August 7, 8, 12, 2014

A Material Defense of Inductive Inference

John D. Norton
Department of History and Philosophy of Science
Center for Philosophy of Science
University of Pittsburgh
Pittsburgh PA USA 15260
www.pitt.edu/~jdnorton

1. Introduction

Skepticism over inductive inference is so deeply woven into our philosophical tradition that we name its many forms after our philosophical heroes or villains: Hume's problem of induction, Hempel's problem of the raven, Goodman's problem of grue and Quine's problem of underdetermination. This proliferation of skepticism surrounds inductive inference with a miasma of philosophical decay. Yet, at the same time, inductive inference in science brings us the most extraordinary achievements. We live with a disturbing tension. Doubting inductive inference in the generality is philosophically respectable. There can be no inductive justification of inductive inference, Hume assured us. Yet doubting inductive inferences in the specific seems pointlessly quarrelsome. Are we to doubt the inductive inferences that tell us that the planets orbit the sun, that matter is made of atoms, that life evolved, that microbes carry contagion, and so on? To echo C. D. Broad's lament: inductive reasoning is the glory of science but the scandal of philosophy.[1]

[1] Broad's (1926, p. 67) exact wording has proven less quotable: "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 standard response by philosophers is to quarantine our skeptical doubts in an isolation ward where the fever may rage. We do not allow the contagion to pass beyond these confines, lest we risk derision when our non-philosophical friends discover that we think nothing justifies the belief that the sun will rise tomorrow.

Sometimes skepticism is merely troublesome sophistry and quarantine is the best response. However sometimes skepticism is a clue that there is a real problem. If the skepticism is persistent, spanning not just centuries but millennia, it might well be a strong signal of a deep unsolved, foundational problem. This I believe to be the case with inductive inference. The skepticism persists because have not properly identified the foundational problem and so have had no chance to solve it.

The real problem is that we have simply misunderstood how inductive inference works. We think it is sufficiently like deductive inference that we can model our account of inductive inference on it. We try to distinguish the valid inductive inferences from the invalid in the same way as we do with deductive logic: by checking which of them conforms with one of a set of universal inference schemas. That is how formal logic works. Valid inferences are distinguished from invalid by their form. After two millennia, we need to accept that this model has failed for inductive inference and is responsible for the lingering miasma that surrounds inductive inference.

We dispel it with a better understanding of the nature of inductive inference. The correct model, I will argue below, is that inductive inferences are not warranted by conformity with a universal template. None succeed universally, so that there is no universally applicable logic of induction. Rather, inductive inferences are not warranted formally but materially. They are justified by facts, that is, by the factual matter that is itself the content of argumentation.

My principal goal in this paper is to illustrate how this material theory of induction emerges as the natural response to our failure to identify a universal, formal logic of inductive inference. In Section 2, I will review several of these failures, including the failure of universality of Bayesian inductive logic. In Section 3, I will argue that the mode of its failure will lead us directly to the material theory of induction, which replaces the ever-elusive universal logic of inductive inference with many localized logics of induction, each adapted to specific domains by the facts prevailing there. Section 4 provides a general argument for the view. It is illustrated in Section 5 and 6 with the case of Galileo’s law of fall. Conclusions are in Section 7.

2. Skepticism about Universal Logics of Inductive Inference

There is no formal logic of inductive inference that succeeds universally. I stress the "universally." Most logics work quite well somewhere. I assert that none work everywhere. This is a strong claim and one that needs considerable work to sustain. For there are very many formal logics of inductive inference. My approach has been to coalesce these many logics into one of three large families of accounts, each of which is powered by one idea. This coalescence makes the refutation tractable, for the failure of universality of representatives of each family is easier to demonstrate. This exercise has been explored a little more fully in Norton (2003, 2005). Here I will reproduce a few examples of it.

2.1 Inductive Generalization

The first family is inductive generalization.[2] It is powered by the notion that an instance confirms a generality. Expressed in syllogistic logic, it becomes the schema of enumerative induction. Expressed in first order predicate logic, it becomes Hempel's instance account of confirmation. This is the argument form most commonly invoked in traditional skeptical analyses. The past history of sunrises inductively supports future sunrises. The past history of bread nourishing inductively supports bread always nourishing. They are all instances of schema of enumerative induction:

Some As are B.
____
All A's are B.

The schema has survived only through our willful indulgence in ignoring of what everyone surely knows: Most commonly, when some A's are B, it is *not* the case that all A's are B. The schema simply does not work. Its uncritical use almost always produces bad results. We avoid disaster only by carefully contrived selection of our A's and B's.

The problem becomes quite apparent if we look at real cases in science. After extraordinary labors spanning years, Marie Curie finally managed to isolate a mere tenth of a gram of radium chloride. She inspected its crystalline form and, on the strength of this one sample's properties, immediately declared a general conclusion (Curie, 1904, p. 26):

---

[2] For a pedagogic introduction, see Norton (2009).

> The crystals, which form in very acid solution, are elongated needles, those of barium chloride having exactly the same appearance as those of radium chloride.

These inferences and many she made like it involve highly selective choices of A and B. She could have generalized from her tenth of a gram sample to conclude that all radium chloride is in Paris; or prepared by chemists by fractional crystallization; or comes in tenth of gram weights; or is roughly at room temperature; and so on endlessly. She chose only very specific A's and B's.

How can a formal theory accommodate this narrowness of selection? The only resource is to add extra formal clauses to the schema that specify just which A's and B's are allowed. It takes only a little reflection to see how hopeless is the task. We must find clauses that will authorize just Curie's careful selection as well as all those that might come up in every other application of the schema. Indeed a non-chemist will likely fail to see just how carefully contrived is Curie's selection of the property to be generalized. We shall see below that the choice of B as "elongated needles [exactly like] those of barium chloride" was no mere idle convenience. It was carefully chosen.

2.2 Hypothetical Induction

This family of accounts of inductive inference is powered by the notion that an hypothesis accrues inductive support when it deductively entails affirmed evidence. The most familiar form is the astronomical saving of the appearances. Hypotheses over the motions of heavenly bodies accrue support when they conform to and correctly predict the celestial motions we observe (the "appearances"). The great difficulty with this basic notion is its profligacy. If we have any hypothesis that saves the appearances, so will any logical strengthening of it. Each of Copernican astronomy simpliciter and Copernican astronomy conjoined with some of his neoPlatonic assertions about the sun both save the celestial phenomena equally well. Are they then equally well supported?

As a result, there have been many attempts to rein in simple hypothetical induction, often called "hypothetico-deductive confirmation." It is done by adding conditions that must be met before an hypothesis can accrue support from its affirmed deductive consequences. I will discuss just one. According to "inference to the best explanation" we require that the hypothesis not just entail deductively the affirmed evidence; it must also explain it. In his celebrated work, J. J. Thomson (1897) found that cathode rays deflected in electric and magnetic fields just as if they were constituted of massive, charged particles with a specific mass to charge ratio. The best explanation of this fact was that cathode rays just are beams of these particles. He thereby set aside the alternative view: "the almost unanimous opinion of German physicists [that] they are due to some process in the aether," or, more briefly, some sort of wave phenomenon.

The success of the inference was short lived. By the 1920s, with the rise of quantum mechanics, the wave character of the electron was soon affirmed. Davisson and Germer (1927) found that cathode rays, scattered off a crystal of nickel, formed diffraction patterns just as if the rays were waves with wavelengths given by the quantum de Broglie formula.[3] The best explanation was that these rays are de Broglie waves of this wavelength.[4]

We have two inferences to the best explanation that give strikingly different conclusions. That fact alone does not impugn inference to the best explanation as an inductive inference form. For inductive inference is fallible and the display of failed inductive inference may merely be illustrating the fallibility. Rather, the two inferences illustrate the major failing of inference to the best explanation. It is that explanation, understood formally, contributes rather little to the outcome of the inference. That outcome is mostly controlled by the factual background we assume when making the inference.

This manifests in the fact that both of these inferences provide strong support for their hypotheses. However the schema of inference to the best explanation provides no formal structure for these judgments of strength. If we know in the abstract that some hypothesis is the best explanation of the evidence, we have no way to assess how strongly the hypothesis is supported. It may be strong, weak or negligible. However, once we look at the specifics in the background facts, we can then make the judgment, at least qualitatively. Thomson's cathode rays

---

[3] Curiously, J. J. Thomson's son, G. P. (George Paget) conducted similar experiments at the same time and also affirmed the wave character of electrons. J. J. did not find this to be a refutation of classical physics, but an affirmation of the success of a classical account of the electron as a composite particle and field structure. For further discussion of J. J. and G. P.'s work and their interactions, see Navaro (2010).

[4] That just this hypothesis specifically is supported is clear from Davisson's (1937) and G. P. Thomson's (1937) cautious formulations in their Nobel Prize acceptance speeches, as well as the award speech (Pleijel 1937).

respond in perfect concert to electric and magnetic fields of varying intensity as expected of charged particles. Davisson and Germer's cathode rays deliver a diffraction pattern that is the distinctive fingerprint of waves of the requisite wavelength. Hence both inferences are strong.

This lesser formal role for explanation reflects the deeper problem with inference to the best explanation. It is not a properly developed logic at all. It lacks is a unique, stable, formal account of explanation. Accounts of explanation are notoriously scattered. If to explain is to subsume under a covering law from which the explanandum is deduced, inference to the best explanation reverts to simple hypothetico-deductive confirmation, unless we can provide general characterization of just what a law is. If we take explanation to be the displaying of probability raisers, then we need to find a probability space in which we can assign probabilities to the various propositions concerning the nature of cathode rays. Or if to explain is to display the causes, then we need to find a clear pathway through the tangled thicket that is the present literature on causation. Or if to explain is to unify, we need some account of the difference between mere conjunction of propositions and their unification. Perhaps we can make progress on all these questions. But that is mere hope for the future and is not now giving us a precise formal theory of inductive inference. It is, to use Lipton's (2004, pp. 2, 55, 57) wording, more a "slogan." It is an advertisement for what might one day be a properly developed logic.

2.3 Probabilistic Induction

Philosophical fashions change. Even though the idea has been with us for centuries, it is only in the last few decades that a probabilistic approach to inductive inference has risen to be the dominant approach, in the form of Bayesian confirmation theory. Its appeal is immense. There are many contexts in which it produces analyses of extraordinary power. It also has the virtue of a mechanical calculus. Once you have determined the probability space and have a modicum of probability assignments, solving an inductive problem, even of some complexity, may reduce to mere computation, often only a little more challenging that simple arithmetic.

This undeniable appeal has encouraged Bayesians to discount or overlook the shortcomings of the system. Two shortcomings are of foundational importance. First, the Bayesian view is that the entirety of inductive inference is subsumed by its probabilistic approach. This is mistaken. Bayesian analysis succeeds only in constrained domains in which grounding for its probabilities can be found. Elsewhere it returns meaningless numbers that can mislead profoundly. Second, the Bayesian view mischaracterizes the nature of inductive inference. It regards it as a branch of mathematics, so that the explication of inductive inference is largely the deriving of theorems in the probability calculus. I will argue below that inductive inference is better characterized as an inseparable part of empirical science.

Elsewhere, I have joined a minority tradition of Bayesian critics and written at some length on the shortcomings of Bayesian confirmation theory. See Norton (2008, 2010, 2010a, 2011). Here I will work through one problem to illustrate some of the lingering weaknesses of Bayesianism. Norton (2010) identifies the "inductive disjunctive fallacy" to which Bayesians are prone. It arises as follows. Assume that we have a very large number N+1 of mutually exclusive and exhaustive outcomes $a_0, a_1, \ldots, a_N$. Now assume that we simply have no evidence that supports any of the outcomes whatever. It is not that we have no grounds that favor any one outcome over another. Rather our evidence is completely bereft of anything helpful in deciding their truth or discriminating among them.

How can a Bayesian characterize this circumstance of what I shall call "completely neutral inductive support"? Each of $a_0, a_1, \ldots, a_N$ must be assigned a very small probability, spreading the measure widely, for otherwise we are favoring one. Write this as

$$P(a_i) = \text{small}_i \ \text{ where } i = 0, 1, \ldots, N.$$

The actual value small_i assigned to each a_i can vary. They definitely need not all be the same. They merely need to be very small and non-zero, for a zero amounts to a negative certainty. We now compute the probability assigned to the disjunction of all the outcomes excluding a_0:

$$P(a_1 \text{ or } \ldots \text{ or } a_N) = P(a_1) + \ldots + P(a_N) = \text{small}_1 + \ldots + \text{small}_N$$

Since the sum of all the probabilities must be unity ("additivity"), we know that

$$1 = \text{small}_0 + \text{small}_1 + \ldots + \text{small}_N$$

so that

$$\text{small}_1 + \ldots + \text{small}_N = 1 - \text{small}_0 = \text{nearly-one}$$

This last sum must be nearly one since small_0 is very small. Combining we have

$$P(a_1 \text{ or } \ldots \text{ or } a_N) = \text{nearly-one}$$

That is, we are now near certain of (a_1 or ... or a_N) or, equivalently, near certain that the outcome is not a_0.

Recall the initial assumption: we simply have no evidence at all concerning the truth of the outcomes. Yet a simple, rather mindless manipulation of the probabilities has given us near certainty, in contradiction with our initial assumption. Hence I characterize this inference from no evidence about the outcomes to near certainty an inductive fallacy, the "inductive disjunctive fallacy."

One might imagine that no one would seriously fall into the mechanical manipulation of probabilities that leads to the fallacy. It turns out that there are many instances of it, as recounted in Norton (2010, Section 4). For example, Van Inwagen (1996) uses it to answer the (pseudo) profound question (p. 95) "Why is there anything at all?" The answer proceeds, in effect, by attaching the possible world with no beings to outcome a_0, since there can be only way to have no beings. All the very many possible worlds with beings are attached to outcomes $a_1, \ldots, a_N$. It is now concluded that outcome a_0 of a world with no beings is (p.99) "as improbable as anything can be."

Once the fallacy is displayed, its source is clear. It stems directly from the key formal property of a probability measure: it is an *additive* measure. All the probabilities of mutually disjoint outcomes must add to unity. Hence we have no way to represent completely neutral support. If we assign a very small probability to some outcome, additivity forces us to assign high probability to the disjunction of the rest. That is we assign strong support or belief to the original outcome's negation; and that is incompatible with having completely neutral evidential support.

There are, broadly speaking, two ways Bayesians can respond. I categorize them as an "inelastic" and an "elastic" response. With the inelastic response, the Bayesian insists that the probabilistic computations must be respected. There is no fallacy. There is merely an error in our interpretation. Perhaps we should simply discount the possibility of completely neutral evidence at the outset. Or, more credibly, a subjective Bayesian may insist that all the probabilities involved are arbitrarily selected, subjective beliefs, akin to the way subjective Bayesians treat prior probabilities. The difficulty with this last response is that, once we discount the probabilities as expressing mere opinion, they cease to represent degrees of inductive support, as they should in an inductive logic. The probabilities are supposed to transform from pure opinion to a measure inductive of inductive support as conditionalization proceeds. However there is no objective criterion in the system that tracks the conversion. Merely having a high probability is not enough to show that the conversion is near completion, as the inductive disjunctive fallacy illustrates.[5] Given these failures, in my view, the only viable, inelastic response is simply to accept that there are cases that elude the Bayesian system and that this is one of them. The computation is mathematically correct but merely inapplicable to the case at hand.

The elastic response accepts that the additivity of a probability measure prevents it representing directly situations of completely neutral inductive support. However probability measures can be used indirectly to represent them. The proposal is to replace a single probability measure with a set of them. Complete neutrality of evidential support would be captured by allowing all probability measures over the outcome space into the set. The proposal is appealing initially since the complete neutrality of inductive support appears to be captured by allowing in everything possible in the probabilists' repertoire. If everyone speaks equally, no one is favored.

While the proposal fails for technical reasons,[6] it should be resisted by probabilists for a principled reason. It is, in effect, giving up the core of the probabilists' theory: that relations of inductive support are represented by an additive measure. The elastic response allows that the proper account of inductive support includes non-additivity. Probability measures are demoted to artifices. That is, they become adjunct structures used to simulate another non-additive logic whose principles are not clearly articulated. We no longer have a probabilitistic logic of induction. Rather we have an elastic language that is deformed as needed to accommodate whatever inductive behavior is deemed appropriate in the case at hand.

In sum, both elastic and inelastic responses lead to the same outcome: there are inductive problems that lie outside the Bayesian reach.[7] We arrive at this directly from the inelastic response and also from the elastic response since the latter reduces additive probability measures merely to tools used in the simulation of non-additive inductive relations of support.

---

[5] This version of the inductive disjunctive fallacy includes no conditionalization. However conditionalization could be added merely by extending the a_i to include very many more outcomes $a_{N+1}, \ldots, a_M$, and then conditionalizing on all the outcomes excluding these.

[6] Completely neutral support should be invariant under negation. Sets of probability measures, no matter how extensive, do not exhibit this invariance. See Norton (2007, §6).

[7] For another example see Norton (2010a).

3. Material Theory of Induction

Examples such as these indicate that there is no formal account of inductive inference that succeeds universally. However inductive inferences succeed. These examples also suggest how that is possible. In the case of inference to the best explanation, we saw that explanation itself played a negligible role. What supported the conclusions were background facts. That idea, taken to its extreme is the core thesis of a material theory of induction:

Inductive inferences are warranted by facts, not by formal schemas.

The clearest illustration is in Curie's induction on radium chloride. The attempt to explicate it with the schema of enumerative induction failed. We could not justify why the schema should be limited precisely to the few properties of radium chloride that Curie so confidently generalized.

The justification for this restriction cannot be found in any formal analysis of predicates and properties. Rather, it lies in the researches of chemists in the nineteenth century. The core result is known as "Haüy's Principle" and is named after one of its earliest proponents, Reny Just Haüy. It asserts that, *generally*, each crystalline substance has a single characteristic crystallographic form. The principle is grounded in extensive researches into the chemical composition of crystalline structures and into how their atoms may be packed into lattices. That means that once one has found the characteristic crystallographic form of some sample of a substance, generally one knows it for all samples.

Curie's inductive inference is warranted by Haüy's Principle and not by conformity to any inductive inference schema. There is an inductive risk taken in this conclusion, as indicated by the "generally" in the principle. Some substances admit polymorphism, which means that they form more than one type of crystal.

We can now see why Curie's induction is limited specifically to the crystallographic form of radium chloride rather than the many other properties of Curie's one tenth gram sample: Haüy's Principle is restricted precisely in this way. Indeed, its formulation is extremely hard won. We now know that all crystals fall into one of seven crystallographic families. They are defined by the axes characteristic of the crystalline lattice.[8] Discerning these families constituted

---

[8] The simplest system is the cubic system. When one learns that table salt form cubic crystals, one might imagine that its crystals are all little cubes. They are not. Rather they are many shapes with the distinctive property of being derivable from cubes by cleavage along cleavage planes.

a major mathematical challenge and it was only after the mathematical problem was solved that truly reliable inductive inferences on crystalline forms were possible. When Curie identified radium chloride crystals as just like those of barium chloride, she was adopting the expediency of not specifying the family formally, but merely allowing that it was the same as that of barium chloride. This in turn lent credence to her induction since another principle of chemistry, the law of isomorphism, allowed that analogous chemicals formed similar crystals.

This core idea of the material theory of induction can be applied in the other examples. Probabilistic induction is warranted, according to the material theory, just in so far as there are background material facts authorizing the use of probabilities to represent degrees of support. Such circumstances might include inferences over populations where physical probabilities are introduced through an assumption of random sampling. In the case of the inductive disjunctive fallacy above, the probabilistic analysis failed since, by careful design, the problem situation is bereft of just the facts needed to authorize the use of probabilities in inductive inference.

4. A General Argument for the Material Theory of Induction

There are two major premises in the general argument. The first is:

1. Deductive inference is not restrictive; inductive inference is restrictive.

This premises expresses the distinction traditionally drawn between deductive and inductive logic. Deductive inference is not restrictive in the sense that the conclusion of a deductive argument expresses no further factual restriction than that already expressed by the premises. Inductive inference is restrictive in the sense that the conclusion of an inductive inference must prohibit some of the possibilities that are logically compatible with the premises, else it would be deductive.

Hence, inductive inferences are warranted by facts.

For the prohibition in an inductive argument is just a factual, that is, contingent, restriction on what is possible. The inductive inference is valid just in case that contingent restriction is true. This is still true in the case of probabilistic inductive inference. To say a circumstance is extremely probable or improbable is to assert a factual claim, albeit a probabilistic one.

The second premise is:

2. There is no universally applicable warranting fact for inductive inferences.

One might to try to warrant inductive inferences by means of a universal fact. Such was the proposal by Mill (Bk. III, Ch. III, p. 223) when he sought to ground induction in the "… universal fact, which is our warrant for all inferences from experience, … that the course of nature is uniform..." However no such, singular fact has been forthcoming. Rather, as we see in the examples of this paper, the facts warranting inductive inferences are varied and show no indication of deriving from a single, common, universal fact. The warranting facts may impose some regularity on the inductive inferences that they support. Those regularities, when described systematically, will form an inductive logic. Since there is no universal warranting fact, the resulting inductive logics will be applicable only to the restricted domains in which the warranting facts obtain.

Hence, all induction is local.

5. Illustration: Galileo's Law of Fall

An illustration will show once again the necessity of background facts for inductive inference to be supported. Here is an insoluble inductive problem. Given the first members of a sequence of numbers: 1, 3, 5, 7, …, what comes next? There are many choices. We could continue as the odd numbers: 1, 3, 5, 7, 9, 11, … ; the odd prime numbers including 1: 1, 3, 5, 7, 11, 13, …; countably many more continuations for which rules can be given;[9] and uncountably many more for which no finite rule can be given. With the problem as posed, we have no means to discern among the possibilities. No inductive logic can help us.

What makes the problem inductively insoluble is that the factual context in which the sequence arises is not specified. Once we know the factual context, we can rule out some, many or most of the possible continuation. We can infer inductively.

What we infer will depend sensitively on the background facts. There are many possible factual contexts in which these numbers may appear. They may merely be the numbers read from

[9] The function f(n) = (2n-1) +(n-1)(n-2)(n-3)(n-4) g(n) returns the original sequence 1, 3, 5, 7 for n = 1, 2, 3, 4. But for n = 5, 6, 7, … it returns different numbers according to the arbitrary selection of the function g(n).

the right hand pages of a book; or from the decimal expansion of 359/2,645.[10] Or they may be the numbered balls drawn by a randomizing lottery machine. Or they may be numbers offered to us in a question in an IQ test. Or they may be numbers devised by a clever psychologist who plans to deceive us. Once we know these background facts, the possibilities are reduced and an inductive inference is possible.

The inferences will be fully controlled by these facts and different in each case. If the numbers are page numbers, we will expect the continuation as the familiar odd numbers. If the numbers are lottery drawings, we will spread our expectations probabilistically over the remaining numbered balls. The cases of the IQ test and the deceiving psychologist are more complicated. Each of these background facts will engender a different inductive logic that applies just to the domains in which those background facts prevail.

Let us pursue one case. The numbers 1, 3, 5, 7, … turn out to be classics in the history science. Galileo's (1638) *Two New Sciences* presents Galileo's law of fall in several forms: the speed of fall increases in proportion to the time of fall; or the distances fallen increases with the square of the time of fall. It could also be expressed so: (Third Day, Naturally Accelerated Motion, Thm. II, Prop. II, Cor. I)

> Hence it is clear that if we take any equal intervals of time whatever, counting from the beginning of the motion, such as AD, DE, EF, FG, in which the spaces HL, LM, MN, NI are traversed, these spaces will bear to one another the same ratio as the series of odd numbers, 1, 3, 5, 7;…

That is, a freely falling body falls incremental distances 1, 3, 5, 7 in successive units of time. Thus the total distances fallen in the successive units of time are 1, 1+3 =4, 4+5=9, 9+7=16, and we recover the more familiar squares of the times.

These incremental distances may have a more direct place in Galileo's discovery. Stillman Drake (1978, p. 89) conjectures that Galileo may have measured experimentally the distances a body falls in equal times by using the surrogate for free fall of a ball rolling down a groove in an inclined plane. Gut frets were arranged across the groove so that the noises made by the passing ball beat a uniform rhythm in time. Then the spacing of the frets would measure the

[10] 359/2,645 = 0.13572778828…

incremental distances. Drake's text reproduces a Galileo manuscript (p.87) in which, Drake believes, Galileo recorded the positions of the gut frets.[11]

We will never know exactly how Galileo posed the inductive problem to himself. So let us pose a Galileo-like problem in which we are allowed only to use the resources available to Galileo. We imagine that Galileo has measured near enough that incremental distances fallen in unit time are in the ratios 1 to 3 to 5 to 7. What is the continuation?

Without some further background assumption, nothing can be inferred. Galileo apparently assumed that the continuation is governed by a simple rule, expressible in the limited geometric and arithmetic language available to him. This immediately directs him to the odd numbers for incremental distances and the squares for total distances fallen.

Did Galileo make this assumption explicitly? It is indicated informally in *Two New Sciences* when Galileo introduces the gains of speed in free fall with the rhetorical question (p. 161)

> …why should I not believe that such increases take place in a manner which is exceedingly simple and rather obvious to everyone?

There is a stronger statement in the Assayer. Galileo (1623, pp. 237-38) writes:

> Philosophy is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth.

Galileo does not present the Platonic assumption as abstract metaphysics. It is a methodological guide. It is also a factual assumption. There are many ways things might be in the world. Galileo's Platonism rules out all possibilities save those that can be simply described in the language of mathematics.

---

[11] They were 33; 130; 298; 526; 824; 1,192; 1620; 2,123 (corrected to 2,140). A short computation (by me) shows that the intervals between these distances, taking 33 to be the unit, are: 1; 2.94; 5.09; 6.91; 9.03; 11.15; 12.97; 14.67; which are quite close to the odd numbers 1, 3, 5, 7, 9, 11, 13, 15.

The restriction to simple rules is powerful. But it is not powerful enough rule out all continuations other of 1, 3, 5, 7. One further, often overlooked assumption rules these others out. Galileo's ratios of 1 to 3 to 5 to 7 to … for the incremental distances fallen in unit time succeeds whatever unit is taken for time. It might be a second, a half second, a pulse beat, and so on. The same is true for the total distance fallen. Their ratios are always the squares: 1 to 4 to 9 to 16 to … To see how this works arithmetically, take the incremental distances:

$$1, 3, 5, 7, 9, 11, 13, 15, 17, 19, \ldots$$

Now choose a new unit of time, equal to two of the old units. Hence the incremental distances fallen in the new doubled units of time are:

$$1+3, 5+7, 9+11, 13+15, 17+19, \ldots$$

$$= 4, 12, 20, 28, 36, \ldots$$

$$= 4\text{x}1, 4\text{x}3, 4\text{x}5, 4\text{x}7, 4\text{x}9, \ldots$$

The ratios 1 to 3 to 5 to 7 to … are preserved.

Galileo knew this. He wrote in Corollary 1 above that the result holds if we select "any equal intervals of time whatever." It is a remarkable fact strongly suggested by his experiments. Galileo had no accurately measureable standard unit of time. He had no atomic clock that could deliver one second with extraordinary precision. His units of time were selected arbitrarily in the context of the experiment. When an arbitrary selection of a unit of time delivers just the ratios 1 to 3 to 5 to 7 to …, either Galileo happened by sheer good fortune onto just the right unit of time; or any selection of unit will return the same result. Galileo clearly chose the second option.

This insensitivity to choice of unit is a powerful factual restriction. Virtually all laws of fall will not respect it. Consider, for example, fall in a resisting medium. It initially follows Galileo's law but then asymptotically approaches a limiting constant velocity. The motion will require a time, characteristic of the specific arrangement, to achieve this terminal velocity, near enough. That time parameter gives the motion a definite temporal scale and precludes preservation of the law under a change of the unit of time.

Galileo could quickly affirm, as we did above, that his law of fall respects this invariance under the selection of the time unit (to use a slightly more modern phrasing). He would also have found it impossible to write any other simple law of fall that conformed to it, while preserving the initial segment of incremental distances 1, 3, 5, 7. We do not know if Galileo recognized just how complete this restriction is. Mathematical techniques not available to Galileo show that the *only* laws of fall that respect this invariance have the total distance fallen growing as a simple power of time. (See Norton, 2014a.) These yield a correspondingly restricted set of laws for the incremental distances. The incremental distance d(t) fallen in the unit of time between times (t-1) and t satisfies:

$$d(t) \text{ is proportional to } t^p - (t-1)^p$$

where p is any real number greater than 0. The only case of linear dependence of d(t) on t arises when p=2 for then

$$d(t) \text{ is proportional to } t^2 - (t-1)^2 = t^2 - (t^2-2t+1) = 2t-1$$

These are the odd numbers of Galileo's law, for when t = 1, 2, 3, ..., 2t-1 = 1, 3, 5, …

We need no appeal to simplicity to reduce the possibilities to this one law. Since it has just one free parameter, p, very little data eliminates all the rest. For example, take just the first two numbers 1, 3 of the initial sequence. We have d(2)/d(1)=3 and p must satisfy

$$3 = \frac{2^p - (2-1)^p}{1^p - (1-1)^p} = \frac{2^p - 1^p}{1^p} = 2^p - 1$$

The unique solution is p=2.

In sum, the premise of the inductive inference is a measurement of incremental distances of fall in the ratio 1 to 3 to 5 to 7. The conclusion is that distances of fall in general conform with Galileo's law. The material facts that warrant it are (a) (Platonic assumption) that fall conforms to a rule that may be written simply using techniques available to Galileo and (b) (invariance assumption) that the law is invariant under a change of the unit of time. It turns out that (b) alone is sufficient to warrant the inference, which is something Galileo may have suspected, but likely could not have shown.

6. The Superfluity of a Formal Theory

We might say that Galileo's law of fall is the best explanation for the numerical regularities found in the experiments. However declaring it so adds nothing of any use to the material analysis already given. Once we make the Platonic and invariance assumptions just listed, we have specified the result. At best, the declaration of a best explanation gives us a sense of comfort with the inference. At worst, it creates a spurious unity with other inductive inferences that are intrinsically different from it, but now collected under the umbrella of "best explanations." We are misled into seeing a principle of inductive logic, where there is nothing beyond superficial similarity.

We can embed Galileo's inference into a Bayesian analysis. The Platonic assumption can be expressed as a prior probability distribution over various possible laws that accords higher probability to the simpler law, such as in Jeffreys' (1961, §1.61). We might then also incorporate the invariance assumption into the likelihoods. We would then carry out the computations required by Bayes' theorem and discover the happy outcome that Galileo's law of fall is accorded high probability.

Once again, nothing of value has been added to the analysis given in the preceding section. We have just obscured a simple inductive inference behind a fog of probabilities. Without the Platonic and invariance assumptions, a Bayesian analysis is unable to deliver any result. But once we make those assumptions, we have no need of the Bayesian analysis. It is superfluous. At best we have merely given a probabilistically-minded philosopher a sense of comfort with the result. At worst, we have misled ourselves into thinking that inductive inference is merely a sub-branch of the mathematics of the probability calculus.

7. Conclusion

Inductive inference has been a favorite target of skeptical assault for millennia. It has been so not because of any special malice amongst skeptics against inductive inference. It has been so because it is troubled and those troubles lend themselves to skeptical formulations.

My claim in this paper is that the root cause of the fragility of inductive inference is that, for millennia, we have sought to model our accounts of it on deductive inference. That is, we have sought formal theories of inductive inference in which good inductive inferences are warranted by conformity to universally applicable schemas. The correct account, however, is a material account in which inductive inferences are warranted by facts. Underlying this is a change in our understanding of the nature of inductive inference. It is not a branch of mathematics to be studied in the abstract. It is an inseparable part of the empirical content science.

I doubt that this reorientation will resolve all skeptical challenges to inductive inference. It would be foolish to circumscribe the ingenuity of a skeptic. However notorious skeptical problems concerning inductive inference evaporate. For example, Goodman's (1983, Ch. III) "grue" challenge fails. Nineteenth century crystallographers faced extraordinary difficulties in determining the very few predicates that could be projected for materials like emeralds. Grue-ified predicates are not among them and also probably not "green."[12] (One might be tempted to reanimate the problem by the strategy of "grue-ifying everything," including the background facts that pick out projectible predicates. The reanimation fails since changing everything turns out to be indistinguishable from changing nothing. See Norton (2006).)

More significantly, *the* skeptical problem, the Humean problem of induction, is dissolved. It is argued in Norton (2014) that setting up the problem in the first place requires the separation of the matter of inductive inference from the warranting structures; that is, from the formal schema to which a formal theory requires inductive inferences to conform. This separation of matter from schema leaves a formal theory impoverished in its justificatory resources; and sufficiently so that the Humean skeptical challenge is easy to mount. If one approaches inductive inference materially, however, the distinction of matter from warranting structure dissolves. It turns out that one can then no longer set up the traditional Humean challenge.

References

Broad, C. D. (1926). The Philosophy of Francis Bacon: An Address Delivered at Cambridge on the Occasion of the Bacon Tercentenary, 5 October, 1926. Cambridge: Cambridge University Press.

Davisson, Clinton J. and Germer, L. H. (1927) "Diffraction of Electrons by a Crystal of Nickel," *Physical Review*, **6**, pp. 705-740.

[12] It turns out that all emeralds are green not because of any determining property of the mineral beryl that forms emeralds, but because gemologists decree that only beryl colored green by impurities can be called emerald. That is, "all emeralds are green" is simply true by definition. "All emeralds are grue," turns out to be a contradiction.

Davisson, Clinton J. (1937) “The Discovery of Electron Waves,” Nobel Prize Lecture, December 13, 1937. http://www.nobelprize.org/nobel_prizes/physics/laureates/1937/davisson-lecture.pdf

Drake, Stillman (1978) *Galileo at Work: His Scientific Biography*. Chicago: University of Chicago Press. Repr. Mineola, NY: Dover, 2003.

Galilei, Galileo (1623) “The Assayer,” pp. 231-280 in Stillman Drake, ed. and trans., *Discoveries and Opinions of Galileo*. New York: Doubleday & Co., 1957.

Galilei, Galileo (1638) *Dialogues Concerning Two New Sciences*. Trans. Henry Crew and Alfonso de Salvo. MacMillan, 1914; repr. New York: Dover, 1954.

Goodman, Nelson (1983) *Fact, Fiction and Forecast*. 4th ed. Cambridge, MA: Harvard University Press.

Jeffreys, Harold (1961) *Theory of Probability*. 3rd ed. Oxford: Clarendon Press.

Lipton, Peter (2004) *Inference to the Best Explanation*. 2nd ed. London: Routledge.

Mill, John Stuart (1882) *A System of Logic*. 8th ed. New York: Harper & Bros.

Navarro, Jaume (2010) “Electron diffraction chez Thomson. Early responses to quantum physics in Britain,” *British Journal for the History of Science*, **43**, pp. 245-275.

Norton, John D (2003) "A Material Theory of Induction" *Philosophy of Science*, **70**, pp. 647-70.

Norton, John D (2005) "A Little Survey of Induction," in P. Achinstein, ed., Scientific Evidence: Philosophical Theories and Applications. Johns Hopkins University Press, 1905. pp. 9-34.

Norton, John D (2006) "The Formal Equivalence of Grue and Green and How It Undoes the New Riddle of Induction." *Synthese*, **150**, pp. 185-207.

Norton, John D (2007) "Disbelief as the Dual of Belief." *International Studies in the Philosophy of Science*, **21**, pp. 231-252.

Norton, John D. (2008)"Ignorance and Indifference." Philosophy of Science, **75**, pp. 45-68.

Norton, John D. (2009) "A Survey of Inductive Generalization." http://www.pitt.edu/~jdnorton/homepage/cv.html#survey_ind_gen

Norton, John D. (2010) "Cosmic Confusions: Not Supporting versus Supporting Not-". *Philosophy of Science*. **77**, pp. 501-23.

Norton, John D (2010a) "There are No Universal Rules for Induction," Philosophy of Science, **77**, pp. 765-77.

Norton, John D. (2011) “Challenges to Bayesian Confirmation Theory,” *Philosophy of Statistics, Vol. 7: Handbook of the Philosophy of Science*. Prasanta S. Bandyopadhyay and Malcolm R. Forster (eds.) Elsevier.

Norton, John D. (2014) "A Material Dissolution of the Problem of Induction." *Synthese*. **191**, pp. 671-690.

Norton, John D. (2014a) “Invariance of Galileo’s Law of Fall under the Change of the Unit of Time.” http://philsci-archive.pitt.edu/id/eprint/10931.

Pleijel, H. (1937) "Nobel Prize in Physics 1937 - Presentation Speech," http://www.nobelprize.org/nobel_prizes/physics/laureates/1937/press.html

Sklodowska Curie, Marie (1904) *Radioactive Substances*. 2nd ed. London: Chemical News Office.

Thomson, George P. (1937) “Electronic Waves,” Nobel Prize Lecture, June 7, 1938. http://www.nobelprize.org/nobel_prizes/physics/laureates/1937/davisson-lecture.pdf

Thomson, Joseph John (1897) “Cathode Rays,” *Philosophical Magazine,* **44**, pp. 293-316.

Van Inwagen, Peter (1996) “Why Is There Anything at All?” *Proceedings of the Aristotelian Society* **70** (suppl.), pp.95–120.