed theory of planetary motion, not Hume's Principle of the Uniformity of Nature. If challenged to say why we take this scientific theory seriously, we would reply by citing other observations and other background theories as well. (65–66)

All that is needed for this analysis to coincide with the material dissolution is for Sober to affirm a benign termination of the regress. Here he falters. Through an obliquely answered rhetorical question, he concludes that there is no "stage where an empirical belief that is not strictly about the here and now is sufficiently supported by current observations, taken all by themselves" (66).

Such a stage is incompatible with his earlier conclusion that observations can support a hypothesis only relative to a background theory. Sober concludes that

> The thesis that confirmation is a three place relation sustains Hume's skeptical thesis, but not the argument he constructed on its behalf. (66)

Sober's objection to a benign termination to the regress, we can now see, depends on a tacit adherence to the hierarchical structure of relations of inductive support denied in Chapter 2 of this volume. Without it, we are freed from the requirement that a warranting fact must be drawn from somewhere in a later stage of the regress. A benign termination is possible merely using warranting propositions supported elsewhere.[16]

Okasha (2001) recounts the key idea of the material dissolution of the problem of induction in a section headed "IV. No Rules of Induction, No Humean Argument." The section ends thus:

> To conclude, a Humean sceptical argument will only work if our inductive behaviour can be characterized as a process of rule-governed ampliation. There is no necessity that our inductive behaviour can be so characterized. I have offered reasons for thinking that it cannot be. If this is correct, then Hume's argument cannot be converted from a valid one into a sound one, and the threat of inductive scepticism is successfully parried. (324)

Okasha also recognizes that an inductive rule is applicable only if the background factual conditions are hospitable. In the material theory, it is inferred from this circumstance that rules of inductive inference can be applied only locally in suitably hospitable domains. Here, unfortunately, Okasha takes a different course that precludes a full dissolution of the problem of induction. He treats inductive rules as universally applicable and finds this to require us to abandon all rules of inductive inference. That is, he writes,

16 See Okasha (2005) for an account of Sober's analysis and the material dissolution as presented in Norton (2003).

> To use an inductive rule is to assume that the world is arranged in a particular way, as I have stressed. . . . So following any particular inductive rule does seem less than fully rational. It embodies a fixed commitment to the world's being in a certain state; but *qua* empiricists we should undertake such commitments only provisionally, not hold on to them at all costs. (321)

The result is that Okasha must seek some other account of the inductive practices of science. He explores Bayesianism, understood as the dynamics of opinion change, and Popper's deductivist elimination of induction. Hume's problem is escaped but at the cost of denying that science infers inductively.

15. What Justifies Induction in the Material Theory

Showing that there is no problem of induction in the material theory might seem to leave the fundamental question unanswered. What, one might still wonder, justifies the practice of inductive inference, according to the material theory? Although the answer was implicit in the discussion in the previous section, it might be helpful to make it more explicit.

The question can appear to be unanswered if it is accompanied by a false presumption. In asking "What justifies . . . ?" the presumption might be that we can identify a particular thing that does the justifying. That was the sort of answer that Mill tried to give with his principle of uniformity of nature. In the material theory of induction, there is no single identifiable thing that justifies inductive inference. Rather, the justification for inductive inference is distributed over the entirety of the complicated network of relations of inductive support that comprises a mature science. In the early stages of a new science, when these networks are not fully in place, justification might be only partial. At least some of the justificatory work is done by propositions, introduced hypothetically, without themselves having proper support. The goal, as the science develops, is to provide support for each of these hypotheses so that no proposition of the resulting mature science is without inductive support.[17]

Perhaps an analogy will help to illustrate the sufficiency of this answer. The vitalists of the eighteenth and nineteenth centuries sought in vain for

17 This notion of distributed support has already appeared in variant forms in Chapters 2 and 5 of this volume.

the animating spirit that distinguishes living matter from dead matter. As biology advanced into the twentieth century and our knowledge of the details of life processes became increasingly detailed, the futility of the search for this *élan vital* became clear. However, there was no simple answer to the question of what makes something alive. A biologist could examine in great detail any portion of a living organism and find only inanimate chemical and electrochemical processes, even if of great complexity. We can point to no single thing that animates matter. The best — in fact the only — answer to the question of what makes some organism alive is just this: it is no one piece of the organism. Its life derives from the synthesis of all of the many processes of its many parts.

16. Critical Responses to the Material Dissolution

Section 6 of my first paper on the material theory of induction (Norton 2003) described how that theory eluded the problem of induction. I have described in the preface to this volume how this dissolution of the problem of induction generated a critical response out of proportion to its place in the original paper. However, the criticism revealed that I had not developed the details of the dissolution well enough. It needed to be sharpened. Here I will recall that criticism and show how subsequent refinements have responded to it. There were two broad areas of concern, indicated below.

16.1. From Particulars to Generalities

First, I had correctly identified the regress of justifications in the material theory as benign and merely recapitulating ordinary relations of support in standard science. However, I had not identified the nonhierarchical structure of these relations and the role of hypotheses in its erection. Rather, in Norton (2003, Section 6), I merely asserted that the regress is benign and gave some inconsequential speculation on the possibilities for its termination. They included a termination in "brute facts of experience."

Both John Worrall (2010) and Tom Kelly (2010) found this inadequate. As Worrall correctly noted,

> However, if we follow this backward direction, we clearly meet what seems to be an insuperable problem: the accreditational buck has to stop somewhere: it cannot be an infinite chain (or

> rather tree . . .). . . . [W]e know that nodes in the tree must contain, at some stage, universal claims — and so we would still have to account for some initial act (or acts) of generalization. And given that we want each node to be justified, we would seem to be back at the same old problem. (746)

And then

> I am unsure what a "brute fact" of experience is. But presumably brute facts for Norton here had better be singular: if so, then the problem has not been solved since the tree needs to go universal at some point; . . . (747)

Kelly set up his objection by defining *E*:

> . . . [C]onsider that time immediately before we acquired our first piece of inductive knowledge. Let E represent the totality of our knowledge at that moment. (760)

Trouble, Kelly continued, then ensues:

> Suppose that we try to take a first, minimal step beyond E. Again, intuitively, this proposition will be our first piece of inductive knowledge. In that case, we must have recourse to at least one known material postulate. Of course, that material postulate has to be a part of E, since it has to be known, and E represents the totality of our knowledge at the time. . . . My worry is that, given that the only empirical knowledge that one has at that point is observational knowledge and its deductive consequences, there would not be anything suitable around to play the role of material postulate. (761)

In brief, the concern is that we start knowing only particular facts. To extend our knowledge inductively to generalities of vastly greater scope, we need a material postulate of vastly greater scope. By supposition, we have no such fact in our starting point.

This is an objection that needs a response, and I am grateful to Worrall and Kelly for pressing me on it. The response to these worries came in Norton (2014) (and is elaborated in Chapter 2 here). Their objection fails. It neglects the use of hypotheses as a way of extending the inductive reach of evidence well beyond its initially limited scope. We can and routinely do take a first faltering step in inductive inference by hypothesizing the warranting fact needed. This warranting fact can be of generality greater than the facts from which we initially proceed. The key is that its use is provisional. We have a positive obligation to return to the hypothesis and show in subsequent investigations how it is supported inductively. When we succeed, we commonly end up with cogent but massively entangled relations of inductive support. If we do not succeed, then we must concede that the inference has no warrant and should be abandoned.

It is a lesson hard won by authors of philosophy papers that their solutions to problems can be overlooked. Such has happened with works by Schurz (2019) and Schurz and Thorn (2020). They mischaracterize the material approach to induction as a "uniformity account" (Schurz 2019, 17; Schurz and Thorn 2020, 89): that is, an account based on uniformity assumptions. Then they assume that the regress of inductive support depends on a sequence of uniformity assumptions of increasing generality that cannot terminate satisfactorily.[18] Both texts provide instances of such sequences. Readers should be forewarned that these sequences are proposals by Schurz and Thorn and not part of my account. Their supposition is based on a mistaken assumption about how the warranting facts of an inductive inference themselves are to be warranted. The supposition is that the successive warranting facts in this process inexorably must become ever more general. In this way, relations of inductive support are supposed to be adapted to a hierarchy of increasing generality.

That inductive support, materially understood, avoids just such sequences was an important consequence of my identification (Norton 2014, Section 10) of the nonhierarchical structure of relations of inductive support, further elaborated here in Chapter 2. Rather, relations of inductive support cross over one another in a massively entangled structure that respects no such hierarchy of generality. Schurz and Thorn (2020) draw their treatment of the

18 "A closer look at Norton's example shows that the uniformity assumptions that justify inductive inferences become more and more general" (Schurz 2019, 17); ". . . the uniformity assumptions that justify material inductive inferences become unavoidably more and more general" (Schurz and Thorn 2020, 90). Independent of any consideration of the material theory, Bird (1998, 111) characterizes the regress form of the problem of induction in terms of an unsustainable regress of ever more general, justifying facts.

material escape from Norton (2003), supplemented by references to Worrall (2010) and Kelly (2010). They do not cite Norton (2014) and make no accommodation for its assertions. My fuller response to them is in Norton (2021a).

16.2. Logic versus Epistemology

Second, I had not separated questions of inductive logic from those of the epistemology of beliefs, as I have now done in Section 9 of this chapter. That this should have happened in my response (Norton 2014) to Kelly (2010) was almost inevitable since his critique mingled the two throughout. Kelly presented a core claim of the material theory in epistemological terms:

> In what sense are inductive inferences "grounded in" material facts? . . . [W]hat is required is that the person drawing the inference knows (or at least, reasonably believes) that they obtain. (759)

This Kelly soon reinforces as the key supposition that will lead to his objection to the material dissolution:

> . . . Norton's view is that knowledge of the underlying material postulate is what is required: "In order to learn a fact by induction, the material theory says that we must already know a fact, the material postulate that licenses the induction" (2003, 666).[19]
>
> Let us call this commitment of the material theory:
>
> **Prior Knowledge**: in order to learn a fact by induction, one must have prior knowledge of the material fact that licenses the induction. (2010, 760; Kelly's emphasis)

Kelly's narrative here takes a central claim of the material theory of induction from the context of the logic of induction and reconstitutes it as a claim in the epistemology of belief. With this revision, as quoted above, Kelly sets up E: the totality of our knowledge at the moment immediately before we acquire

19 The remark quoted from me ("In order to learn a fact . . . know a fact . . .") reports a consequence of the material logic of induction for the epistemology of belief. The "knowing" is not constitutive of inductive inference relations in the material theory. Kelly mistakenly makes it so.

our first piece of inductive knowledge. He can now pose what appears to be an insurmountably difficult problem. How can we proceed from E to make the first induction to a generalization of vastly greater scope?

Understood as a problem of inductive logic, it is not so formidable. We have some body of particular facts. Which inductive inferences can it support? As I recounted in Chapter 2, once we abandon the unnecessary hierarchical restrictions on applicable material postulates, we find that there is no barrier to them grounding an extensive science with propositions of general scope, as long as the propositions of E themselves are varied enough. We can even recover the inductive structure from a sequence of inductive inferences that employ hypotheses provisionally.[20]

If, however, we conceive E epistemologically, as some sort of exhaustive specification of the beliefs of a fictional primitive human, then now we have posed a new and more difficult problem. We have somehow to imagine the unimaginable. What is it to be such a human, fully grasping many particulars but no generalities? What would such a human do next? Would such a human have any confidence that generalities are somehow within inferential reach? What might motivate such a human even to want to try?

This epistemological formulation of the problem led me (Norton 2014, Sections 6–7) to give some epistemological analysis of what I called "the historical-anthropological objection." I agreed with John Worrall about the spuriousness of the epistemological problem posed. We have no reason to believe that our forebears were ever in the cognitive state represented by E. Even while objecting that the problem as posed engaged in wild speculation, I sought to make the point by responding with more speculation of my own on the prospects of primitive cognition in what I called a "counter-fable."

Looking back, I stand by the content of my analysis. However, I now regret not choosing a more cautious response. The material theory of induction has no trouble dealing with the inductive logic of the problem. Once the problem is enmeshed with fabrications of fictitious primitive humans in the epistemology of belief, it can no longer be addressed responsibly by armchair philosophers. Even though this was the basic point that I sought to make, it

20 One might worry that this use of hypotheses strays into the epistemology of beliefs. The use of hypotheses, as described in Chapter 2, is akin to the positing of a hypothesis in ordinary deductive logic as part of a *reductio ad absurdum*. In both cases, the hypotheses figure in explicit logical relations over propositions. Beliefs need not enter the analysis.

was a mistake to engage in any more detail,[21] for it invites the misapprehension that the material theory of induction has some responsibility to make sense of primitive humanoid cognition. It does not. Its compass is restricted to inductive logic defined over propositions and especially those that enter into routine science. It has no responsibility to the inchoate speculations of a primitive Adam when he first stumbles out of his cave.

De Grefte (2020) entangles logic and epistemology in a sequence of dubious arguments. First, he argues that "proponents of the material theory of induction are in fact committed to an externalist epistemology." Here I resist all attempts to enmesh the material theory in issues of epistemology and have no interest in connecting the material theory of induction with any particular epistemology. Lest the point pass, however, I should report that de Grefte's efforts to establish a commitment to an externalist epistemology are weak. As far as I can see, internalists can employ the material theory of induction simply by being aware of the material facts authorizing the inductive inferences behind their reasoning.

Second, de Grefte (2020, 100) argues "that externalist epistemologies are generally able to dissolve the problem of induction." In Section 10 of this chapter, I argued that this is a mistake. That there is no problem of induction in an externalist epistemology does not solve a problem in inductive logic. Moreover, reliabilist externalist epistemologies are felled by a problem analogous to the problem of induction.

Hence, finally, with these two failures, there is no foundation for de Grefte's claim:

> Like extant forms of externalism, Norton's material theory of induction dissolves the problem of induction. But since the material theory *entails* an externalist epistemology, one may suspect it is this externalism that does the epistemological work here. (2020, 104; de Grefte's emphasis)[22]

Weintraub's (2016, Section 4) appraisal of the material dissolution illustrates again the dangers of mixing logic and epistemology incautiously.

21 This regret also applies to remarks in Norton (2003, as in 668n9), in which I assert (correctly) that brute facts like "the ball is red" already presupposed universal knowledge.

22 See also my response in Norton (2021a, Section 6).

After recounting the much-cited "bismuth" example of Norton (2003, 649),[23] she writes

> But it is extremely implausible to suppose that if bismuth is *in fact* an element, but we justifiably believe that it *isn't* or have no opinion about the matter, our belief that it melts at 271 C is justified, our sample of positive instances notwithstanding. (72; Weintraub's emphasis)

That is, Weintraub supposes that we have mistakenly come to believe falsities of the background domain or perhaps have no suitable background beliefs. Then she correctly notes that we would be unable to justify the appropriate conclusion concerning the melting point of bismuth. There is no fault here in the inductive logic. The fault lies in the translation of logic into belief states. The cognizer proceeds by supposition from false or inadequate beliefs. It is a failure outside the compass of the material theory of induction.

Her dismissal of the material dissolution of the problem of induction seems to rest on a misreading of the material theory. Weintraub (2016, 72) characterizes the material theory as "an attempt to eliminate induction," grouped with Popper's inductive eliminativism. I understand her to hold that the material theory treats inductive inferences as enthymemes. That is, they will be rendered deductive with the addition of the material postulate as another premise.[24] Weintraub reports correctly some truisms of deductive logic, such as "that all observed instances of bismuth were elements doesn't entail that *all* instances of bismuth are elements" (72; italics in the original). However, these truisms are insufficient to support her conclusion: "Norton's attempt to dissolve the problem of induction, I conclude, fails (again) because its characterization of our practice is erroneous" (72). Weintraub's critique is based on an erroneous characterization of the material theory.

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23 From "some samples of the element bismuth melt at 271C," we infer that "all samples of the element bismuth melt at 271C" using the warranting fact that "all samples of bismuth are uniform just in the property that determines their melting point, their elemental nature. . . ."

24 Here Weintraub overlooked the disclaimer in Norton (2003, 651): "Chemical elements are generally uniform in their physical properties, so the conclusion of the above induction is most likely true." A footnote explains the inductive risk taken: "Why 'generally'? Some elements, such as sulfur, have different allotropic forms with different melting points."

Finally, Skeels (2020) somehow manages to convince himself that there are two "Nortons" who advocate two different material theories. They correspond to the real logical version and Skeel's invented epistemological version of the material theory. In the first, justifications derive from facts and, in the second, from knowledge. Skeels then seeks to use his misidentification to impugn the material dissolution of the problem of induction. See Norton (2021a, section 14) for my response.

16.3. More Treatments

For completeness, I recall some other treatments of the material dissolution of induction in the recent literature.

Livengood and Korman (2020) accept the material dissolution of the problem of induction as a matter of inductive logic. However, they urge that rational entitlement to future beliefs goes beyond consideration of evidence and inductive logic. The entitlement fails in the absence of a suitable explanatory relationship between the belief and the fact to be believed. As I indicate in my response (Norton 2021a, Section 9), this problem goes beyond the concerns of the material theory of induction. It is an issue of the epistemology of belief formation, and I hope that epistemologists can resolve it.

Jackson (2019, 164) disputes the material dissolution of Hume's problem by disputing a key condition of the material theory itself, that warranting facts must be facts: that is, truths. He argues, erroneously, that this precludes proper warrant for eighteenth-century predictions that employed Newton's laws of motion. There is no problem here. Our best theory of gravity, general relativity, returns Newton's entire theory in the weak gravitational fields pertinent to eighteenth-century physics. Jackson also worries that "scientifically ignorant people" might no longer have a warrant for inferring that night will follow day. Having learned my lesson, I will not be lured again into speculating about the inductive practices of fictitious or vaguely specified "scientifically ignorant" people. If inventions and fictions are to be avoided, then Jackson is well advised to do the same.

Peden (2019) offers a friendly amendment to the material dissolution of the problem of induction. He argues that it would benefit from supplementation by the combinatorial justification for induction of Williams and Stove, in conjunction with what is sometimes called "direct inference," "statistical syllogisms," or "proportional syllogisms." Whether this supplement is helpful is a topic that needs to be dealt with elsewhere. However, I am wary of such gifts since my fear is that they create more problems than they solve.

17. Conclusion

Hume's problem of induction has the reputation of being one of the most fearsome and intractable problems of philosophy. In her synoptic article, Henderson (2020) reports Russell's dark warning: "If Hume's problem cannot be solved, [Russell laments, then] 'there is no intellectual difference between sanity and insanity.'" Henderson finds a huge range of solutions in the present literature and an enduring belief by many that none succeeds. When such diversity persists, we can only conclude that, so far, we are doing a poor job of protecting ourselves from the lamentable conclusion that Russell feared.

Of the many solutions currently on offer, in my view, the best is Reichenbach's pragmatic solution. It is a dominance argument. We should infer inductively, even if we cannot justify induction as leading to the truth, since, pragmatically, if any method can work, then induction will work. The pragmatic solution has its best exposition and elaboration in Salmon (1967). Over half a century after its publication, I still find it to be one of the best treatments of Hume's problem. Ingenious as it is, Reichenbach's pragmatic solution is unsatisfying. It puts us in the same position as a drowning man, clutching at straws. Both we inductive inferers and the drowning man would like some further assurance of the efficacy of our desperate measures. We should like something a little stronger than "What have you got to lose?!" That this pragmatic answer and clever formal elaborations of it should retain a firm position in the literature is a sure index of the literature's failure to treat the problem well.

This despondent view was my view until I began work on the material theory of induction. It became clear then that even the most intractable problems are defined within a framework. What can make them intractable is precisely that we seek solutions within the framework. If we can break out of that framework, then perhaps the problem can be solved. In the best case, the problem can no longer even be set up. That proves to be the case when we adopt a material theory of induction. The problem of induction, in its most intractable modern form, is a problem for universal rules of induction. Once we adopt a material theory of induction, we abandon universal rules of induction. We break out of the confining framework. The problem of induction can no longer be set up. It is dissolved.

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