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The Rise and Fall of Karl Popper's Anti-inductivism

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Karl Popper's attempt to find an account of the rationality of science without inductive inference was a bold and philosophically rigorous response to the problem of induction. It was bound to fail since science is ineliminably an inductive enterprise. The failing lay not with scientists for using a defective argument form but with philosophers who were unable to account for the success of inductive inference. Such an account is provided by the material theory of induction.

1. Introduction

Karl Popper's account of the rationality of science emerged first in print in the early 1930s and, in the course of the twentieth century, rose to prominence. Its leading idea was that our best science is not supported inductively by the evidence, for inductive inference, Popper argued, could not be justified. In its place, Popper proposed a version of scientific rationality that employed deductive inference alone. Science advances through a cycle of conjectures and refutations. Theories are put to a test by confronting their predictions with experience. Those whose predictions are contradicted by experience are falsified and discarded. Scientists are freed to continue the search for better theories. Those that survive are designated as "corroborated," which simply means that they have survived this severe testing. Since the deductive relation of falsification plays an essential role in this cycle, Popper proposed that the falsifiability of a science was not just a necessary property of the science, but the distinctive feature that demarcated it from other intellectual endeavors.

This paper has a historical and a critical part. The first historical part will recount how Popper's starting point, his anti-inductivism, aligned with a growing sense in the early part of the twentieth century of foundational flaws in inductive inference. Other philosophers of science of this time, most notably Hans Reichenbach, recognized the problem, but continued to develop accounts of scientific rationality in which inductive inference played a major role. Popper was distinctive, however, in the rigor of his response. If inductive inference was foundationally flawed, then we must find an account of the rationality of science that does not use it. Under the inspiration of Einstein's successful predictive testing of this theories, Popper offered his deductively-based falsificationism as that account.

Sections 2 and 3 below recalls how inductive inference has traditionally been a locus of concern and criticism for philosophers, in spite of its centrality in science. It was with good reason identified by C. D. Broad in 1926 as the "glory of science [and] … the scandal of philosophy." Hume's devastating critique of induction in the eighteenth century had all but completely faded from nineteenth century philosophical discussion. Section 4 recalls how, in the early twentieth century, Bertrand Russell and Hans Reichenbach revived and accepted Hume's critique as demonstrating that the justification of induction involved a fatal circularity. Yet they did not abandon inductive inference, but sought to retain it by stratagems that now appear contrived and suspect.

Karl Popper also endorsed Hume's critique and, as Section 5 reviews, introduced an improved regress version of it to protect it from the accusation that its invocation of circularity rendered it meaningless. Where Russell and Reichenbach hesitated, Popper did what the rigor of philosophical analysis required: if induction could not be justified, then there must be an account of the rationality of science that does not employ it. With inspiration from the predictive successes of Einstein's general theory of relativity, as Sections 6, 7 and 8 recount, Popper formulated his falsificationist account of the rationality of science.

The second critical part of this paper gives a more sober assessment of Popper's project. It was a bold conjecture by Popper that there could be a serviceable account of the rationality of science without induction. It was advanced by Popper in a tumultuous era of philosophical thought in the 1920s and 1930s in which new and extraordinary ideas were advanced to solve old problems. Popper's proposal kept company with the exuberance of the logical positivists of the Vienna circle. They declared with unfettered optimism that their verifiability criterion of meaning and their reliance on formal logic would usher in a new era in philosophy.

Section 9 recounts that, in spite of its admirable rigor, Popper's conjecture was just too bold. Science is inherently an inductive venture and is so in a very complicated way. A purely deductive account of its rationality does not have the resources to provide a surrogate for how inductive inference is used science. Section 10 reviews what is, in my view, the greatest failing of Popper's falsificationism: it does not describe what successful science does. Two examples illustrate the failure. When evolutionary theory in biology and climate science were each under threat from skeptics, they produced elaborate, cautious and thorough statements of the rationality underlying their sciences. One is from the US National Academy of Sciences. The other is from the Intergovernmental Panel on Climate Change ("IPCC"). They defend their science with a sustained use of inductive inferences.

Sections 11 and 12 review further difficulties for Popper's falsificationism. Popper repeatedly recalled how he took Einstein's testing of general relativity to be an inspiring example of falsificationism. A closer look at Einstein's evidential practices shows no special attachment to falsificationist ideas. His methodology was adapted opportunistically to whichever he felt would work best for the particular problem in physics at hand. Section 12 collects more specific problems for falsificationism. The first concerns the difficulties of purging inductive notions from the practice of science. The second concerns the falsificationist notion that an assessment of the admissibility of some theory or hypothesis cannot be determined by a static inspection of the evidence, but must be given in a recounting of its history. The third collects ways in which falsifiability fails as a criterion that demarcates sciences.

Section 13 returns to the problem that vexed Russell, Reichenbach and Popper in the early twentieth century: the problem of induction. It was for them an insoluble problem because they assumed that inductive inference is governed by universally applicable, formal rules. If that assumption is dropped in favor of a material conception of inductive inference, then their versions of Hume's problem of induction can no longer be set up. The problem of induction is dissolved. Induction can be both the workhorse of science and the glory of philosophy. Section 14 offers a synoptic reflection.

To preclude confusion, the "rise" and "fall" pertains to the philosophical merit of Popper's falsificationism. His analysis rose by this measure when it was first proposed, since it was superior in its response to Hume's problem of induction than the contemporary responses of Russell and Reichenbach. The inevitable fall derives from a similar measure. It reflects the failure of falsificationism to provide a serviceable account of the evidential practices of actual science, its internal difficulties and that Hume's problem can be escaped in way that does not impugn inductive inference. We could assess Popper's falsificationism in a way not employed in this chapter, that is, by its popularity. Then its early history is of a fall, since it was largely dismissed, marginalized or ignored by other philosophers of science at the time of its proposal. Its rise came later with its growing popularity among the larger scientific community that found the falsifiability demarcation criterion to be especially useful. Such a popularity account is not the subject of this chapter.

PART I. The Rise

2. Inductive Anxieties

Inductive inference has traditionally been a locus of hesitation and concern among philosophical writers. Francis Bacon's celebrated riposte contributed to an existing tradition of complaints. He wrote (1620, p. 83):

> 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.

It was followed by Hume's celebrated and even more damaging critique that inductive inference cannot be justified without a harmful circularity. That is, all inductive inference assumes that the future will resemble the past. Hume objected (1777, p. 38):

> 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.

Hume's critique is now remembered as introducing one of the most challenging of philosophical problems, *the* problem of induction. His writing "interrupted [Kant's] dogmatic slumber"[1] and may even have motivated Thomas Bayes (1763) to his celebrated introduction of inverse probabilities.[2] The notoriety of Hume's problem of induction did not endure. In the nineteenth

[1] Kant (1783, p. 7).

[2] See Norton (2024, Ch. 6, §5).

century, Hume's circularity objection to inductive inference faded in favor of the older sense of inductive anxiety.[3]

Henry Sidgwick, a celebrated Cambridge philosopher of the late nineteenth century gave the anxiety this expression in his "Criteria of Truth and Error" (Sidgwick, 1900). He presented induction in science as having a founding role in empiricism (p. 15):

> I take the principle of Empiricism, as an epistemological doctrine, to be that the ultimately valid premises of all scientific reasonings are cognitions of particular facts; all the generalisations of science being held to be obtained from these particular cognitions by induction, and to depend upon these for their validity.

What followed immediately was an expression of his exasperation with induction:

> I do not accept this principle I think it impossible to establish the general truths of the accepted sciences by processes of cogent inference on the basis of merely particular premises; and I think the chief service that J. S. Mill rendered to philosophy, by his elaborate attempt to perform this task, was to make this impossibility as clear as day.

Explicit complaints like Sidgwick's identified the inductive support for science just in a narrow sense of inductive inference. That is, in this earlier literature, "induction" commonly designated the simplest form of inductive inference, enumerative induction, indicated by Bacon above. In it, we infer from some instances of *A*'s that are *B's* to all being so. This narrow reading persisted up to the early twentieth century, even though relations of support between theory and evidence in science had already routinely employed a wider repertoire of inference forms. We would now include them under a much more expansive use of the term "inductive inference" or just "induction" as what we might now call "ampliative inference." Bacon (1620) himself had advanced his method of tables as such an expansion of the repertoire. His method found an influential nineteenth century expression in John Stuart Mill's (1882, Book III) methods. Hypothetico-deductive methods had been used since at least the seventeenth century and the term "hypothetico-deductive" was used freely by the end of the nineteenth century. As we shall see below, Darwin explicitly employed what we would now call "inference to the best

[3] For a survey of this hiatus in the problem of induction in this period, see Norton (2024, Ch. 6, §6).

explanation" in developing the arguments of his *Origin of Species*. Bayesian methods maintained a lesser following, such as in W. Stanley Jevons, *Principles of Science* (1874).

Whereas these early, explicit expression of concern targeted this narrower sense of inductive inference, the general complaint about the lack of foundation of inductive could apply to them all.

3. The Glory of Science and the Scandal of Philosophy

From the lofty heights of philosophical analysis, the foundational insecurity of inductive inference was plainly visible. The enduring awkwardness was that science had thrived using this foundationally insecure form of inference. A weary sense of the longevity of the tension is what prompted Broad's (1926, p. 67) celebrated lament that reads more fully as:

> May we venture to hope that when Bacon's next centenary is celebrated the great work which he set going will be completed; and that Inductive Reasoning, which has long been the glory of Science, will have ceased to be the scandal of Philosophy?

The problem was already compelling in Hume's time. It is what woke Kant from his slumbers. Euclid's geometry and Newton's mechanics had secured, it was then thought, the final, irrefutable truths of geometry and mechanics. Yet Hume showed that our inductive methods were not able to justify this certainty. Kant's *Critique* (1787) sought to restore the certainty to geometry and mechanics. Since we now know that Euclid's geometry and Newton's mechanics are not final truths of nature, he had set himself an impossible challenge. Kant's failure was inevitable.

The mismatch been the success of science and the fragile foundations of inductive inference provided Sidwgick the means he needed to impugn an empiricism that is based on inductive inference. His 1882 "Incoherence…" made the point (p. 543):

> If, finally, the reader who has got through this paper should say that my cavils cannot shake his confidence in experience, or in the aggregate of modern knowledge that has progressed and still progresses by accumulating, sifting, and systematising experience—I can only answer that my own confidence is equally unshaken. The question that I wish to raise is not as to the validity of received scientific methods, but as to the general epistemological inferences that may

> legitimately be drawn from the assumption of their validity. It is possible to combine a practically complete trust in the procedure and results of empirical science, with a profound distrust in the procedure and conclusions especially the negative conclusions—of Empirical Philosophy.

No doubt, this conclusion was uncongenial to inductivists. In my view, it correctly identified where the problem lay. It was not that science was employing a failed methodology. Rather the failure was of the philosophers of induction. They had been unable to arrive at a vindication of the success of scientific methods. There was work to be done, not by the scientists, but by the philosophers.

4. The Revival of the Problem of Induction

4.1 Bertrand Russell

In the early twentieth century, skepticism about the foundational security of inductive inference was reinforced by a revival of Hume's problem of induction. That revival was most visible in Bertrand Russell's engagingly written, popular *The Problems of Philosophy* of 1912. In its Chapter VI, "Of Induction," he recapitulated Hume's analysis, though not mentioning him by name. In its final form, the target concerned what he called the "principle of induction." Its formulation spanned several pages (pp.103-105). The central idea was that sufficiently many instances of the association of *A* with *B* will render is near certain in probability that the next case will conform with the association; and, in a stronger version of the principle, that the association will hold as a general law.

The fatal circularity now followed (p. 106, Russell's emphasis):

> 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.

After this evisceration of inductive inference, subsequent chapters tried to find some basis for our knowledge of the world by taking a meandering path through notions of logical and a priori truths, universals and intuitive, self-evident knowledge.

The problem clearly continued to worry Russell. His 1923 *Human Knowledge: Its Scope and Limits* included this statement of its goal (p. 11): "To discover the minimum principles required to justify scientific inferences is one of the main purposes of this book." He allowed that such knowledge could at best be "only *likely* to be true." (p. 11, Russell's emphasis). Thus, he continued, his account would be probabilistic. Part VI of the volume was to provide the foundation for this knowledge. That is, he sought (p. 13):

> … the minimum assumptions, anterior to experience, that are required to justify us in inferring laws from a collection of data; and further, to inquire in what sense, if any, we can be said to know that these assumptions are valid.

These assumptions resided in a collection of five postulates and supporting notions. They included the positing of "causal lines," exemplified by the collision of billiard balls, in the second postulate (pp. 507-508):

> II. The postulate of separable causal lines
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> It is frequently possible to form a series of events such that, from one or two members of the series, something can be inferred as to all the other members

The third "postulate of spatio-temporal continuity" was formulated to deny "action at distance." (pp. 509-10). The fifth "Postulate of analogy" was (pp. 511-12):

> Given two classes of events *A* and *B*, and given that, whenever both *A* and *B* can be observed, there is reason to believe that *A* causes *B*, then if, in a given case, *A* is observed, but there is no way of observing whether *B* occurs or not, it is probable that *B* occurs; and similarly if *B* is observed, but the presence or absence of *A* cannot be observed.

Overall, Russell's response to the tension between the glory of science and the scandal of philosophy is this: make substantial ontological posits in which inductive inference survives in a curtailed form as assignments of probability. The postulates were rich in content and the less plausible for it, if enduring foundations for scientific inference are sought, for they are likely just to reflect the present state of an ever-evolving physics.

Russell's overall conclusion conveyed a pessimism that suggests he was well-aware of the limits of his proposals. He wrote (p. 13):

> That scientific inference requires, for its validity, principles which experience cannot render even probable, is, I believe, an inescapable conclusion from the logic of probability.

4.2 Hans Reichenbach

For Russell, probabilities entered his analysis as one component among many. For Hans Reichenbach, it was the core concept in his analysis of scientific rationality. He sought to supplement and even replace certainty and deductive relations with probabilistic relations. In later reminisces, written in 1936, he recalled how Hume's critique had already, apparently in the 1920s, led him to this focus on probability theory (Reichenbach, 1936a, pp. 6-7):

> But one problem remains still unsolved which since then has caused greatest difficulties to philosophy; and moreover no consistent empiricism can be developed as long as it remains unsolved: that is the problem of induction. Since Hume's splendid critique, this problem dominates all epistemology and, now that the solution suggested by Kant has been proven untenable, one had to find another.
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> These questions led me to the problem of probability. For a conclusion based on inductions is in fact a conclusion based on probability. Thus, I frequently interrupted other work in order to work on the problem of probability.

His evaluation of the profound import of Hume's critique appeared in a publication of 1930/31 (Reichenbach, 1930/31b, p. 183, my trans.):

> It has become sufficiently clear in the historical discussion of the problem of induction that it is not a question of logical necessity. Hume's real achievement was to have recognized this, and nothing essential has been added to this realization since then. And Hume also clearly demonstrated that it is not possible to justify the law of induction through experience, because every such inference presupposes the same law at a higher level. This epistemological fact cannot be doubted, and philosophical theories that fail to recognize this fact are not to be considered.

This brief formulation of Hume's critique suggested that the difficulty lay in some sort of regress. Reichenbach soon published more careful versions in which the problem resided in a circularity.

Reichenbach's (1938) *Experience and Prediction* presented Hume's critique as challenging what he called a "principle of induction" (pp. 340-41). This version of the principle asserted that the frequency of successes in repeated trials approaches a limit close to the observed frequencies.[4] Hume's critique was then summarized as:

> 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.

This circularity version of Hume's critique was also given a few years earlier at greater length in Reichenbach's *Wahrscheinlichkeitslehre* (1935).[5]

Reichenbach's response to this problem was conditioned by a strong attachment to inductive inference. He had already announced the indispensability of the principle of induction in Reichenbach (1930a, pp. 64-65, my trans.):

> … this principle [of induction] absolutely cannot be discarded, because it is the proper means through which truth is determined in science. If we were to give up the principle of induction, arbitrariness would thereby enter into science, and any arbitrary assertion about physical nature would be compatible with existing observations.

Reichenbach was caught between irreconcilables. Induction is both unjustifiable and indispensable for science. His solution was an act of desperation, the pragmatic solution to the problem of induction. According to it, we cannot justify induction, but we should use it anyway

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[4] This looks like a law of large numbers in probability theory, but it is not since there are no probabilities.

[5] That is, it appears in a later revised, English translation, Reichenbach (1949, p. 470). I have been unable to check that the corresponding German text is in the 1935 edition.

since we have nothing better. Reichenbach gave a quasi-formal statement of it in his *Theory of Probability* (1949, p. 475):

> Thesis θ. The rule of induction is justified as an instrument of positing because it is a method of which we know that if it is possible to make statements about the future we shall find them by means of this method.

Reichenbach offered colorful metaphors to illustrate his pragmatic solution. For example (Reichenbach, 1936b, p. 157):

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

Reichenbach gave a similar metaphor in *Experience and Prediction* (1938, p. 349). It concerned a pragmatic choice by a gravely ill patient to undertake a surgical operation whose success was uncertain because there is no other option.

These metaphors are engaging and can give us some momentary comfort in Reichenbach's solution. However, they should not be mistaken for good arguments. The fisherman does have something to lose if his efforts preclude finding food by other means. The patient may choose not to have surgery if the operation itself and its side effects are dire. Metaphors aside, a pragmatic recommendation to use induction can only be endorsed if we can give a fuller assessment of the costs and benefits; and that requires a richer specification of the circumstances of the particular scientists undertaking investigations. These conditions will vary from instance to instance. In any case, the pragmatic solution is a poor match with how scientists conceive inductive inference. They rely on it, not out of desperation, but because of their trust in induction. A proper solution would vindicate this trust.[6]

5. Popper's Regress Form of the Problem of Induction

Reichenbach's reminiscences, reported above, gave Hume's problem a strong presence in the motivation for his analysis of inductive inference. While he knew of Hume's work, Popper

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[6] Is such a vindication possible? See Section 13.

gave no comparable report in autobiographical remarks in *Conjectures and Refutations* (1962, Ch.1) and his *Unended Quest* (1992). Nevertheless, Popper's highly influential *Logik der Forschung* (1935)/*Logic of Scientific Discovery* (2005) takes Hume's problem as its starting point. In Chapter 1, the first section is "The Problem of Induction." Where other authors had formulated Hume's problem as a harmful circularity, Popper formulated it as a problematic, infinite regress.

Popper took the target of the problem to be a principle of induction. He quoted Reichenbach (1930b, p. 186) for his formulation of the principle as the means of determining truth in science. In Popper's (accurate) translation, Reichenbach's text read (Popper, 2005, pp. 4-5)

> '. . . this principle', says Reichenbach, 'determines the truth of scientific theories. To eliminate it from science would mean nothing less than to deprive science of the power to decide the truth or falsity of its theories. Without it, clearly, science would no longer have the right to distinguish its theories from the fanciful and arbitrary creations of the poet's mind.'

Popper's regress formulation of the problem of induction then followed (p. 5):

> Now this principle of induction cannot be a purely logical truth like a tautology or an analytic statement. Indeed, if there were such a thing as a purely logical principle of induction, there would be no problem of induction; for in this case, all inductive inferences would have to be regarded as purely logical or tautological transformations, just like inferences in deductive logic. Thus the principle of induction must be a synthetic statement; … To justify it, 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.

This version of the problem of induction is an abbreviated version of one found in what Popper described in a later publication (2009, preface) as drafts and preparatory writings from 1930-33 for *Logik der Forschung*. In those preparatory notes, Popper (Book 1, Ch.III) described the analytic caution that led him away from the circularity formulation to the regress formulation. Circularities in references among sentences can lead to notorious internal contradictions. The most famous is the liar sentence, which in its simplest form asserts: "This sentence is false."

Russell (1908) had examined these forms of viciously circular self-reference and proposed a typed language in which such self-reference could not arise. Popper was concerned that his formulation of the problem of induction could not be dismissed as merely this sort of internally inconsistent, vicious circularity. He concluded (Book 1, Ch. III, Popper's emphasis):

> 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.

Over the pages following, Popper gradually developed a hierarchy of principles of induction in which the infinite regress appears. He eventually gave it 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.

This summary was then followed by a strong, programmatic statement:

> This line of argument is the foundation of the critique of inductivism.

6. Popper's Falsificationism

Popper's response to Hume's problem was unlike that of Russell and Reichenbach. He accepted the success of Hume's critique in its regress form and sought to develop an account of the rationality of science that employed no inductive notions. He wrote in the early pages of *Logik der Forschung* (2005, pp. 6-7, Popper's emphasis):

> The theory to be developed in the following pages stands directly opposed to all attempts to operate with the ideas of inductive logic. It might be described as the theory of *the deductive method of testing*, …

The ensuing account is too well known to require anything more than the briefest summary here. The rationality of scientific investigations resided in a continuing cycle of conjectures and refutations. A scientist may conjecture a new theory and seek to test it by making predictions from it that differ from those of existing theories. Popper recounted what is to happen next (2005, p. 10, Popper's emphasis):

> Next we seek a decision as regards these (and other) derived statements by comparing them with the results of practical applications and experiments. If this decision is positive, that is, if the singular conclusions turn out to be acceptable, or *verified*, then the theory has, for the time being, passed its test: we have found no reason to discard it. But if the decision is negative, or in other words, if the conclusions have been *falsified*, then their falsification also falsifies the theory from which they were logically deduced.
>
> It should be noticed that a positive decision can only temporarily support the theory, for subsequent negative decisions may always overthrow it. So long as theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is 'corroborated' … by past experience.

Popper emphasized his account did not employ inductive inference (p. 10):

> Nothing resembling inductive logic appears in the procedure here outlined. I never assume that we can argue from the truth of singular statements to the truth of theories. I never assume that by force of 'verified' conclusions, theories can be established as 'true', or even as merely 'probable'.

In this cycle of conjecture and refutation, the decisive advances come when a new theory is falsified by failed predictions. Popper identified this possibility of falsification as distinctive of science. That its propositions are falsifiable is the criterion that distinguishes or demarcates it from other systems of thought (p. 18, Popper's emphasis):

> … I shall certainly admit a system as empirical or scientific only if it is capable of being tested by experience. These considerations suggest that not the *verifiability*

> but the *falsifiability* of a system is to be taken as a criterion of demarcation. … In other words: I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience*. …

7. Falsificationism in Einstein's Work

If we are to identify episodes in science where Popper's falsificationist models fits well, the leading example is Einstein's work in physics. Einstein's habit was to conclude his presentation of important new theories with three predictions whose subsequent verification would confirm his theories. Two of his famous papers of the 1905 *annus mirabilis* concluded this way. They are his light quantum paper (Einstein, 1905a) and his paper on special relativity (1905b)

The most prominent example came with Einstein's general theory of relativity. After the theory had achieved a stable form in November, 1915, Einstein wrote a synoptic review of the theory, Einstein (1916). It concluded (pp. 818-22) with three famous predictions: the red shift of light from the sun, the bending of starlight grazing the sun and the retrodiction of the anomalous motion of Mercury's perihelion. They functioned as tests of Einstein's theory. The theory passed them and was secured as the default theory of gravity. The most visible of these tests was the successful eclipse expeditions of 1919 that measured the bending of starlight grazing the sun. The resulting media frenzy made Einstein into a prominent public figure.

It is, of course, no accident that Popper's account fits Einstein's practice. Popper recalled in various autobiographical recollections[7] how Einstein's success with general relativity impressed him greatly and provided him a model for rationality in science. In a BBC interview with Gerald Whitrow, Popper clarified the important role of Einstein's work in Popper's thinking (Whitrow, 1967, p. 23):

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[7] See for example *Conjectures and Refutations* (1962, Ch.1) and his *Unended Quest* (1992, pp. 37-38).

> Einstein's influence on my thinking has been immense. I might even say that what I have done is mainly to make explicit certain points which are implicit in the work of Einstein.

He elaborated on the points derived from Einstein's example (pp. 25-26):

> The Einsteinian revolution has influenced my own views deeply: I feel that would never have arrived at them without him. … Thus, what Einstein's example may teach the philosopher is that science consists of bold speculative guesses controlled by merciless criticism which includes experimental tests.

8. Popper's Rigor

In all these analyses, Popper displayed a laudable purity of philosophical analysis. Once he had concluded that we could not justify inductive inference, Popper felt justified in taking extreme measures. It was to see if we could do science without induction. His unflinching acceptance of the consequences of Hume's analysis compared favorably with both Russell's and Reichenbach's reactions. They both struggled with unstable compromises.

Russell had sought to save inductive inferences with a collection of ontological posits that could only serve the momentary state of science. To presume that induction was based on a denial of action at a distance might fit well with the rise of field theories in the physics of the nineteenth century. It does not fare well in the physics that followed. The non-locality and non-separability of quantum states makes an aversion to action at a distance appear quaint.

Reichenbach's pragmatic solution is, in my view, even less defensible. To sustain using induction pragmatically requires a more thorough exploration of the particular circumstances of each scientific investigation. In each we must ask, what are the costs? What are the benefits? It matches poorly with the practice of science. Investigators do not infer inductively because they do not know whether it works but use it in desperation because they have nothing better. On the contrary, they use it because they have the highest confidence in inductive inference.

Part II. The Fall

9. The insoluble problem

In the 1930s, when Popper first advanced his falsificationist account of science, it was, in my view, the most responsible of the reactions to the problem of induction. However, it came at an unsustainable cost. Popper now sought an account of the rationality of science that used no inductive notions. It proved to be an insoluble problem for reasons that should have been evident from the start.

Science is an inherently inductive venture. And it is not inductive in any simple way. For the scope and variety of investigative problems addressed by science are huge, so that the scope and variety of inductive stratagems are correspondingly great. This diversity is reflected in the wide array of different formal accounts of inductive inference in philosophy of science.[8] Yet no collection of formal accounts ever seems final. The reason, I believe, is that the complexity of the inductive investigations of science is so great that no general set of formulae or universal rules, no matter how elaborate, can capture it. In the place of such rules, according to the material theory of induction,[9] inductive practices vary from factual domain to factual domain and the inductive inferences appropriate to each domain are warranted by facts peculiar to the domain.

Popper's attempt to reconceive scientific methodology without inductive inference meant that he needed to find a surrogate able to substitute for this great complexity. Appealing as its logical simplicity may be, such a simple scheme as that of conjectures and refutations has no chance of recreating this great complexity. It may succeed with a few carefully chosen examples, but it will fail with many more.

The philosophers of the 1930s were caught in a trap. It was well characterized by Broad's lament of induction as the glory of science and the scandal of philosophy. The philosophers of that era could not muster the resources to escape. The real problem was to know where the fault lay. As Russell and Reichenbach acknowledged, if philosophical analysis cannot vindicate the use of inductive inference, it is a mistake to lay the blame on science, to imagine somehow that

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[8] For an attempt at a survey that systematizes these many accounts, see Norton (2005).

[9] See Norton (2021, 2024).

induction has no role in science and to suppose that its pervasive success in science is really a widespread delusion among scientists. The blame for philosophy's failure to vindicate induction lay squarely with the paucity of philosophical resources then available. The correct path was to seek some novel philosophical analysis that would vindicate inductive inference. That such an analysis is possible was not evident in the 1930s.

In Section 13 below, I will argue that this vindication can be supplied if we abandon the formal accounts of inductive inference used in the 1930s and later in favor of a material conception. Before we turn to the material theory, the sections immediately following this one will articulate how Popper's ambitious, falsificationist program has failed.

10. Science is Inductive

The principal failing of Popper's falsificationism is its starting point. It seeks an account of the rationality of science without employing any inductive notions. That is its original mistake and the one from which it cannot recover. The goal of this section is to recall briefly the centrality of inductive inference in science. In earlier centuries, induction was understood to be both a procedure for discovery and a mode of justification. Mill's (1882) methods are a familiar example. In the twentieth century, induction and inductive inference reduced to the second, its role in justification. Inductive inference now features in science in two ways that distinguish it from Popper's falsificationist analysis:

- *A falsificationist account* is dynamic and identifies our present best candidates among scientific theories by their place in a dynamic process of conjectures, testing of predictions and refutations.

- *An inductive analysis* is static. It supplies strengths of inductive support for theories and hypotheses on the evidence that are independent of the historical processes that led to the appraisal.

Many forms of inductive inference provide the assessments of the strengths of support, but always to the same end. Results in science are then found to be more or less strongly supported by the evidence, independently of how the results were generated. Sometimes the strength of support is measured probabilistically, but generally only when good statistical models are available.

• *A falsificationist account* denies that evidence can ever establish theories securely. For example, Popper wrote (2005, p. 10): "I never assume that by force of 'verified' conclusions, theories can be established as 'true', or even as merely 'probable'."

An inductive analysis allows that, if the inductive support of the theory or hypothesis is strong enough, the theory or hypothesis will be taken to be established beyond reasonable doubt. It always allows the possibility of error in principle no matter the strength of the inductive support.

Inductive inference and inductive support are ubiquitous in science in the two ways indicated above. Norton (2021, 2024) recounts many examples as part of the developing and defending of the material theory of induction. The presence and essential role of static, inductive relations of support becomes apparent when a science is under threat and needs to defend itself. The two subsections that follow provide two examples.

10.1 Evolutionary Biology

The theory of evolution, as originally conceived by Charles Darwin's *Origin of Species* (1872), had no major role for predictions. The theory focused on establishing facts about the past. The diversity of modern biological species has emerged, it asserted, from a process of variation and natural selection. Accordingly, the word "prediction" appears only seven times in the whole text and at least some of these few occurrences announce a failure of possible prediction.[10] The all-consuming goal of the work was to establish his historical account of evolution on the basis of the biological evidence. To this end, he used two identifiable, inductive argument forms. The first four chapters used an argument from analogy. Pigeon breeders alter the characteristics of subsequent generations of their birds by domestic selection. Darwin urged that nature produces different species analogously by natural selection.

[10] "But which groups [of presently dominant organisms] will ultimately prevail, no man can predict; for we know that many groups, formerly most extensively developed, have now become extinct." (p. 96)

After these four chapters, Darwin's text employed the inductive inference form that we would now call "abduction" or "inference to the best explanation."[11] He assembled a prodigious display of examples throughout biology and argued they were best explained by his theory. In concluding remarks in the sixth edition of *Origin*, Darwin made clear that he understood the general argument form and that he took its successful application to support the truth of the explaining theory (1872, p. 421):

> It can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection, the several large classes of facts above specified. It has recently been objected that this is an unsafe method of arguing; but it is a method used in judging of the common events of life, and has often been used by the greatest natural philosophers. The undulatory theory of light has thus been arrived at; and the belief in the revolution of the earth on its own axis was until lately supported by hardly any direct evidence.

Darwin's theory was immediately beset with opposition. One component was largely religiously motivated by Darwin's portrayal of humans merely as evolved animals.

This religiously-motivated opposition has continued to the present. In the decades around 2000, the US National Academy of Sciences produced a collection of publications designed to support evolutionary theory and to defend its place in education. The critics of evolution had settled upon Popper's account of falsifiability as a demarcation criterion for science and used the slender role of prediction in evolutionary biology to impugn the theory as unscientific. The Academy did not give the correct response of rejecting the applicability of Popper's criterion as the sole and decisive criterion for assessing the scientific status of evolutionary theory. Instead, it scrambled to find ways that evolutionary theory could make predictions. Institute of Medicine (2008, pp. 2-3) gave pride of place to the discovery of a predicted intermediate tetrapod, *Tiktaalik.* Their general narrative emphasized explanation in accord with Darwin's original text, but also added Popper's requirement of testability, such as in Institute of Medicine (2008, p. 80).

When this Academy document turned to the task of making the case for evolutionary theory, the notion of successful prediction had no significant presence, even as retrodictions, that

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[11] See Norton (2021, Ch. 9, §4) for an account of Darwin's argument within the material theory of induction.

is, prediction about the past. Chapter 2, "The Evidence for biological Evolution," followed the traditional inductive model of identifying specific pieces of evidence and showing how they support particular components of the theory under investigation. The notion of successfully tested predictions, that is, predictive inferences *from* the theory *to* the evidence, in some dynamic process, plays no role in the evidential case made.[12] Various items of evidence support specific components of evolutionary theory in inductive inferences that proceed *from* the evidence *to* the relevant component, where these inferences display static relations of support between evidence and the relevant component.

"Pull quotes," that is sentences extracted from the main text and displayed in larger type, indicate the direction of inference from evidence to theory. For example:

> The fossil record provides extensive evidence documenting the occurrence of evolution. (p. 22)
>
> Common structures and behaviors often demonstrate that species have evolved from common ancestors. (p. 24)
>
> Molecular biology [DNA evidence] has confirmed and extended the conclusions about evolution drawn from other forms of evidence. (p. 28)

The overall import of the chapter was summarized at its outset (p. 17) as:

> Many kinds of evidence have contributed to scientific understanding of biological evolution. Some of this evidence—such as the fossils of long extinct animals and the geographical distribution of species—was familiar to scientists in the 19th century or earlier. Other forms of evidence—such as comparisons of DNA sequences—became available only in the 20th and 21st centuries.
>
> The evidence for evolution comes not just from the biological sciences but also from both historical and modern research in anthropology, astrophysics, chemistry, geology, physics, mathematics, and other scientific disciplines, including the

[12] The word "prediction" appears only once (p. 19) in its making the evidential case and then in relation to the prediction by big bang cosmology of the cosmic background radiation. This was not a predictive test of evolution but of big bang cosmology. The word "test" appears just twice in this Chapter 2. For example: "Hypotheses based on this evidence then can be tested by examining the fossil record." (p. 25)

> behavioral and social sciences. Astrophysics and geology have demonstrated that the Earth is old enough for biological evolution to have resulted in the species seen today. Physics and chemistry have led to dating methods that have established the timing of key evolutionary events. Studies of other species have revealed not only the physical but also the behavioral continuities among species. Anthropology has provided new insights into human origins and the interactions between biology and cultural factors in shaping human behaviors and social systems.

The synoptic summary of this evidential case does not conform with the provisional character of a theory that is merely well-corroborated. Instead, we read of conclusions of near certainty (p. 11):

> Because the evidence supporting it is so strong, scientists no longer question whether biological evolution has occurred and is continuing to occur.

And (p. 33)

> But today there is no scientific doubt about the close evolutionary relationships between humans and all other primates.

10.2 Climate Change

In contrast with evolutionary theory, the scientific analysis of climate change is devoted primarily to making predictions. The goal is to provide policy makers with sufficiently secure predictions to enable sound policy making. The discovery that climate change is due in large measure to human actions has been subjected to sustained opposition. In response, climate scientists have sought to make the case for the role of human actions. The task of assessing climate change has been assigned to the "Intergovernmental Panel on Climate Change" ("IPCC"), which has become the authoritative voice of climate scientists. The IPCC routinely publishes assessments of many aspects of climate change. Starting in 1990, each few years, the IPCC publishes an assessment of the evidence for climate change. The sixth report, *Climate Change 2021; The Physical Science Basis* (IPCC, 2021) offers "… a full and comprehensive assessment of the physical science basis of climate change, based on evidence from more than 14,000 scientific publications available by 31 January 2021." (p. vii). The full report of nearly 2400 pages is simply overwhelming in the massive compilation of evidence and, I expect, beyond the comprehension in totality of almost all readers.

Our concern is the methods upon which the IPCC bases its results. Popper and Popperian ideas have no explicit presence in them. The words "Popper" and "falsifiability" appear once in the report's many pages. They appear in a single, perfunctory sentence in which falsifiability is included in a list of five "epistemic values" attributed jointly to Popper and his antagonist, Thomas Kuhn.[13] Further generalities on the methods used in the report are hard to make since the report collects the work of very many different climate scientists. The report does, however, seek to synthesize the results of this massive body of work in a form that is more readily comprehensible. The form employs a traditional notion of inductive inference in assigning "degrees of certainty" to what it calls "key findings."

The methods are summarized in the report's "Box 1.1: Treatment of Uncertainty and Calibrated Uncertainty Language in [this report]" (IPCC, 2021, pp. 169-70). The methods are in turn drawn from a "Guidance Note" (Mastrandrea et al., 2010), prepared for the previous IPCC report. The report used two "metrics," as they were called, to assess the degree of certainty of various results (IPCC, 2021, p. 169):

1. *Confidence:* a qualitative measure of the validity of a finding, based on the type, amount, quality and consistency of evidence (e.g., data, mechanistic understanding, theory, models, expert judgment) and the degree of agreement.
2. *Likelihood:* a quantitative measure of uncertainty in a finding, expressed probabilistically (e.g., based on statistical analysis of observations or model results, or both, and expert judgement by the author team or from a formal quantitative survey of expert views, or both).

These metrics were applied through a complicated, five step process. The first, qualitative "level of confidence," when agreement can be reached, is expressed with five "qualifiers":

very low, low, medium, high, very high.

The second, quantitative measure, "likelihood," when agreement can be reached, is expressed probabilistically. The probability ranges (expressed as percentages) are in turn interpreted by the likelihood labels indicated:

[13] IPCC (2021, p. 171) They are "explanatory power, predictive accuracy, falsifiability, replicability, and justification of claims by explicit reasoning." The list represents neither Kuhn's nor Popper's views well.

Virtually certain: 99-100%
Extremely likely: 95-100%
Very likely: 90-100%
Likely: 66-100%
More likely than not: >50-100%
About as likely as not: 33-66%
Unlikely: 0-33%
Very unlikely: 0-10%
Extremely unlikely: 0-5%
Exceptionally unlikely: 0-1%

These metrics are custom inventions of the IPCC analysis and there is a great deal more to their application. Their complexity reflects the fact that the assessment of the import of evidence in science is not a simple matter. It cannot be captured by an elegant formula like "conjectures and refutations" or the assessment that some theory has passed a rigorous test. It is messy and complicated.[14]

Popper held that "scientists do not seriously hold that their theories can be true or 'verified'."[15] The climate scientists of the IPCC think otherwise. Their "Summary for Policy Makers" is repeatedly unequivocal. Here are just a few examples of many:

> It is unequivocal that human influence has warmed the atmosphere, ocean and land. Widespread and rapid changes in the atmosphere, ocean, cryosphere and biosphere have occurred. (p.4)
>
> Observed increases in well-mixed greenhouse gas (GHG) concentrations since around 1750 are unequivocally caused by human activities. (p. 4)
>
> It is *very likely* that well-mixed GHGs were the main driver … of tropospheric warming since 1979 and *extremely likely* that human-caused stratospheric ozone

[14] A recognition of the complexity of inductive support and that no simple, universal rules capture it is the principal motivation for the "material theory of induction" in Norton (2021, 2024).

[15] In Whitrow, 1967, p. 24.

> depletion was the main driver of cooling of the lower stratosphere between 1979 and the mid-1990s. (p.5, IPCC emphasis)
>
> It is *virtually certain* that the global upper ocean (0–700 m) has warmed since the 1970s and *extremely likely* that human influence is the main driver. (p. 5, IPCC emphasis)

11. Popper's Einstein

11.1 Einstein the "unscrupulous opportunist"

The difficulty in seeking general methodological principles in Einstein's work is that Einstein was not a systematic philosopher and never pretended to be. He read and knew philosophy, but he used philosophy opportunistically, according to whichever philosophy suited his present purposes in physics. Famously, in later recollections (Einstein, 1949, p. 684), he admitted freely that he "… must appear to the systematic epistemologist as a type of unscrupulous opportunist." He proceeded to list how his philosophical positions in one aspect or another may appear as realist, idealist, positivist, Platonist or Pythagorean.

By carefully selecting among Einstein's many writings, we can portray Einstein as holding just about any sort of philosophy. If we want to find falsificationist ideas in Einstein's writings, we need to look no further than a popular article he wrote in 1919, "Induction and Deduction in Physics." There he wrote (Einstein, 1919, his emphasis):[16]

> … while the researcher always starts out from facts, whose mutual connections are his aim, he does not find his system of ideas in a methodical, inductive way; rather, he adapts to the facts by intuitive selection among the conceivable theories that are based upon axioms.
>
> Thus, a theory can very well be found to be incorrect if there is a logical error in its deduction, or found to be off the mark if a fact is not in consonance with one of its conclusions. But the *truth* of a theory can never be proven. For one never knows if future experience will contradict its conclusion; and furthermore there are always other conceptual systems imaginable which might coordinate the very same facts.

[16] Translation from Janssen et al. (2002).

This popular article was published just when Popper recalled his excitement with Einstein's work in general relativity. Was this article an inspiration for Popper's falsificationism, we may wonder.[17] Popper later denied in correspondence with John Stachel that he knew of this article by Einstein.[18]

11.2 Einstein on General Relativity

In modeling his account of falsificationism on Einstein's work, Popper developed an oversimplified caricature of Einstein that emphasized those aspects amenable to Popper's views. We saw in Section 6 above that Popper praised Einstein for his "merciless criticism" of his own theories.

Einstein was surely as aware as anyone of the weaknesses of his ideas. However, truly novel advances in science cannot survive if their proponents are too ready to accept their falsification. Self-criticism must be moderated by prudent tenacity. If the proponent of the new theory does not persevere with a theory in trouble, no one else will save the theory.

This tenacity is quite evident in Einstein's work. In 1913, in collaboration with his mathematician friend Marcel Grossmann, Einstein produced the first sketch of his general theory of relativity, Einstein and Grossman (1913). The theory was, by his own later assessment, a failure in lacking general covariance, which was the formal property that, he held, generalized the principle of relativity. Yet he indulged the theory. Instead of abandoning it, he mounted arguments to defend it from this failure. He soon advanced his "hole argument," which sought to show that general covariance, later the signature property of general relativity, was physically inadmissible. It was only two years later, when the roster of identified failures of the 1913 theory had grown and became undeniable, that Einstein finally relented and abandoned the basic equations on his 1913 theory.[19]

Einstein's return to a generally covariant theory brought one of his greatest successes: he found, to his jubilation, in November 1915 that the new theory correctly recovered the anomalous motion of Mercury. We need not try to imagine what would have happened had

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[17] Popper reported in his interview with Whitrow (1967, p. 24): "… since Einstein, scientists do not seriously hold that their theories can be true or 'verified'."

[18] See Janssen et al. (2002, p. 220).

[19] The literature on this episode is immense. An early contribution is Norton (1984).

Einstein known that his earlier theory did not recover this motion. In June 1913, Einstein and his life-long friend, Michele Besso, exchanged a manuscript in which they calculated the motion of Mercury in the 1913 theory. They found that the theory recovered only 18” of the known 43” anomalous advance per century of the motion of Mercury’s perihelion. After November 1915, that gap between 18” and 43” would be sufficient reason to doubt and even abandon a new gravitation theory. In June 1913, it elicited no known negative comment from Einstein.[20]

Writing many years later to Max Born on May 12, 1952, Einstein conceded that he did not regard the three celebrated, predictive tests of general relativity as essential to establishing the theory. If his thinking was falsificationist in 1916, it was no longer so in 1952. He wrote:[21]

> Even if there had been no deflection of light, no perihelion motion and no redshift, the gravitational equations would still be convincing because they avoid the inertial system (the phantom that affects everything but is not itself affected). It is actually rather curious that humans are mostly deaf to the strongest arguments, while they always tend to overestimate the accuracy of measurements.

11.3 Einstein on the Miller Experiments

We find Einstein doggedly resisting falsification when Dayton C. Miller reported his success in experiments of 1925 at detecting an ether drift, in contradiction with Einstein’s 1905 special theory of relativity.[22] This was no nuisance result that could be easily ignored. Miller was then the President of the American Physical Society and was using components of the apparatus employed in Michelson and Morley’s famous experiment of 1887 that found no ether drift.

In his interview with Whitrow (1967), Popper presented Einstein’s reaction as conforming with Popper’s falsificationist ideas. Popper remarked (pp. 26-27):

> When D. C. Miller, who had always been an opponent of Einstein, announced that he had overwhelming experimental evidence against special relativity, Einstein at

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[20] For an account of this episode, see Janssen and Renn (2022, Ch. 6).

[21] Translation from Janssen and Renn (2022, p. 60).

[22] For more details of this episode, see Norton (2021, Ch.3, §6).

> once declared that if these results should be substantiated he would give up his theory.

Popper was correct, in so far as Einstein did concede the grave import of the experimental results, if they were correct. Einstein said as much in a popular article reacting to Miller's experiment. He wrote (1926, p.1, Einstein's emphasis):

> If the results of Miller's experiments were to be confirmed, then the theory of relativity could not be maintained. … Thereby, the principle of the constancy of the constancy of the speed of light would be *refuted.* [This principle] is one of the two foundational pillars on which the theory rests.

This concession immediately proved to be rather empty, for Einstein was adamant that the results of Miller's experiments would not be vindicated. The above remarks were followed immediately by (Einstein's emphasis):

> In my opinion, there is as good as *no probability at all* [*gar keine Wahrscheinlichkeit*] that Herr Miller is correct.

Einstein then gave a list of technical reasons for his strongly negative assessment. He concluded with an overall assessment given in terms of bets:

> In summary, I can say: if you, dear reader, wanted to use this interesting situation to make a bet, then it is better to bet that Miller's experiments will prove to be faulty, or that his results have nothing to do with an "ether wind." At least I would be quite happily ready to make such a bet.

12. Problems of Falsificationism

The principal reason for the failure of falsificationism, in my view, has been given above. Science is thriving and in a way that depends essentially on the use of inductive inference. There are cases in which the falsificationist model can be made to fit, such as Einstein's initial treatment of general relativity. There are many more cases in which, demonstrably, the model does not fit. In such cases, the best that can be said of falsificationism is that it is a recommendation for how scientists should practice their science. The falsificationist model has been visible to them for many decades. The scientists have almost universally not followed the recommendation and do not seem to have suffered for their decision.

In addition to this general problem, we can identify specific technical problems facing falsificationism. Most of them have already appeared in the literature, so a brief recounting here suffices.

12.1 Problems of Deductivism

That purely deductive relations among propositions suffices for an account of the rationality of science has been challenged by Reichenbach and his most successful student, Wesley Salmon.

Reichenbach (1935) argued, in response to the publication of Popper's *Logik der Forschung*, that inductive notions persist tacitly in Popper's account of conjectures and refutations. Since Reichenbach conceived of inductive notions as essentially probabilistic, he formulated his concerns in probabilistic terms.

First, he argued, Popper was mistaken to think that the refutation of a theory could be effected by purely deductive means. For no result in science can be known with absolute certainty. We are only assured of them probabilistically, even if the probability is high. That applies not just to theories but to the observational or experimental evidence that refutes a theory. The refutation must accommodate the probability of the refuting evidence in the analysis and is inductive in at least that aspect.

Second, the process of conjecturing new theories will also have probabilistic elements. When a theory has been judged falsified, the scientist can choose among a huge range of new proposals. Most them will be unpromising antecedently and the scientist will proceed with the one that seems most promising. These judgments of what is promising and unpromising are inductive notions that, in Reichenbach's conception, are assessed by the probability of the proposals considered.

Wesley Salmon's (1981) "Rational Prediction" identifies a fatal lacuna in the falsificationist account. That science makes predictions is important. The principal burden of the IPCC report on climate change is to sustain the prediction that continued human action will lead to further global warming. The whole point of the prediction is that it is rational to believe it and thus prudent to act on its basis. Salmon insists that falsificationism has no basis for concluding that accepting predictions such as these are rational. We only have a basis for accepting a prediction made by some theory if the theory is true or at least likely to be so. Falsificationism provides no basis for judging a theory true or likely to be so. The best we can say is that it hasn't been refuted yet. The IPCC would surely find it hard to convince governments internationally to take climate action on predictions with such a provisional status.

Inductive considerations are also hidden in the conditions for successful corroboration. Popper gave this summary of the conditions (1962, p. 36, his emphasis):

> [Corroborations] should count only if they are the result of *risky* predictions; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory--an event which would have refuted the theory.

These judgments require us to be able to make rational predictions about what "we should have expected" on the basis of some prior theory. Otherwise the predictions are not "risky." But just such rational predictions are what Salmon argues lies outside the grasp of falsificationism.

Finally, successful corroboration is similar in structure to successful hypothetico-deductive confirmation. In both cases, they arise when a theory makes a prediction that is subsequently verified. It follows that corroboration is beset by the same problem facing hypothetico-deductive confirmation: the relation is holistic. We learn only that the theory as a whole stands in some desired relation to the verifying evidence. The standard holistic complaint is that we do not have a mechanism for attributing the credit for the success to any particular component of the theory. For example, for several centuries, Newton's mechanics was successfully corroborated/confirmed through its verified predictions. Yet Newton's posit of an absolute state of rest deserved none of the credit.

This sort of holism is well-adapted to the case that impressed Popper greatly, the predictive testing of general relativity. It was a natural mode to choose. Einstein's gravitational field equations of 1915 consisted of ten, non-linear, coupled differential equations in the metric tensor. Einstein did not try to see how individual experimental results might support various components of this massively complicated set of equations. Support for them came from verifying their predictive consequences, as well as more general arguments of principle.

This holism contrasts with another case in which this sort of holistic appraisal is a poor strategy. The basic equations of Maxwell's electrodynamics, as they had been reformulated by the end of the nineteenth century, consisted of four coupled, linear differential equation in the electric and magnetic field strengths. The evidential support for these equations was not holistic. Rather, each term in the equations was supported by a particular, experimentally-found effect.

12.2 Problems of Dynamicism

Falsificationism locates the rationality of science in the process used to arrive at well-corroborated theories. That produces a curious problem. We can ask, what is the status of evolutionary theory in biology? An inductivist would pose the question by asking how well evolutionary theory is supported by the evidence. A falsificationist would have to dismiss the question as ill-posed, since the question presumes inductive relations of support. Such inquirers would be told that they should ask a different question: how well has evolutionary theory passed tests in its history. Such inquirers should be forgiven for objecting, as I would, that the question asked is independent of the particulars of the history. We just want to see the evidence for evolutionary theory displayed, so that we can assess the strength of support. It does not matter how that evidence was secured—whether it was by a painstaking process of conjectures and refutations or by something haphazard.

This last difficulty is related to a problem that is an artifact of the dynamical approach: the notion of an *ad hoc* hypothesis. According to it, whether some hypothesis can be corroborated by its verified consequences depends on how the scientist came to propose the hypothesis. Was it "cooked up" artificially, since the scientist already knew of the verified consequences in advance? Then it is deprecated as *ad hoc* and inadmissible in Popper's (1962, p. 37) account. Or was it generated independently of this knowledge? Then it is admissible. The result is that the same hypothesis may be deemed viable or not according to the history of how it was produced.

This problem does not arise for inductivists. For them, the question is merely how well the hypothesis is supported by the evidence. If the hypothesis was "cooked up" *and* that is somehow harmful, that harm should be apparent in the weakness of the inductive support. For example, the hypothesis that we are momentarily at rest in a nineteenth century electrodynamic ether may be *ad hoc* or not according to whether the proposer knew of the repeated null results of nineteenth century ether drift experiments. For the inductivist, the hypothesis is rejected as ill-supported by the evidence, independently of how the hypothesis came to be proposed and what the proposer knew.

12.3 Problems of Demarcation

The idea that we can demarcate science by the criterion of falsifiability is appealing for its great simplicity. There is no need to engage in the details of some dubious investigation. We just ask if the resulting theory is falsifiable; and if it is not, Popper's cudgel descends on its head.

It is a hard-won life lesson that something that is too good to be true is likely not true. And so it is with this demarcation criterion. The question of whether some investigation is proceeding in a scientifically responsible manner has no simple answer, because scientific investigations are not simple. Proponents of evolutionary theory in biology paid little attention to making novel prediction. They had no need of them since they supported their theory with massive compilations of evidence. We saw in Section 10.1 above, that this opened evolutionary theorists to easy criticism by opponents of evolutionary science, who decried the theory as unfalsifiable.

There are many further ways in which the demarcation criterion in troublesome. At one extreme, it is too permissive. In dubious forms[23] of parapsychology, psychics are attributed the ability to view all manner of things remotely and even to assist the police in murder investigations. These dubious forms do make falsifiable prediction and are categorized as science by the demarcation criterion. Predictions that truly test the psychics commonly fail. That does not matter to the criterion. The theories made testable predictions.

At the other extreme, the demarcation criterion is too strict. It is easy to imagine quite respectable scientific investigations in which prediction is impossible. Consider, for example, a very thorough investigation of some recently discovered archaeological site. All the evidence of the site is collected painstakingly and cataloged before the site is leveled for new construction. That evidence is then used to support a general theory about the site. Charred bones are evidence of the inhabitants' diets, for example. At no point were predictions made in developing the general theory; and no novel prediction is possible since all possible evidence has been collected and identified. The theory is unfalsifiable by novel predictions and thus fails to be scientific, by the standards of the demarcation criterion.

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[23] Why dubious? It is because they depend on supposing channels of communication that lie outside all known science.

Carl Hempel (1950, pp. 47-48), in a mildly worded analysis, identified a fatal technical problem for the criterion: what is judged scientific by the criterion is not preserved under simple logical operation such as negation and conjunction of propositions; and (I add) disjunction and material implication. It follows that apparently entirely innocuous suppositions or inferences can move investigators in and out of the realm of science, as demarcated by the criterion. Examples are easy to find. The proposition:

All electrons are spin one half.

is falsifiable and thus scientific. We may contemplate its negation without a concern that we commit some transgression:

There are electrons that are not spin one half.

This proposition is not falsifiable since we cannot check all electrons. We are no longer contemplating a scientific hypothesis. Deductive inference can also lead us out of science. Here is a celebrated, falsifiable proposition in gravitational astronomy:

The planet Vulcan, with its suitably computed mass and orbit, allows Newtonian theory to account for the anomalous motion of Mercury.

It is falsifiable since it was falsified when Vulcan failed to appear in the computed position. A deductive consequence of the proposition is:

There are ways that Newtonian theory can account for the anomalous motion of Mercury.

This proposition is no longer falsifiable since it leaves unspecified which resources Newtonian theory might use to account for the anomalous motion. It is not a scientific proposition.

To an inductivist, all these difficulties derive from a mistaken supposition that was made at the outset; and they disappear when that mistaken supposition is discarded. The mistaken supposition is that it is possible to find a simple formula that demarcates science from non-science. Within falsificationism, the supposition is natural since the criterion arises from an already oversimplified account of scientific rationality: conjecture, predictive testing and refutation. If a theory is not falsifiable, then it is precluded from participating in this dynamic and thus cannot partake in scientific rationality.

Since inductivists have no special commitment to this dynamic, they have no need for such an oversimplified criterion. For inductivists, the pertinent question is just how well some theory or hypothesis of interest is supported by the evidence. At this coarse level of description, the question looks simple. However, it is far from simple, since the modes of inductive support are complicated and diverse.

That some hypothesis or theory is not falsifiable can have a role in the inductivist account. It is an indication that something is amiss in the way the theory has been formulated. Might it be that the theory is so contrived that it cannot be well supported by the evidence? We might imagine that someone posits the existence of a fifth fundamental force of nature. But that force, they add, manifests only in another parallel universe that is inaccessible to us. The posit cannot be falsified; and correspondingly we can find no supporting evidence for it. The failure is of the impossibility of evidential support.

13. The Problem of Induction

13.1 Induction in the 1930s

In the 1920s and 1930s, when Popper was devising his falsificationist account of the rationality of science, induction was the glory of science and the scandal of philosophy. For science was inferring inductively without hesitation and thriving for it. However, philosophers were beset by the argument of the problem of induction that, they felt, had shown definitively that inductive inference is unjustifiable.

In retrospect, the failure lay with the philosophers, not the scientists. The philosophers' unquestioned assumption was that a theory of inductive inference must posit universal rules that distinguish the good inductive inferences. That assumption, I believe, was already fatal. Yet the situation was worse in this early era. Popper and his contemporaries then employed a conception of induction inference that was already lagging far behind the practices of science. The "principles of induction" with which they worked were minor variations on the hopelessly naïve argument form of induction by simple enumeration. Carnap's (1936/37) massive "Testability and Meaning" gave an elaborately technical, formal account of confirmation relations that, on first glance, looks rich and impressive. On a second look, however, the basic conception of confirmation proves to be thin, but its naivete is hidden by elaborate flourishes of pointless logical formalism. Reichenbach's treatment of induction probabilistically is the most promising. However even it falls short in spending its efforts on a formal analysis of probability while failing to establish that his probabilistic inferences are those actually used by scientists in real examples.

13.2 The Material Theory of Induction

The problem of induction can be dissolved if we abandon the foundational assumption, unquestioned in the 1930s, that inductive inference is governed by universal rules. The material theory of induction abandons universal rules or principles or schemas of inductive inference. In the place of these universal rules, the material theory identifies the warrant for each individual, inductive inference in some particular fact or facts specific to the domain of the inference. How these warrants arise has been described extensively in *The Material Theory of Induction* (Norton, 2021) in general terms and in many examples. It may be helpful to include one example here as an illustration of the theory: the analogical argument of the first four chapters of Darwin's *Origin* mentioned in Section 10.1.

The evidence for Darwin's analogical inference was the use of artificial selection by pigeon breeders. They use artificial selection to produce desired traits in new generations of pigeons. By analogy, Darwin infered that, elsewhere, a selection by nature through scarcity of resources will lead to new populations with traits better adapted to their environment. What warrants this inference? In formal approaches, we would try to represent the evidence and the theory supported in more abstract terms and then show that the inference fits with some universal abstract schema. Norton (2021, Ch.4) explores the range of formal schemas employed in the literature on analogical inference. None suffice and there is little hope that any ever will. As the chapter shows, each new instance of analogical inference in science may require some further adjustment and adaptation of the existing schemas. The process is ever-escalating with no apparent end.

The material theory looks elsewhere for the warrant for each specific analogical inference. The warrant resides not in a general formal rule, but in a background fact peculiar to the domain of the inference. That fact for this Darwinian case is just that domesticated pigeons and animals more generally share heritable traits. Moreover, both artificial and natural processes can affect differentially the survival of the carriers of some heritable trait according to whether they carry the trait.

13.3 The Dissolution of the Problem of Induction

Since the material theory of induction employs no universal schemas or rules, Hume's and later formulations of the problem of induction cannot be mounted. There is no factual principle of induction whose justification requires the principle to be applied to itself. There is no infinite hierarchy of principles of induction each justifying the principles one level down, while each principle becomes more abstract and fanciful as we proceed up the levels. The traditional arguments used to formulate the problem of induction can no longer be stated. The problem is dissolved.

How is inductive inference justified within the material theory? The theory does not seek to justify inductive inference after the manner of formal theories. There, it is assumed that there is some singular thing that justifies induction. That thing might be a general principle of induction. Or it might be some overarching fact about the world that can be compactly stated, such as an assertion of the uniformity of nature, where the sense of uniformity is inevitably left maddeningly vague.

The material theory of induction does not seek some elusive, singular thing that, at a stroke, justifies induction. It has no need of such a thing. Instead, the material theory just asks what background fact justifies each particular instance of inductive inference. There is no single background fact warranting all inductive inferences. The warranting fact will, in general, be different for each instance of inductive inference.

In an emerging science, some propositions will not have strong inductive support with identifiable warranting facts. Their place in the science is only provisional, as useful hypotheses still awaiting inductive support. In a mature science, however, every proposition is inductively supported with some suitable warranting fact. That includes all the warranting facts themselves. What results is a massively tangled, non-hierarchical network of relations of inductive support. If we pick any particular proposition in a mature science, we will be able to identify the evidence that supports it inductively and the material fact that warrants the inductive relation of support.

Since that holds for every proposition in a mature science, there is no need to ask for anything more. The inductive support for the science is just the totality of these individual relations of support. The justification of induction does not reside in some singular principle. It is distributed over all of a mature science in the many individual warrants for each of its inductive inferences.

These last remarks are a brief sketch of how the material theory of induction dissolves the problem of induction. The full account is given in *The Large-Scale Structure of Inductive Inference,* Norton (2024, Ch. 6), with the remaining chapters providing support for components of the general argument offered in Chapter 6. There are many more details in the general argument. It addresses the natural worry that the problem of induction returns in the material theory in some other guise. Perhaps, we may worry, that there is a harmful circularity or an unsustainable infinite regress. That neither arises is shown by a careful analysis of the large-scale structure of inductive inference provided in Norton (2024), in both general terms and many examples from the history of science.

Once we understand inductive inference materially, induction can be both the workhorse of science and the glory of philosophy.

14. Conclusion

In the 1920s, the world emerged from the darkness of the Great War to End All Wars and the misery of the flu epidemic. It was a new era of freedom, innovation and excitement. People were liberated everywhere to explore formerly unthinkable, audacious ideas. In Popper's hometown, the logical positivists of the Vienna Circle proclaimed in their manifesto (Hahn et al, 1929) that the ponderous and obscure pronouncements of the old metaphysics could be wiped away as unintelligible nonsense with a single, bold idea: the verifiability criterion of meaning. They promised a new era in philosophy. The precision of formal logic would now provide an assured framework in which all problems of philosophy could be solved by objective, logical analysis.

This newfound freedom was encouraged by the physics of Einstein that had so impressed a young Popper. The success of Einstein's general theory of relativity was iconoclastic. The age-old certainties of Euclid's geometry and Newton's mechanics were overthrown. Gravity was not a force after all but a strange new manifestation of the properties of a non-Euclidean space and time.

This was the milieu in which Popper developed his account of the rationality of science. The long-standing tradition was that science was an inductive enterprise. But if philosophers could not justify induction, then why not do away with it? Why not find a new, better account that drew on deductive inference alone? The old ideas were falling and being replaced for the better by bold, new ones. The Vienna Circle had employed their simple, powerful verifiability criterion to wipe away all manner of metaphysical nonsense. Popper could do something similar with his falsifiability criterion to demarcate science. At a stroke, he could wipe away pretend sciences like Marxism and psychoanalysis.

As time passed and the excitement of novel thought faded, the evident flaws of the logical positivists' philosophy could not be overlooked. The verifiability criterion was too simple and too crude. Opponents of inflated metaphysics needed to engage more closely with their targets to sustain their objections. Messier debates could not be avoided. The excitement over the power of formal logic in philosophy faded. Philosophers found themselves embroiled in problems peculiar to the logic that distracted them from engagement with real philosophical problems.

Popper's falsificationist account of the rationality of science was a product of this earlier era, in which bold, simple solutions could be offered for complex problems. Where others in his time made compromises, Popper's account was a principled and rigorous response the problem of induction that so vexed his contemporaries. For that, it deserves much credit. For that, it deserved to rise. It had its limits and its fall was inevitable. It was too dependent on one prominent, but anomalous example, Einstein's predictive successes with general relativity. It could not otherwise be sustained as a serious account of how science did proceed without inductive inference or a proposal for how it should proceed without it. Scientists largely continued to do what they had always done and had to do: they continued to use inductive inferences to determine how their theories were supported by the evidence.

One component of Popper's account lives on as a reminder of the exuberance of this earlier era. The verifiability criterion was appealing in giving logical positivists an easy way to cut off debate and dispatch their metaphysical opponents. Popper's demarcation criterion for science has the same appeal. One's scientific opponent can be dispatched with a simple riposte. Just as the verifiability criterion of meaning was far too simple a solution to a hard problem, the same is true of Popper's demarcation criterion. It can be used to repudiate what is otherwise responsible science; and to elevate as scientific what are otherwise irresponsible fantasies. The criterion survives in modern discourses because it is too easy to wield it and too easy to imagine, mistakenly, that it has a solid foundation in some deeper body of falsificationist theorizing that somehow survives from an earlier era.

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