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History of Science and the Material Theory of Induction: Einstein's Quanta, Mercury's Perihelion

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The use of the material theory of induction to vindicate a scientist's claims of evidential warrant is illustrated with the cases of Einstein's thermodynamic argument for light quanta of 1905 and his recovery of the anomalous motion of Mercury from general relativity in 1915. In a survey of other accounts of inductive inference applied to these examples, I show that, if it is to succeed, each account must presume the same material facts as the material theory and, in addition, some general principle of inductive inference not invoked by the material theory. Hence these principles are superfluous and the material theory superior in being more parsimonious.

1. Introduction

History of science has presented special difficulties for me, when I approach it as a philosopher of science with an interest in evidence and inductive inference. It may be very clear, at a visceral level, that this piece of evidence has provided some scientist very strong evidence

[1] I am grateful for helpful discussion to Gerd Grasshoff, Kaerin Nickelsen and the participants in a block seminar, May 31-June 1, 2007, at the Institut für Philosophie, Wissenschaftstheorie und Wissenschaftsgeschichte, Universität Bern; and for helpful remarks from the referees and editor of this journal.

for that hypothesis or theory. Generally, when I try to apply one of the many standard accounts of inductive inference to the example, I can get one or other account to fit well enough, eventually. Often the fit is Procrustean, succeeding largely because of the original visceral instinct and not from any special powers of the account. And when the accounts do seem to work better, the success is unsatisfying. I do not seem to have vindicated the evidential claim in a principled way. Rather I feel more like a hypochondriac who had gone "doctor shopping" until I finally found a doctor willing to give me the diagnosis I wanted.

These problems have led me to a different way to think about inductive inference. My "material theory of induction" (Norton, 2003, 2005) was devised precisely to enable historians of science to offer philosophically principled evaluations of evidence claims made by scientists. Its goal is to give an account of inductive inference that applies in all cases, so that there is no longer a need to "doctor shop"; and it is to do it in a way that does not require elaborate reconstruction of the scientists' evidence claims. The central claim of the material theory is that inductive inferences are not licensed by universal formal schema. Rather, their warrant is ultimately traceable to matters of fact. Since those facts vary with the domain, there can be no universal logic of induction. Thus, our failure over millennia to find the One True Universal Logic of Induction is explicable and expected. That logic was never there to be found.

My purpose in this paper is to show how the material approach to induction can be used in real historical cases and to display why I believe it is superior to any other account. To do this, I will take two case studies, to be reviewed in Section 2 below: Einstein's thermodynamic argument for light quanta of 1905 and his 1915 recovery of the anomalous perihelion motion of Mercury by general relativity. In Section 3, I will apply the material theory of induction to the two cases and identify the material facts that I believe license Einstein's evidence claims. In Section 4, I will compare the analysis of the material theory with those given by leading accounts of inductive inference, when applied to these two cases. In each case, I will establish that these accounts can accommodate the two cases only in so far as we already assume the material facts required by the material theory. In this regard, whatever these accounts add is superfluous to the justification of the Einstein's evidence claims, for that justification can already be had from the material facts. Finally in the concluding Section 5, I will review the virtues of the material theory. Other accounts of induction incline us to homogenize, to see many inductive inferences fitting as large as pattern as possible. The material theory of induction encourages us to treat inductive inference individually and we see that the two cases, in spite of their formal similarities, have rather different strengths.

2. The Cases

2.1 Einstein's Quanta

On 18th March 1905, Einstein sent the *Annalen der Physik* the first of a series of papers that made that year his *annus mirabilis*. In this first paper (Einstein 1905), "On a Heuristic Point of View Concerning the Production and Transformation of Light," he advanced his light quantum hypothesis, that heat radiation of high frequency ν behaves as if it consists of independent, spatially localized quanta of energy E = hν, where h is Planck's constant. While his use of the photoelectric effect to support this hypothesis is widely known, my concern here is with a much more ingenious and telling argument that forms the centerpiece of the paper and is laid out in its Section 6.[2]

The evidential basis of the argument is an expression for the volume dependence of the entropy of a system of heat radiation of energy E and high frequency ν. If S is its entropy when the system occupies volume V and S_0 its entropy when the system occupies volume V_0, then

$$S - S_0 = k\,(E/h\nu)\ln(V/V_0) \qquad (1)$$

where k is Boltzmann's constant. It is important that this result derives from macroscopic measurements. The experimentalists had made precise measurements of the distribution of energy over the different frequencies of heat radiation. Wien had fitted a well-known distributions formula to the experimental results that worked well for higher frequencies. Using such formulae, any competent thermodynamicist could infer the corresponding entropy distributions, such as (1).

Einstein then used this macroscopic formula to infer directly to the microscopic constitution of the radiation in an argument that, in my view, is the boldest of his corpus of 1905. In an earlier section, Einstein had recapitulated what then seemed to be a superficial truism. Imagine that one has a thermal system that consists of many, independent moving points—n,

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[2] For further discussion of this argument and a discussion of its relation to Einstein's work in statistical physics from this period, see Norton (2006).

say. Such is the constitution, for example, of an ideal gas. If one has a volume V_0 with one point in it, the probability that the point will be found in a subvolume V is just V/V_0. Since the points move independently, it now follows that the probability that all n are to be found in the subvolume is just

$$W = (V/V_0)^n \tag{2}$$

This formula gives the probability that the volume of the thermal system will spontaneously fluctuate to the smaller volume V. For a system of molecules comprising an ideal gas, n will be of the order of 10^{24} for macroscopic samples of gas. So the probability of any significant volume fluctuation is unimaginably small. Whether small or not, the probability of the transition is related to the entropy of the initial and final states by what Einstein called "Boltzmann's Principle":

$$S - S_0 = k \ln W \tag{3}$$

Applying Boltzmann's Principle (3) to (2) for an ideal gas of n molecules immediately returns the expression $S - S_0 = kn \ln (V/V_0)$ for entropy of the gas, from which, as Einstein shows in a footnote, the ideal gas law follows.

Proceeding now to the case of heat radiation, Einstein combined the expressions (1) and (3) to conclude that the probability that a volume of radiation V_0 will spontaneously fluctuate to the subvolume V is

$$W = (V/V_0)^{E/h\nu} \tag{4}$$

Einstein thought the import of this last formula obvious. He wrote without any intervening text:

> From this we further conclude:
> Monochromatic radiation of low density (in the region of validity of Wien's formula) behaves thermodynamically as if it consisted of energy quanta of size hν that are independent of one another.

The thought is clear. The similarity of expressions (2) and (4) led Einstein to infer that the radiation consists of $n = E/h\nu$ independent points; that is the energy E is divided into n independent quanta of size hν. The only hesitation in Einstein's inference is the "behaves … as if" qualification. That qualification is reflected in the "heuristic" of the title, but it is dispensed with elsewhere, such as in the introductory section, with mention of the full array of evidence of the paper.

While Einstein's inference to light quanta above seems irresistible, we should recall that its conclusion directly contradicted the great achievement of nineteenth century optics and electrodynamics, the wave theory of light.

2.2 Mercury's Perihelion

In November 1915 an exhausted Einstein neared the end of his struggle find the gravitational field equations of his general theory of relativity. His final reflections were communicated rapidly to the Prussian Academy in weekly installments, each correcting errors in the previous installment. His efforts were made all the more urgent by the knowledge that Hilbert in Göttingen was working on the same problem. By November 18th, in the third of these communications, Einstein (1915) had in hand sufficient of the final equations to be able to publish the solution to an outstanding puzzle in gravitational astronomy.

Newtonian gravitation theory entails that planets orbit the sun in elliptical orbits. Careful measurements of these orbits had shown that their axes move ("precess") very slowly. In the case of Mercury, the motion is an advance in the direction of the planet's motion of over 500 seconds of arc per century in the orbit's perihelion, the point of closest approach to the sun. All but about 40 seconds of this perihelion motion is explicable in terms of the tiny gravitational tugs of the other planets. This residual 40 seconds of arc, the anomalous motion of Mercury's perihelion, was the largest anomaly among the planets and was recognized in 1915 as a familiar discrepancy.

What Einstein found to his absolute delight that November was that his general theory of relativity predicted precisely this anomalous motion. The figures he quoted in 1915 (p. 938) were that his theory predicted an advance of 43 seconds or arc per century and that this lay comfortably within the range observed by the astronomers of 45 seconds, plus or minus 5.[3]

It would be an understatement to call this achievement an evidential coup. Historically, it functioned as the decisive sign of the success of Einstein's theory. Overnight it set a new standard of empirical adequacy for fledgling gravitation theories. Prior to November 18, 1915, it

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[3] See Earman and Janssen (1993) for an account of Einstein's computation and its astronomical background. For more on Einstein's discovery of his field equations and the role of this episode in it, see Norton (1984, §8) and the more expansive accounts of Renn (2007).

was no special defect of such a theory if it did not predict precisely this 43 seconds of arc. Leading contenders, including Einstein's own "Entwurf" theory of 1913, did not and with no ill effect. After that date, the inability to predict this figure was tantamount to the failure of the theory.[4]

In his communication of November 18, Einstein (p. 831) described his result as follows:

> In the present work, I arrive at an important confirmation of this most radical theory of relativity; that is, it turns out that the secular rotation of Mercury's orbit in the direction of the orbital motion found by Leverrier, which amounts to about 45" per century, is qualitatively and quantitatively explained, *without having to posit any special hypotheses at all*.[5] (my emphasis)

For our purposes, the essential remark is the final clause on the lack of special hypotheses.[6] For that is what was then and remains today truly remarkable about Einstein's treatment of the motion of Mercury. His theory had been essentially uniquely fixed by a series of demands remote from any of the specifics of planetary motion, most notably general covariance of the equations. Had the resulting theory failed to accommodate the motion of Mercury, there would have been no recourse.

As Einstein then knew, there were many ways to accommodate the anomalous motion of Mercury if special assumptions were allowed. A simple example from the time illustrates this.

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[4] This transition is documented in Norton (1992, §16) in the case of Nordström's theory of gravitation of 1912. In 1914, Nordström could proudly proclaim that "the laws derived [from his theory] for [free] fall and planetary motion are in the best agreement with experience." In 1917, in an otherwise sympathetic review, Laue derived the same theory's formula for perihelion motion and noted that it predicted a retardation, not an advance. The blow was severe enough for Laue not even to bother to compute the actual value predicted for Mercury's perihelion motion, lamenting the "impossibility of explaining its perihelion motion."

[5] Einstein's footnote here emphasized the importance of his achievement: "E. Freundlich has recently written a noteworthy paper on the impossibility of satisfactorily explaining the anomalies of Mercury's motion on the basis of Newtoian theory (*Astr. Nachr.* 4803, Bd 201. Juni 1915)."

[6] "ohne dass irgendwelche besondere Hypothese zugrunde gelegt werden müsste"

Newton had already found in his *Principia* that any deviation from an inverse square law of attraction for gravity would manifest as a rotation of a planetary orbit. In Book 1, Prop. 45, Cor. 1, he considered a power law in which force dilutes with distance r as $1/r^{2+\lambda}$ and applied it to the case of planets moving in near circular orbits. He found that the modified law produces orbits that complete in $360/(1-\lambda)^{1/2}$ degrees, which means that the planet returns to perihelion, the point of closest approach, after this many degrees. For an inverse square law, $\lambda = 0$ and the orbit completes in 360^o. That means it is stationary. If λ is slightly greater than zero, the orbit completes in slightly more than 360^o and we have the case of an advancing perihelion. Hall in 1894 and Newcomb in 1895 had proposed just this modification as a way of accommodating the anomalous motion of Mercury. A value of $\lambda = 0.0000001574$ would suffice.[7] (For this proposal and others discussed below, see Earman and Janssen, 1993, §3; Zenneck, 1901, §15.)

What this example shows is that the anomalous motion of Mercury could be accommodated if one was prepared to introduce special hypotheses, such as adjustments to the inverse square law. Many other special hypotheses are possible. Since the bulk of Mercury's 500 second of arc per century perihelion motion was due to the gravitational influence of the other planets, it was easy to posit another as yet unknown planet "Vulcan" as responsible for the residual motion.

Needless to say, things weren't quite that easy. Hall's hypothesis of an adjusted inverse square law fails theoretically in that it is not a relativistic law and observationally in that it affords too large a perihelion motion to Venus and Earth. And Vulcan failed to oblige by being visible to telescopes in its computed location. These failures mark the beginning of a series of retrenchments that continue today. The astronomer Seeliger in 1906 rescued the missing mass of Vulcan by supposing that it was distributed in a diffuse band of intra-Mercurial matter, visible as the zodiacal light. Exploration of admissible theories that can also accommodate Mercury's perihelion motion continues in the context of the parametrized post-Newtonian ("PPN") formalism. (Will, 2007; Misner, Thorne and Wheeler, 1973, Ch. 39.)

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[7] For then one orbit requires $360/(1-0.0000001574)^{1/2} = 360 + 0.000028332$ degrees. Since the Mercurial year lasts 87.97 days, there are 100 x 365.25/87.97 = 415.198 Mercurial years in a century. Over this time the angular excess of 0.000028332 degrees accumulates to 0.000028332 x 3600 x 415 = 42.35 seconds of arc.

The PPN formalism deals with a class of metrical theories of gravity, judged admissible by present theoretical and observational standards. When applied to the weak gravitational field of the sun, the equations of all these theories collapse into a single set of equations with eleven parameters, the relativistic generalizations of Hall's λ parameter. The different theories are then distinguished by the values assigned to these parameters. Typically theories are associated with a range of parameter values. Just has Hall had to fix a value for his λ to accommodate the known motion of Mercury, for each theory in the PPN formalism, particular values of the parameters must be chosen in order to secure compatibility with observations within out solar system. Einstein's general theory of relativity remains distinctive among these theories in that it has no free parameters whose values must be "tuned by hand" to allow the theory to accommodate observations. It requires, as Einstein reported, no special hypotheses. These investigations continue with the remarkable outcome that, whenever a clear decision between Einstein's theory and a competitor becomes possible, Einstein's theory wins.

3. Material theory of Induction

According to Einstein, the measured entropy of radiation inductively supports the light quantum hypothesis; and the observed perihelion motion of Mercury is strong inductive evidence for his general theory of relativity. How are we to assess whether these inductive claims are correct? According to the material theory of induction (Norton, 2003, 2005), we determine the validity of an inductive inference not by displaying its conformity to some universal inductive inference schema. We do it by displaying a material fact.

In a narrower context, one has a similar outcome from the application of Lewis' (1980) "principal principle." It leads us to conform our beliefs to objective chances whenever they can be had. So, if we are inferring inductively over the outcomes of games of chance or the outcomes of radioactive processes, then those inferences would conform to the probability calculus, for, in both cases, the outcomes are governed by objective chances.

This example generalizes in ways Lewis might not have endorsed. We should always, I urge, let the material facts prevailing dictate how we reason inductively. If those material facts do not contain objective chances, then it is no longer clear that our inductive inferences ought to be governed by the probability calculus. Indeed I have identified cases in which the material facts obtaining are such that we cannot responsibly conform our inductive inferences to the probability calculus. (Norton, 2007, §8.3; forthcoming, forthcoming a)[8]

3.1 The Material Facts

What material facts govern Einstein's two inductive inferences? In general, the prevailing material facts are quite varied in form. In this case, however, they have the same general form, although we shall later see important differences beneath this similarity:

> (M) Were the world governed by another hypothesis or theory (other than the light quantum hypothesis/general theory of relativity), it is very unlikely that the evidence (volume dependence of entropy of radiation/Mercury's anomalous perihelion motion) would obtain.

Once this proposition (M) is accepted, I shall argue below that the inductive inferences in question are warranted. Before doing so, some elucidations of (M) are in order.

3.1.1 (M) is a Modal Fact

(M) is a factual claim about the world, even though it does assert something rather more exotic than everyday facts. It asserts what might happen were other hypotheses or theories to govern the world. In this regard, (M) is a great deal more speculative than other examples of material facts that I have used elsewhere (2003, 2007). These examples include the fact that all samples of one chemical element, generally speaking, have the same melting point. That licenses an inference from the melting point of one element to all. Another example is the law of radioactive decay, which licenses the assigning of probabilistic degrees of inductive support to the occurrence of decay events for radioactive atoms.

An objective reading of the term "(un)likely" is intended in (M). It is akin to the objective reading such as appears in the factual sentence

"It is very unlikely that a radioactive atom remains undecayed after a period of 10 half-lives."

Here the term "unlikely" is part of the expression of a fact about radioactive decay. The complication is that the term "unlikely" can also be given an epistemic reading. We might say

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[8] The examples are indeterministic physical systems whose complete physical specification fails to provide any physical chances for the different futures admitted by a given present state.

"We judge is unlikely that the radioactive atom remains undecayed after a period of 10 half-lives."

Of course the two sentences are closely connected. What warrants the second epistemic claim, according to the material theory, is just the first objective fact about radioactive decay.

(M) is a modal fact; it is a fact about what is possible. Modal facts are familiar in science and often quite untroubling. It is a fact that the earth could have had two moons, for example. (M), however, goes beyond these examples by making assertions in the larger domain of possibility that arises when we ask which laws other than our physical laws (light quantum, general relativity) are possible.

(M) presumes that there is a disciplined sense of possibility for this larger domain. In the case of gravitation theory, Einstein expected that viable, alternative gravitation theories would likely be field theories in which gravitational action diminishes with distance and that this diminution would, to first approximation, conform to an inverse square law. These expectations about the broader realm of possible theory are less commonly expressed explicitly. They may be articulated, however, under more extreme conditions. Such conditions arose for Einstein in his later decades as he faced the challenge of the quantum. His well-known critique of quantum theory was based on the assumptions that a viable matter theory had to be separable and local. That is, the state of a system here is independent of the states of systems elsewhere (separable); and that effects between systems do not propagate instantaneously (local). Theories of this type populate the realm of possibility (See Howard, 1985.)

Nonetheless, I find (M) worrisome. It does reduce the cogency of Einstein's inferences to something more speculative than those supported by the law of radioactive decay. As is detailed in Section 3.2, I believe (M)'s acceptance is due more to instinct than systematic analysis. These instincts can err. Einstein's insistence on separability and locality in the quantum context are now generally regarded as errors.

However the presumption of (M) seems unavoidable in an explication of Einstein's inductive inferences. Indeed I will argue in section 4 below that no other account of inductive inference succeeds without it. All other accounts that apply to these two cases must also presume (M) and add further presumptions deriving from the specific approach they use. A virtue of the material theory of induction is that it forces us to make (M) explicit, so that the strength or weakness of the inductive inference is apparent. We shall see below that other accounts tend to obscure (M) beneath other clutter, while still fully relying upon it.

3.1.2 (M) is Vague

Second, the sense of "likely" involved in (M) is not the abstract theoretical notion of probability and measure theory. It is the rough and ready sense of ordinary judgment, with no guarantee that it is used consistently. It is the same notion that arises in "It is very unlikely that there are reptilian aliens from a distant planet offering to cure cancer with their advanced science for all patients willing to send a money order to the postal box indicated in the advertisement." The notion is instinctive and primitive. The idea that it is really a disguised form of the quantity discussed within the calculus of probability can only seem natural to a philosopher with an overdeveloped sense of rigor.

This vagueness of the notion of "likely" answers a concern that its use already commits us to universal inductive inference schemas of exactly the type denied by the material theory.[9] While the notion may support many schemas, we can address the concern by considering the simplest of them:

> *Very likely,* P.
>
> Therefore P in this case.

While this certainly looks like a universal schema, the appearance is deceptive. The strength of the inference and even the character of the uncertainty is quite context dependent. We can see this if we consider a few sample inferences that fit the form of the schema.

> *Very likely,* ten cards dealt from a well-shuffled deck will not be ace, two, …, ten of spades.
>
> Therefore, these ten cards dealt from a well-shuffled deck will not be ace, two, …, ten of spades.

Compare it with

> *Very likely,* ten cards dealt from a well-shuffled deck by an accomplished stage magician will not be ace, two, …, ten of spades.

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[9] I am grateful to Thomas Kelly to directing my attention to this concern.

> Therefore, these ten cards dealt from a well-shuffled deck by an accomplished stage magician will not be ace, two, …, ten of spades.

While the inferences are formally the same, the sense of "likely" and the type of inductive risk taken in accepting them differ greatly. In the first, "likely" merely derives from familiar probabilistic facts arising in stochastic processes. In the second, "likely" embodies factual presumptions about the interest and ability of a stage magician to deceive us and about our ability to observe sufficiently astutely to detect the deception. Because of their different factual bases, the two inferences differ in strength and in the character of the inductive risk.

Another example illustrates a different sort of uncertainty again in the same schema. In non-inflationary big bang cosmology, it is antecedently judged unlikely that, among the many orders of magnitudes of values possible, the density of matter emerging from the big bang should take on the unique "critical" value needed ensure that the geometry of space is Euclidean. That is,

> *Very likely,* the density of matter will not be the critical value.
>
> Therefore the density of matter will not be the critical value in our universe.

The sense of "likely" invoked here is quite unlike the probabilistic sense of well-shuffled decks. There are no physical processes prior to the big bang that could randomize the selection of the matter density. It just has the value it happens to have. We might imagine a creator throwing darts at a board of possible values. But that is a parable that inserts probabilities by fanciful metaphor; there is no physical sense in which we have probabilities across the many different worlds judged possible in cosmological theory.

In sum, the one word "likely" is used to capture many different senses of near certainty. The inability of our language to find compact terms that can label differentially all these senses gives the illusion of a single, universal schema, whereas there are many with differing meanings of "likely" specialized to the facts of their domains.

3.2 Why Einstein Would Accept the Material Facts

Why Einstein would accept (M) seems a matter of comparable instinct, but this time it is the instinct of an accomplished theorist who knows how hard it is to get any credible physical theory to give the result wanted, let alone in a simple and uncontrived way.[10] Take the probability formula (4). Certainly it would seem hopeless to try to recover it from the then dominant wave theory of radiation, where volume fluctuations must come from interference effects. If we have a system with energy E equaling hν, how are we to recover the result of (4) that, with probability 1/n, the system will spontaneously fluctuate to 1/n th its volume? That is the natural behavior of a single localized particle, for there is always a chance of 1/n that the particle is in some nominated 1/n th subvolume. If the system has any other constitution that differs from that of a localized particle, those differences ought to interfere with the recovery of the result. This reflection is somewhat superficial. But whatever Einstein had in mind has to be quite spare, if it is to capture his presumption that readers would be able to pass immediately from the probability formula (4) to the light quantum hypothesis.

In the case of Mercury's perihelion, Einstein's boast of not needing any special assumptions suggests why he would accept (M). The accounts then known that accommodated Mercury's motion all had free parameters. Hall's λ could take any small value. Seeliger's masses could be distributed in many ways. If those hypotheses were the correct ones, then, aside from the fact of Mercury's anomalous motion, there is no antecedent reason favoring one value or distribution over any other. It seems quite unlikely that the actual value or distribution should then be just the one that accommodates the anomaly. There were also theories that did not have these adjustable parameters. They gave fixed results and those results did not match Mercury's perihelion advance of 43 seconds of arc per century. The theories included Newton's original theory, Nordström's theory of 1912, Einstein and Grossmann's "Entwurf" precusor to general relativity of 1913; and more. So either way—free parameters or not—(M) follows.

Finally, the security of (M), realized in the two cases, differs markedly. We shall see from results reviewed in Section 4.4 below that we can be very secure in accepting (M) when applied to the light quantum. However we cannot be as secure accepting (M) in the case of Mercury's perihelion. The reason is that the perihelion motion of Mercury only probes the very

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[10] This experience stands in direct contradiction with the mythology in the underdetermination thesis literature that scientists are always awash in multiple theories, all fully adequate to the evidence. In practice theorists feel fortunate if they can find even one theory properly responsive to the evidence. For a critique, see Norton (2008).

weak gravitational fields surrounding the sun. Theories of gravity must cover strong gravitational fields as well, including those that arise in the vicinity of gravitationally collapsed bodies. These strong fields, according to general relativity, may harbor singularities in spacetime structure and also provide bridges to other sectors of our universe. We should be cautious using data from weak gravitational fields to choose among gravitation theories that deal with such extraordinary possibilities in the domain of very strong gravitational fields.

3.3 How They Warrant Einstein's Claim

It would seem easy to infer Einstein's evidence claim from (M); and we can, with a little care. Schematically, the inference proceeds from (M) as:

(M1) If *not-hypothesis*, then it is very unlikely that *evidence*.

from which we infer

(M2) If *not-hypothesis*, then it is very likely that *not-evidence*.

Then, by mimicking contraposition in deductive logic,

(M3) If *evidence*, then it is very likely that *hypothesis*.

The inference from (M1) to (M2) is, I believe, unproblematic. It merely replaces an "unlikely that" with a "likely that not". The inference from (M2) to (M3) may also seem unproblematic. It seems to be a minor variant of a valid deductive inference from

(D2) If *not-hypothesis*, then *not-evidence*.

to its contrapositive

(D3) If *evidence*, then *hypothesis*.

However the addition of the "very likely" modifier makes a difference that requires other background assumptions to hold if the inference from (M2) to (M3) is to proceed. For, unlike its deductive counterpart (D2), (M2) allows for the possibility that the hypothesis is false, but the evidence still obtains, even if only as a very unlikely possibility. This unlikely possibility may be sufficient to defeat the inference from (M2) to (M3). For, once we have learned the evidence obtains, we may end up judging "*evidence* and *not-hypothesis*" to be more likely than "*evidence* and *hypothesis*." That can happen in two ways, one in which we favor "*not-hypothesis*" and one in which we disfavor "*hypothesis*":

(i) If the hypothesis itself is antecedently vastly implausible, we may continue to judge the hypothesis unlikely, even though the evidence obtains.

(ii) It may be that the evidence is even less favorable to the hypothesis obtaining than it is to the failure of the hypothesis. In that case, the evidence would give more support for the falsity of the hypothesis than the hypothesis.

Loophole (i) is realized in the following example. The famous math prodigy Ramanujan noted that π^4 very nearly equals 2143/22 to eight significant figures and is remarkable in that it uses only the small digits 1, 2, 3, and 4. Take this as the evidence. We may hypothesize it is due to the cosmic interference of a benevolent mathematics genie who placed it there to amuse us. Let us grant that if the genie hypothesis is false, it is very likely that just this formula would not obtain; that is, grant (M2) applies to this case. Nonetheless, most of us find the idea of such a genie antecedently so vastly implausible that, even with the evidence of the value of π^4, we continue to judge the genie unlikely; that is, we deny (M3). The same example can also realize loophole (ii). We may well judge that were there such a benevolent mathematics genie who could interfere with the very content of mathematics, that genie would not leave as obscure an entertainment as this rather facile formula for π^4. In that case, it no longer matters whether we are antecedently well disposed to the hypothesis or not. The evidence disfavors the hypothesis and (M3) fails again.

Recognizing that the inference from (M1) to (M3) must be treated cautiously, we can say that it obtains for the two cases at hand, Einstein's light quantum and the perihelion motion of Mercury. In both cases, loophole (ii) is closed in that the hypotheses (the light quantum hypothesis/ general relativity) entail the evidence, with suitable auxiliaries. Loophole (i) is closed, in the case of general relativity, in that the physics community finds nothing intrinsically objectionable in general relativity. On the contrary, the theory is universally regarded to be so esthetically pleasing that we incline to accept it.[11] The antecedent plausibility of the light quantum hypothesis is harder to judge since it contradicts the enormously successful wave theory of light of the nineteenth century. Presumably that fact did not engender sufficient antecedent doubt that Einstein was dissuaded from writing his paper and the journal from publishing it. If the notion had the antecedent credibility of a mathematics genie, it is hardly likely to have survived.

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[11] Here I share the hesitations of a referee for this journal who pointed out that the esthetics may incline us to acceptance but may not rationally justify the inclination.

The above analysis teases out, at somewhat tedious length, the sort of inferences that people make rapidly. "If there weren't going to be a thunderstorm, then its unlikely that we'd have thunderclouds massing on the horizon. But we do. So there will likely be a thunderstorm." Bayesians will, no doubt, already be mentally starting to compare the inference from (M1) to (M3) with a natural application of Bayes' theorem.[12] While a Bayesian analysis will vindicate the inference from (M1) to (M3), I am not inclined to say that it is what is "really" going on in this inference. The sense of "likely" at issue in (M) is a rough and ready one, with a much impoverished value set for the relevant degrees of support. They are restricted to something like true, false, very likely, very unlikely and perhaps some intermediate value. The associated inferential practice is prone to paradox if used incautiously and in a way that the proper use of probabilities will not produce paradoxes.[13] The rough and ready notions can be applied to cases in which the full precision of the theory of probability is excessive. A case is the example of the mathematical coincidence noted above on the value of π^4. We may speak of its "probability," but if we intend that to invoke the full content of the probability calculus we are surely guilty of spurious precision. Aside from those who have diligently trained themselves, we have a much less formal notion in mind when we instinctively say we find the coincidence and the very idea of a benevolent mathematics genie "unlikely."

Let us say that the probability calculus does fail historically as a description of how we informally use the notions of likely and unlikely. Nonetheless, should we not decide, normatively, to conform our use of the notions to the probability calculus, for that would protect

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[12] The analysis can be recovered directly from Bayes' theorem as given in (6) in Section 4.3 below. (M3) corresponds to $P(H|E)$ being nearly one. It can fail to have that value in the two ways indicated above. First, (i), it can fail if $P(\sim H)$ is very much greater than $P(H)$. Second, (ii), it can fail if $P(E|H)$ is greater than $P(E|\sim H)$. Neither loophole obtains in the cases at hand.

[13] The difficulties are well known. If each of A, B, C, … are individually very likely, then, according to a common, tacit presumption, so is their conjunction. This presumption eventually must fail. While each, individual lottery ticket is very unlikely to win, at least one of all the tickets sold must win. Or while I may believe each individual assertion in my magnum opus very likely to be true, I am also convinced that very likely they cannot all be true. See Sorensen (2006, §§3-4).

us from the inconsistencies and paradoxes alluded to above? That prescription is a double-edged sword. It also commits us to aspects of the Bayesian system we may not want, such as its long-standing difficulty of representing neutral as opposed to disfavoring evidence, or, in subjective terms, ignorance as opposed to disbelief. (For more, see Norton, 2007, 2008a, forthcoming a.)

4. Accounts of Induction

The proposition (M) has a nebulous character. It depends on vague judgments of what is likely and unlikely among a universe of possible hypotheses and theories, most of whose members are only dimly glimpsed. Ought we really to think that this is what powers Einstein's celebrated inductive inferences? Do we not recover a more solid account from one of the many formal accounts of induction to be found in the philosophy of science literature? We do not.

The principal claim of this paper is that all these account are able to vindicate Einstein's inductive claim only in so far as they presume (M). The general principles they add to (M) are comforting, in that they give us a sense of a principled analysis. But those principles are inert. For the proposition (M) by itself is sufficient to vindicate Einstein's claim. Whatever the general accounts add are superfluous for the exercise at hand, that of assessing the cogency of Einstein's inductive claim.

My goal in this section is to make good on this claim. To do this, I will indicate briefly how some of the principal accounts of inductive inference accommodate the two cases at hand, if they can, and indicate how every one of them depends in the end on (M). To make this review tractable, it will be structured by a survey I have given elsewhere (Norton, 2005) of different families of accounts of inductive inference. That survey divides accounts of inductive inference into three families: inductive generalization, hypothetical induction and probabilistic induction.

4.1 Inductive Generalization

Accounts of inductive inference in this family all depend on the principle that an instance confirms the generalization. The simplest is the venerable enumerative induction: If some A's are B, then all A's are B. The family grew with attempts to extend the reach of this limited inference form. In my catalog (Norton, 2005, pp11-14), it includes Hempel's instance confirmation, Glymour's boostrap and Mill's methods. None of the members of this family[14] seem especially well suited to the two cases at hand, excepting analogical inference, which is a variant form of enumerative induction.

If a has properties P and Q and b has property P, then the inductive argument form of analogy allows us to infer that b also has property Q. This argument form can be applied directly to the case of Einstein's light quantum (but not the perihelion of Mercury). We have

> An ideal gas sustains volume fluctuations governed by (2); and it consists of many, spatially localized, independent components.
>
> High frequency radiation sustains volume fluctuations governed by (4), a formula very similar to (2).

Therefore, by analogy

> High frequency radiation consists of many, spatially localized, independent components.

Or at least that is the conclusion that analogy allows us to infer. Few of us would make the inference with much confidence, without closely examining the details. We are very aware of how fragile analogical arguments can be. Ripples in a pond and sound are propagating waves and they are carried by a medium (water, air). Light is a propagating wave. Therefore by analogy light is carried by a medium (ether).

The strength of the analogical inference depends upon the relative weights of the positive and negative analogies, that is, the degrees of similarity and dissimilarity of the entities related by the analogy. The greater the negative analogy, the weaker the inference. In the case of the light quantum, the negative analogy is very great. An ideal gas is very different from heat radiation in many properties. Most importantly, radiation, unlike ideal gases, exhibit wavelike properties, such as interference, and waves are inevitably associated with extended systems in space, in direct contradiction with what the positive analogy seeks to establish.[15] We need to be

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[14] Glymour (1980, pp. 288-89) balks at fitting the three classic tests of general relativity, one of which is Mercury's perihelion motion, into his bootstrap framework. In Norton (2005) I included demonstrative induction within this first family as an extreme form. Its application to these two cases is sufficiently important to be reserved for a separate section below.

[15] Another striking difference is that if we isothermally expand a cylinder filled with heat radiation, then more radiation is created to fill the new space. If that radiation consists of quanta,

quite assured of the force of the positive analogy in the probability formulae to overrule such a strong negative analogy. That is, we need to be assured that the positive analogy is not fortuitous or spurious. And that amounts to saying that we need to hold that the agreement in the probability formulae (the evidence) is very unlikely to come about, were the agreement in constitutions not the case. But that amounts to saying that the analogical argument is very strong only in so far as we already accept (M).

The general principle of analogical reasoning recovers the warrant for Einstein's results, only in so far as we already accept (M). But if we already accept (M), we have no need of the principle to recover the warrant.

4.2 Hypothetical Induction

This second family of accounts (Norton, 2005, pp. 14-17) of inductive inference stem from the simple idea that it is a mark of truth when an hypothesis or theory deductively entails the evidence. This is hypothetico-deductivism; or, to use the name attached to the earliest instance of the notion, it is "saving the phenomena." The difficulty with this mark is that it is too easy to acquire. If some hypothesis saves the phenomena, then so will the conjunction of that hypothesis with just about anything else. Virtually all embellishments seek to tame this indiscriminateness by requiring that, in addition, the phenomena must be saved in right way. The specification of this right way generates the family of accounts.

Simplicity

The simplest embellishment is to require that we choose the simplest hypothesis able to save the phenomena. That simplicity certainly obtains in the case of the light quantum. No one can doubt the simplicity of the idea that radiation just consists of independent points and the elegance with which one proceeds from it to the probability formula (4) and then to the entropy formula (1). The case of general relativity is not so straightforward. Einstein's theory replaces the one, easy to solve, linear field equation of Newtonian theory with ten, non-linear, coupled differential field equations, whose solution, even in simple cases, is a complicated mathematical

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then the expansion creates new quanta. The isothermal expansion of an ideal gas certainly does not create new molecules.

feat. However, at a more elevated, conceptual level, general relativity is reputed to be beguiling simple; all gravitational phenomena are subsumed under the simple idea that the curvature of spacetime goes hand in hand with matter density.

This example shows the difficulty of employing the principle that we should accept the simplest, adequate hypothesis. There can be competing notions of simplicity applicable to the one case; and we may further judge some senses of simplicity to be of the wrong sort for epistemic purposes. We might account for the elliptical orbits of planets with Newtonian theory or with the hypothesis that God wants those motions just as they are. The God hypothesis is four times simpler in that it replaces four laws of the Newtonian account (three laws of motion plus the law of gravitation) with just one hypothesis. Yet the mere counting of sentences is too unstable a measure of simplicity to sustain serious differential judgments of evidential support.

What is the relevant notion of simplicity? In the case that works best, the relevant notion of simplicity seems to reside directly in the idea that, having seen what the light quantum hypothesis says and how it enables an elegant recovery of the evidence, we deem it unlikely that any other hypothesis could plausibly recover the evidence. Or, more cautiously, no other hypothesis could not do it without massively complicating contrivances that would in turn cast the doctored alternative out of the realm of possibility. In the absence of any independent rule that identifies the epistemically active sense of simplicity, we attach the label of simplicity to that feature of the hypothesis; that is, in this case, we identify simplicity with the obtaining of (M).

The general principle of simplicity recovers the warrant for Einstein's results, only in so far as we already accept that (M) obtains. But if we already accept (M), we have no need of the principle to recover the warrant.

Eliminativist Accounts

Another type of approach seeks to embellish hypothetico-deductivism by conditions that have the effect of eliminating the alternatives to the hypothesis. A well-articulated example is Mayo's (1996, Ch. 6) notion of a severe test. Imagine that predicting correctly the anomalous motion of Mercury is offered as a challenging test to a gravitation theory. Then to pass the test, the theory must predict the motion correctly. Now, passing the test would offer no special support if every theory could make that same prediction. To rule out this breakdown, Mayo adds the requirement that passing a test supplies an evidential warrant, only if the test is severe. In one version (p. 180), the severity requirement is:

> There is a very low probability that test procedure T would yield such a passing result, if [the hypothesis] H is false.

In the case of general relativity and Mercury's perihelion, the hypothesis H is general relativity and passing the test procedure is correctly predicting the anomalous motion of Mercury. So the severity requirement amounts to requiring this: if general relativity were false, then very probably there would be some anomalous motion for Mercury other than the 43 seconds of arc per century that is observed and that general relativity predicts. That amounts to requiring that if the hypothesis, general relativity, were false, then the evidence as we now have it would very likely not have obtained. That is just (M). (A similar analysis would give us (M) from the severity requirement in the case of the light quantum.)

The general principle of severity of testing recovers the warrant for Einstein's results, only in so far as we already accept (M). But if we already accept (M), we have no need of the principle to recover the warrant.

Abduction

Another popular embellishment of hypothetic-deductivism is abduction or inference to the best explanation. The added requirement is that the hypothesis or theory must not just save the phenomena; it must explain it and it must explain it the best. The account works well at an intuitive level. The light quantum hypothesis certainly gives a very satisfying explanation of the probability formula (4) and thereby also the entropy formula (1). Einstein himself repeatedly wrote of his "Explanation [*Erklärung*] of the Perihelion Motion of Mercury from the General theory of Relativity"—this being the title of Einstein (1915).

The difficulty with this account emerges, however, when we try to make precise just what is meant by explanation, as we must if it is to figure centrally in account of inductive inference. Einstein gave no clarification of what he meant by the term. Of the various notions of explanation in the literature, it seems to me that the one that fits the two cases at hand is the covering law model. According to it, evidence is explained if it deduced (with appropriate auxiliaries) from a covering law. The evidence of Mercury's motion is deduced from the covering law of general relativity. The entropy formula (1) for radiation is deduced from the covering law of the hypothesis of light quanta.[16]

What is it for an hypothesis to explain best? It is not too hard to identify virtues. Many of them are not of the sort that is relevant to the "best" of inference to the best explanation. For example, Einstein used an elegant iterative computational procedure to wrestle the anomalous motion of Mercury from his theory. Whether the particular computational procedure Einstein used is elegant or not is irrelevant to the evidential warrant. The virtues sought are those that would favor the truth of the hypothesis or theory. In that regard, the obvious virtues are those already outlined in Section 3.2. General relativity entails the anomalous motion of Mercury without special hypotheses. This is epistemically relevant precisely because it indicates that if another theory were the right one, we would likely not have the same evidence obtaining. In the case of the light quantum, the virtue is that the deduction of the probability formula (4) from the light quantum hypothesis is successful at all, since we doubt any other could do it. That automatically makes it the best. The epistemic power of these virtues is already embraced by the simple formula (M), so that we see once again that the success of the abductive account presumes (M) in these two cases.

The principle of inferring to that which best explains the evidence, recovers the warrant for Einstein's results, only in so far as we already accept (M). But if we already accept (M), we have no need of the principle to recover the warrant.

Reliabilism

The final embellishment to be considered here is reliabilism. According to this approach, we cannot assess in isolation the evidential import of an hypothesis successfully accommodating the evidence. That assessment can only be done in the context of the history of the accommodation, showing that it conforms to a reliable discovery process. Best known of these accounts is Popper's (1959) account of science progressing through a cycle of bold conjecture,

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[16] Or perhaps we might construe explanation in the latter case of light quanta only as the revealing of an underlying constitution. It ends up to be pretty much the same, since, from the hypothesis of the constitution of radiation as light quanta, we still deduce the entropy formula (1).

testing against evidence and refutation.[17] A bold conjecture that passes the test is "corroborated," a notion that, in spite of Popper's tireless denials, seems to differ little from the notion of "confirmed." Lakatos' (1970) "methodology of research programmes" is a more elaborate version of Popper's falsificationism in which the development of science is portrayed as a struggle between competing research programs. Einstein's 1915 recovery of the anomalous motion of Mercury would count as a major success for the research programme hosting general relativity. Although this anomalous motion was not a novel fact, accounting for it expanded the explanatory power of the program. That is progressive (p. 117), a praiseworthy feature for Lakatos. The converse of this virtuous accommodation of observation is the non-virtuous *ad hoc* hypothesis.[18] *Ad hoc* hypotheses may accommodate the evidence; yet it is said that they are not supported by it since they were devised precisely with this goal in mind.

The presumption seems to be that we can only meaningfully say that something has evidential import in the context of the sorts of historical tales told by Popper and Lakatos: who had which idea when and what happened next. While that presumption seems dubious to me, I will not dispute it here. Rather, I want to address the question of why passing tests, or being progressive, or not being generated *ad hoc* should have epistemic force. Passing a test or explaining some anomaly ought only to advance a theory in relation to its competitors if we believe that these competitors cannot perform as well. That is we should favor a theory or hypothesis for its ability to entail the evidence just to the degree that we hold its competitors cannot. That is, the reliabilist approach depends upon the assumption that, were other theories or

[17] Mayo's notion of severe testing might also belong here, although her specification of just what counts as a severe test is ahistorical, so it can be applied without recounting the precise history of the test.

[18] For example, the failure of nineteenth century ether drift experiments to detect an ether current is accommodated by the *ad hoc* hypothesis that we just happen to be at rest in the ether. I urge readers to resist the temptation of dismissing Einstein's formulation of the light quantum hypothesis as a defective, *ad hoc* hypothesis, even though it was explicitly designed to accommodate the known entropy properties of heat radiation. The idea was too ingenious for such rude dismissal.

hypotheses to be the right ones, then we would not expect to have the evidence that we do have. That is the proposition (M).

The reliabilist approach recovers the warrant for Einstein's results, only in so far as we already accept (M). But if we already accept (M), we have no need of it to recover the warrant.

4.3 Probabilistic Induction

This family of accounts of inductive inference takes its inspiration from the theory of probability developed in the seventeenth century as a means of analyzing games of chance. These physical probabilities behave like degrees of inductive support or degrees of belief, so the proposal is that these degrees everywhere ought to conform to the same calculus. Other members of the family employ other calculi in the attempt to remedy defects of the probability calculus as a logic of induction. Here I will consider only the accounts of induction that retain the probability calculus as the governing calculus and represent the support that some background B gives to an hypothesis H by the probability P(H|B). The evidential import of an item of evidence E is gauged by conditionalizing on E. These accounts are typically called "Bayesian," since Bayes' theorem is used to compute that conditional probability. In the three subsections that follow, the analysis will treat the cases of objective and subjective Bayesianism separately and, first, a complication attached to the Bayesian analysis of already known evidence.

4.3.1 Glymour's "Problem of Old Evidence"

When E supports H, conditionalizing on H increases the probability of H; that is, P(H|E&B) > P(H|B). If that increase is our measure of support, then according to Glymour's (1980, pp. 85-93) "problem of old evidence," Einstein's evidential warrant fails completely in the two cases considered here. For in both cases, the evidence (the entropic properties of radiation/ Mercury's anomalous perihelion motion) was already known at the time the theories were proposed. Writing B for the background knowledge then known, E for each of these items of evidence and H for *any hypothesis whatever*, the fact that E was already known entails that

$$E\&B = B$$

from which we immediately infer that

$$P(H|E\&B) = P(H|B) \tag{5}$$

It now immediately follows that H accrues no support from E. Since H is any hypothesis, E has become evidentially inert, contrary to every intuition.

Glymour presented the problem as one specifically challenging probabilistic accounts and deduced (5) by the rather indirect route of an application of Bayes' theorem. We can see immediately that the problem is far more general. It will arise in any account of evidence that assigns degrees of support as the binary relation [hypothesis | evidence], where these degrees need no longer be probabilities or even numeric (as in Norton, 2007). For we will still have [H|E&B] = [H|B].

It seems far too cheap to let this problem derail such a large class of accounts of evidential import. So, while there have been some ingenious constructions proposed to evade this problem in the Bayesian context (see Earman, 1996, Ch.5), I believe the best response is the obvious one. The import of evidence E should be assessed against a background B' from which E has been deleted. Glymour is right that we take great liberties in presuming that such a background can be identified unequivocally, although Howson and Urbach (2006, pp. 297-301) seek to establish that the resulting uncertainty in the analysis is small. Nonetheless that liberty seems comparable or even more modest than others routinely taken in Bayesian analysis.[19] Bayesians will assign probabilities to outcomes conditioned on the supposition that some hypothesis does not obtain, even though they cannot pretend any precise grasp of the full range of possibilities opened by the falsity of the hypothesis.

4.3.2 Objective Bayesianism

In objective Bayesianism, conditional probabilities P(H|E) represent the inductive support evidence E, conjoined with the tacit background, provide for H. The adjective "objective" indicates that the degree of support is taken to be a relation between H, E and the background and that it has one correct value; if different people ascribe different values to P(H|E), at most one is correct.

We presume that the prior probability P(H) of the hypothesis (light quantum hypothesis/general relativity) is tacitly conditionalized on a background from which we have

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[19] There is also a precedent in legal proceedings, in which the evidential record must be purged of improperly secured evidence.

deleted the evidence E (entropy formula (1)/anomalous motion of Mercury). In that case, Bayes' theorem can be written as

$$P(H \mid E) = \left(1 + \frac{P(E \mid \sim H)}{P(E \mid H)} \frac{P(\sim H)}{P(H)}\right)^{-1} = \left(1 + P(E \mid \sim H) \frac{P(\sim H)}{P(H)}\right)^{-1} \tag{6}$$

where we have set P(E|H)=1 to reflect the fact that in this case the hypothesis H (with suitable auxiliaries) entails the evidence. We read immediately from (6) that P(H|E) is close to one, that is H is very likely when E is presumed, just in case P(E|~H) is small. That this likelihood is small asserts that E is very likely false if we presume the falsity of H. But this last presumption is just (M).

Thus the success of the entire analysis depends on this presumption (M).[20] We have seen above, however, that the judgments that establish (M) are quite imprecise. They are a mix of an accomplished theorist's instincts and a belief that what we cannot imagine, cannot be. The formula of (6) bears an impressive aura of precision. But since the outcome of its calculation depends sensitively on the imprecise quantity P(E|~H), it risks being an exercise in spurious precision. Of course we need not take that risk to recover Einstein's evidential warrant. As we saw in Section 3.3, that warrant can already be secured directly from (M) and without any special pretensions of precision.

4.3.3 Subjective Bayesianism

In the approach of subjective Bayesianism, the conditional probabilities are freely chosen degrees of belief, subject only to the constraints of the probability calculus, and their values may and vary from agent to agent. Indeed, they are expected to vary. In a subjective Bayesian analysis, degrees of belief will be updated according to Bayes' theorem, as in (6), upon acquiring evidence E. However that updating need not capture the evidential bearing of E at all. The updating is controlled by the likelihood P(E|~H). If this likelihood is merely chosen arbitrarily, as subjectivism allows, then the shift in belief arising from the application of Bayes' theorem will merely reflect the arbitrary prejudices of the agent who chose the value of P(E|~H).

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[20] The other way that P(H|E) can turn out close to one is if P(~H) is very small, that is P(H) is close to one. But that just asserts that we already think H very likely, so the displaying of a correspondingly large P(H|E) is no longer revealing the evidential import of E.

This means that the subjective Bayesian account is unable to vindicate Einstein's two inductive inferences *specifically*. Rather, the hope of the subjective approach is more global. If Einstein persists in updating his degrees of belief by conditionalization, eventually in the limit of many such conditionalizations, the hope is that Einstein's degrees of belief will converge with those of others engaged in the same exercise and they will mass their probabilities on true hypotheses. One might also hope that, in this process, sufficient convergence has been achieved by the time Einstein conditionalizes on E, so that the likelihood P(E|~H) he uses reflects the objective evidential relation expressed by (M). Then the conditionalization on E would reflect the evidential bearing of E. However there can be no assurance that Einstein's initial arbitrary selection of prior probabilities will admit this.

In practice, of course, those who describe themselves as subjective Bayesians are often not so purely subjective and practice a hybrid of the subjective and objective approach. For them, prior probabilities may be chosen arbitrarily, but likelihoods, such as P(E|~H), are chosen in the same manner as do objectivists; that is, in a way that is hoped to capture the objective support ~H lends to E, as is expressed in (M).

In sum, a Bayesian analysis, whether objective or subjective, recovers the warrant for Einstein's results, only in so far as it presumes or accommodates (M). But if we already accept (M), we have no need of the Bayesian analysis to recover the warrant.

4.4 Demonstrative Induction

In an ampliative inductive inference, we take some inductive risk in proceeding from the premise of the evidence to the conclusion of the hypothesis in question. It may turn out that the conclusion is false, even when the evidence is true. There are many interesting cases in which this inductive risk can be eradicated. These are cases in which we discover that we have already sufficient presumptions in our background knowledge that, with their aid, we may *deduce* the hypothesis from the evidence. In effect, we already took the same inductive risk when we incorporated the presumptions into our background knowledge. The modified inference is deductive, that is, demonstrative. Hence it is known as a "demonstrative induction" (Norton, 2005, pp. 13-14).

The inference form is sometimes also known as "Newtonian deduction from the phenomena" since it was used by Newton in his celebrated *Principia*. A simple example in

Newtonian physics illustrates the argument form. Famously, Newton's inverse square law of gravity entails Kepler's third law of motion for the planets. That law relates their period T to their radii R according to $R^3 \propto T^2$. So, by hypothetico-deductive confirmation, the truth of Kepler's third law lends support to the inverse square law of gravity.

Demonstrative induction eliminates the inductive risk taken in inferring from Kepler's third law to the inverse square law of gravity by making the inference deductive. To see how it does this, take the simple case in which we presume that planetary orbits are circular. For a planet orbiting at speed V in a circular orbit of radius R with period T, it follows from Newton's mechanics that its acceleration $A=V^2/R$ and from Euclidean geometry that $V=(2\pi R)/T$. Combining, we have

$$A = V^2/R = (2\pi)^2 (R/T)^2 (1/R) = (2\pi)^2 (R^3/T^2) (1/R)^2$$

Kepler's third law tells us that (R^3/T^2) is a constant, so we deduce from the above relation that $A \propto (1/R)^2$, which is Newton's inverse square law, at least applied to this special case.

An awkward point in Einstein's light quantum argument comes when he infers from the probability formula (4) to the hypothesis of light quanta. The inference is inductive and we have explored in some detail what its character may be. In all those accounts, there has been some sense of fragility. If it is an argument from analogy with the formula (2) deduced for ideal gases, just how much inductive risk do we take in accepting the analogy and proceeding to Einstein's conclusion that light energy is localized in points just like the molecules of an ideal gas?

Dorling (1971), in a remarkable demonstrative induction, has shown that we actually take no real inductive risk at all. The argument from (4) to the light quantum hypothesis can be made deductive. In Dorling's words (p. 3)

> …I shall now show how [(4)] alone, in conjunction with some of the usual statements of the probability calculus, actually entails:
>
> (A) There is a probability equal to zero of the energy of the cavity being anything other than an integral multiple of $h\nu$.
>
> (B) If the total energy in the cavity is equal to $nh\nu$, then there is a probability equal to unity of there being exactly n distinct points in the cavity with energy $h\nu$ located at each point.

I believe in addition that Dorling's arguments establish the probabilistic independence of the distribution of spatial points of (B). So Dorling's arguments return the light quantum hypothesis, in so far as we are willing to proceed from judgments of probability 0 and 1 to truth and falsity.

Dorling's argument is not especially simple. It starts with a few special cases and then arrives at the general result by recursion. We can, however, get a general sense of how his argument proceeds by looking at his two simplest cases.

Take the case in which $E=h\nu/2$. In that case, the probability that all the energy has fluctuated to the left half of the volume V_0 is $W=(V/V_0)^{E/h\nu}=(1/2)^{1/2}$. So the probability that the energy has fluctuated to either left or right half volume is $(1/2)^{1/2}+(1/2)^{1/2}=2^{1/2}>1$, which contradicts an axiom of the probability calculus. Hence $E=h\nu/2$ is impossible.

Take the case of $E=h\nu$. The probability of finding the energy fluctuating into some particular subvolume of size V_0/n th is just $W=(V/V_0)^{E/h\nu}=(1/n)$. If we imagine the volume V_0 divided into n such disjoint subvolumes, there is a probability $n(1/n) = 1$ that the energy has fluctuated into just one of those subvolumes. However, since n can be as large as we like and the subvolumes as small as we like, this can only be true if all the energy $h\nu$ is localized in just one point.

This is an elegant and persuasive account of how we can proceed from the probability formula (4) to Einstein's light quantum hypothesis. However, what it does not provide is a plausible reconstruction of what Einstein intended when his text passed without comment from the probability formula (4) directly to the light quantum hypothesis. If Einstein explicitly had in mind an argument like Dorling's, some further elucidation would be called for. Perhaps the best we can say is that Einstein sensed intuitively what Dorling's argument establishes without doubt: that no other hypothesis about the energy distribution of radiation could give Einstein's formula. But sensing intuitively just that much is, in effect, just to ascribe to (M).

Demonstrative induction has a special connection with the material theory of induction that will be discussed in the Conclusion below.

5. Conclusion: Virtues of the Material Approach

The material theory of induction, in my view, affords a philosopher of science the best way of approaching the evidence claims of scientists. The first advantage is that it does not require us to portray scientists as secret methodologists, covertly or unconsciously conforming their inductive inferences to our favorite principle of inductive inference. What has made it tempting to imagine this sort of covert or unconscious behavior is that it does seem to work in relation to scientists' deductive inferences. Scientists do seem to conform their deductive reasoning to the familiar deductive argument forms. However that is no assurance that a similar reconstruction will work for inductive inferences. Indeed our enduring failure to settle on one correct account of induction continues to make the reconstruction efforts dubious. For we still do not know whether an Einstein inferring from the anomalous perihelion motion of Mercury should be portrayed as secretly computing Bayes' theorem or secretly sifting an hypothesis space for the best explanation.

The material theory of induction relieves us of the need to fit a scientist's inductive inferences into some elusive set of universally valid templates that prescribe good inductive inferences. The material theory tells us that there are no such things. Rather it enjoins us to seek the warrant for a scientist's evidence claims in other material facts.

In doing so, we may still need to ascribe some tacit presumptions to the scientist. In the two cases here, these were the factual claim (M) applied to each case. However, in this regard, what I showed in Section 4 is that the material theory is a strict improvement on every other applicable account of induction surveyed. For each such account needed to make the same presumption (M) to recover the evidential warrant and, in addition, to propose some general inductive principle. A philosopher of science following the material theory of induction can have the same results without the need to resort to any of these inductive principles.

Finally, the material theory of induction gives us more hope than any other account in assessing the strengths of various inductive inferences. Let us ask: how strong was Einstein's inference from the entropic properties of radiation to the light quantum hypothesis; and how strong is the support accrued to general relativity from its explanation of the anomalous motion of Mercury?

If we follow any of the accounts of induction surveyed in Section 4, the search for an answer immediately throws us into terminally nebulous assessments. If Einstein's argument is one from analogy, how good was the analogy? How do we assess the goodness of an analogy? Or, if Einstein's argument depended upon the simplicity of his hypotheses, just what it is to be simple; and how do we translate degrees of simplicity into measures of strength? Or if Einstein is inferring to the best explanation, how are we to assess the difference between explaining and merely accommodating; and how do we translate that into measures of strength? Or, if our analysis is Bayesian, how will the elegance of our theorems ever overcome the fact that the entire analysis depends upon a conditional probability P(E|~H) whose value is known more through intuitions and hunches.

The material theory of induction focuses our investigation more productively. It enjoins us to consider the particular material facts that carry us from evidence to theory. In doing so, we naturally treat each inductive inference as a unique individual, each with its own special properties, as opposed to homogeneous instances of a single argument form. The natural question we are led to ask is this: if one set of facts is carrying us there weakly, are there others that do so more strongly? This is practical, heuristic advice, as useful to the scientist as the philosopher. For it tells us that we better understand the strength of an inductive inference by knowing more factually about the case at hand. The problem is not to be solved by a flight to ethereal heights where we ponder just what it means to be simple or to explain better. Rather we should look to what we know or could know about radiation to see how that might affect our induction. The extreme form of this analysis is Dorling's successful demonstrative induction to Einstein's light quantum hypothesis. [21] For unlike any other analysis, Dorling's assures us that we can dispense with all the hesitations about Einstein's induction. It can be converted pretty much into a fully deductive argument.

The difference between this case of the light quantum and the case of Mercury's perihelion is striking. We saw above in Section 3.2 that we need to exercise some caution in accepting (M) in the case of Mercury's perihelion, for the evidence of the perihelion motion

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[21] Another example is how Poincaré and Ehrenfest in 1911 and 1912 resolved the problem of assessing how much support the hypothesis of quantum discontinuity accrued from its success at entailing Planck's distribution law for black body radiation. They showed that, with suitable auxiliaries, the deduction could be inverted. They deduced quantum discontinuity from the evidence of Planck's distribution law. See Norton (1993).

plumbs only the weak field.[22] So the material theory of induction leads us to see that the two inductive inferences discussed here actually have very different strengths.

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[22] We could try to mount a demonstrative induction for this case akin to Dorling's for the case of the light quantum. One possibility is that we could deduce from the perihelion motion of Mercury relevant parameters of the PPN formalism. But these at best fix the weak field behavior of one class of gravitation theory. We are far from a demonstrative induction from Mercury's perihelion motion to general relativity.

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