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Chapter from a book, *The Material Theory of Induction*, now in preparation.

What Powers Inductive Inference?

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1. Introduction

This chapter summarizes the case for the material theory of induction, drawing on material in other parts of this text. There are three arguments for the material theory of induction. The first two are:

1. Failure of universal schema. Through many examples in this text, we see that no attempt to produce a universally applicable formal theory of induction has succeeded

2. Accommodation of standard inferences. These same examples show that the successes of many exemplars of good inductive inferences can be explained by the material theory of induction.

These first two arguments suffice, I believe, to make a solid case for the material theory. They are developed in Sections 2 and 3. They make the case without giving an intuitive grounding for why the material approach is the right one. They establish *that* it is, not *why* it is. For the

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arguments succeed by showing that the other, formal approach fails and that the material approach does work where its competitor fails. The third argument, however, is grounded in the foundational question developed in Section 4 of why any inductive inference should work at all. That is, it asks "what powers inductive inference?" The question presumes that we cannot take the success of inductive inference for granted. If it works, it does so for an identifiable reason. The material theory answers that inductive inference is powered by facts. For emphasis:

3. Inductive inference is powered by facts.

There are two steps in the argument developed more fully in Section 5.

The first step notes that inductive inference is, by its nature, ampliative. That is, unlike deductive inference, the conclusion asserts more than does the premises. It amplifies what the premises say. For each sort of inductive inference, there will be worlds hostile to its success. Generalizing chemical properties of samples, for example, is futile in a world without stable chemical properties. Using an inductive inference presupposes that, as a factual matter, we are not in one of those hostile worlds. If the notion of these facts is construed broadly enough, commitment to them is all there is to accepting the logic. These are the facts warranting the inductive inference.

The second step specifies the character of these facts. They are not universal contingencies such as would warrant a universally applicable inductive logic. That is shown by our failure to identify a universally applicable inductive logic and our failure to exhibit such a universally warranting fact explicitly. Rather the facts hold true only in limited domains, so that there are many of them and the inductive logic each warrants has local applicability only.

The two sections following provide illustrations of the two steps. Sections 6 and 7 consider the inductive problem of extending the series 1, 3, 5, 7. It is insoluble without background facts to warrant the inference. Section 8 displays some more examples of warranting facts. Finally, our predisposition for treating inference formally is strong. Section 9 will seek to weaken the presumption that all theories of inference must be formal by indicating limitations in the formal, non-contextual treatment of the most favorable case, deductive inference.

2. Failure of Universal Schema

Formal approaches to inductive inference depend upon supplying a universal template or schema. For example, in the last chapter, we saw the schema of enumerative induction:

Some (few) A's are B.

Therefore, all A's are B.

These templates are then used to generate the licit inductive inferences by substitution of content for the placeholders A and B. The enduring difficulty for formal theories is that no general account of inductive inference has provided a clearly articulated, exceptionless schema. Therefore, all formal accounts fail and, by elimination of its only known rival, we gain support for the material theory.

That all the schema fail is hard to show directly since there are many of them. What can be shown, however, is the failure of a representative sample, as is done in various chapters of this text.[2] The mode of failure displayed by this sample is sufficiently straightforward to make it likely that it will afflict all candidate schemas.

In the preceding chapter, we saw in the example of crystalline form that the schema of enumerative induction fails. For it to be applied successfully to crystalline forms, we needed to add additional, formal conditions contrived to rule out all but the very small set of properties of crystals that support inductive generalization. The sequence of additional conditions seemed to have no discernible end. Once even a few were added, however, it was already clear that the schema had lost all semblance of generality.

In the next chapter, we will look at the requirement of the reproducibility of experiments, which is often introduced as a gold standard of evidence. On closer examination, however, it proves to be something less. It is a guide whose verdict is sometimes accepted and sometimes discarded. There is no formal rule that tells us when the principle is to be upheld and when not. It is a principle that holds except when it does not.

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[2] In earlier work (Norton, 2003, 2005), I sought to be more systematic. I showed how virtually all accounts of inductive inference fell into one of three families, each being powered inductively by a single idea. Since the failures in the earlier work are here are spread over the three families, we have some assurance that they are adequately representative of the range of accounts.

A following chapter looks at reasoning by analogy. It is a form of inductive inference whose use has pervaded science from antiquity to the present. Once again we find that the bare schema is too impoverished to be used exceptionlessly. Efforts over the past century to augment the schema have led to supplements of monumental size while still not delivering a self-contained formal schema. This pattern of failure continues in subsequent chapters that consider inductive inferences supported by considerations of simplicity or explanatory success. They appear superficially to depend on inductive principles pertaining to simplicity and explanation. But closer examination fails to yield a serviceable formulation of the principles that is developed beyond mere slogans.

Finally, the most popular account of inductive inference is, at present, the Bayesian account. Its problems will be the subject of the third part of this book. We shall see that the account fails to be universally applicable. There are examples of inductive problems to which it cannot be applied; or at least efforts to do so require stratagems that compromise the account as an inductive logic. A preview of one problem facing the Bayesian account is provided in Section 7 here.

These examples embody modes of failure that afflict, I believe, all candidates for universal schema of inductive inference. The schema may simply be too vaguely specified at the outset to count as a logic of induction, as is the case with inference to the best explanation. Or, if they are precisely specified, they prove too permissive and authorize far too much, such as enumerative induction. Efforts to restrict them may specialize them so narrowly to one particular domain that they lose their universality. Or these efforts may burden them with more conditions. In adding them, we may need to import new notions—natural kinds, explanation, lawfulness—and these in turn require further conditions for their explication; and so on without termination.

3. Accommodation of Standard Inferences

The last section reviews the failure of familiar, formal schema for inductive inference. These schemas were devised because, to some degree, each fits some collection of inductive inferences we deem licit. The second argument for the material theory is merely the other side of the coin of this failure. Where the formal approach fails for these repositories of licit examples, the material theory succeeds.[3]

Once again this can be read from the analyses of the surrounding chapters. Curie inferred inductively from the crystalline form of mere specks of radium chloride to all samples of radium chloride. What licensed the inference was a fact hard won from the preceding century's work on crystals. It is what I have called the Weakened Haüy's Principle: *Generally*, each crystalline substance has a single characteristic crystallographic form.

In the next chapter, we visit the requirement of the reproducibility of experiments. The requirement proves not to be a universal inductive principle. Rather it arises in connection with a loosely affiliated but irregular collection of inductive inferences concerning repeated experiments. Their otherwise inexplicable irregularity become intelligible when we recognize that the inferences are warranted by two classes of facts: those specifying when some process will yield the experimental outcome of interest; and those specifying what may confound the experimental outcome. These facts specify when a replication of an experiment is evidentially significant. More important, they specify when the replication is not evidentially significant. The variation in the facts from case to case explains the irregularity of the whole collection.

Arguments from analogy are so varied in their form that, as we shall see in a coming chapter, they defy complete characterization even by quite elaborate formulae. The material theory resolves the problem by reconceiving analogy as a factual matter. There is a fact of analogy that asserts the similarity of two systems. This fact warrants inductive analogical inference. The resulting inferences have as varied a form as the facts of analogy themselves. It is this broad range of variation that defeats efforts to find a universal formal characterization.

This pattern persists with the analysis of inductive inferences grounded in notions of simplicity or explanation. Invocations of simplicity in specific cases prove to be abbreviated invocations of background facts. Similarly, in specific inferences to the best explanation, that an

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[3] Norton (2003) works through the three families of accounts of inductive inference showing briefly how the inferences of each account are materially warranted. The treatment of so many accounts there is necessarily brief. The present text seeks to show the material warrant for standard examples of successful inductive inferences at much greater depth. As a result, fewer examples are treated.

explanatory relation exists by itself contributes little to the evidential import. That import is derived from specific background facts that vary from case to case.

Finally, it will be argued in Part III of this text that the probability calculus does not comprise a universal logic of induction. However there are many cases in which it is demonstrably the right formal system for inductive inference. These are cases in which there are hospitable facts that warrant the introduction of probabilities. For example, if the individuals who are the subject of our inferences are selected randomly from a population, then the fact of random selection introduces probabilities to which our priors in Bayes' theorem should be matched. If we have controlled studies that tell us the probability that some combination of traits in individuals results in a certain illness, then our Bayesian likelihoods should match those probabilities. In such cases, inductive support conforms with the Bayesian system, but only because in this domain there are identifiable facts that warrant it.

4. The Mystery of Inductive Inference

The discussion so far has been devoted to the most visible of the problems associated with inductive inference:

(*which?*) Which are the good inductive inferences?

An answer specifies how we distinguish the good from the bad inferences. The material theory of induction says we do it by identifying warranting facts not licensing universal schema. That problem is entangled with another problem that is more fundamental, but is largely overlooked in the present literature. How can inductive inference work at all? That is:

(*powers?*) What powers inductive inference?

Once we accept that inductive inference is powered by background facts, it becomes clear why identifying warranting facts has to be the answer to the "*which?*" question.

This second question "*powers?*" needs some elaboration. For it is easy to take for granted that induction lets us do something remarkable. It lets us amplify our knowledge. We pay a small price for this amplification. Our new knowledge is not as certain as the old knowledge from which we proceeded. Sometimes the uncertainty is large. In important cases, the uncertainty is miniscule. Whether it is small or large, we still seem to be getting more than we should. The problem, the big mystery of induction, is to understand how this amplification can happen.

To sharpen the sense of why we need a solution of this second problem, consider an analogous problem. Imagine that we find an oracle. In the darkness, we see the dim outline of the sibyl, wailing and flailing. Her cries reduce and focus into sharp proclamations that time proves to be important and accurate, mostly. And all this for the price of a goat and few drachma in the bronze bowl. Were this to happen, we would not be satisfied merely to note that this oracle has extraordinary predictive powers. We would and should want to know how they are possible. What is it in the order of things that enables this sibyl open the portal?

The puzzle is the same with induction. It performs a similar miracle, but without the movie-quality special effects. Experience gives us a small part of space for a small span of time. Yet from knowledge of that fragment, we come to be sure that all things began some 14,000,000,000 years ago in an intense conflagration; that tiny smudges of light in the night sky are great galaxies of stars that duplicate our sun many times; and much more, down to the miniscule structure of microbial life. We must ask, what is it in the order of things that allows induction to open this portal? What powers inductive inference?

The dominant trends in the present literature solve neither of these problems well. To solve them, the two problems need to be treated together. We cannot hope to know which are the good inductions without a clear and explicit idea of what powers induction. The literature so far has tried to solve the problems by working with the model of deductive inference. That has driven us astray, for millennia. It has led us to seek a non-contextual account of what powers induction (*powers?*) and a formal answer to the problem of which are the good inductive inferences (*which?)*. Neither work for induction. The central claim of this chapter is that a successful account of induction is contextual and material.

5. The Foundational Argument

The most compact argument for a material theory of induction proceeds by answering the foundational question of what powers induction. It is powered by facts. As indicated in the introduction, the argument has two steps.

Premise 1. Inductive inference is ampliative.

This means that the conclusion of an inductive inference amplifies. It asserts more than the premises. This distinguishes inductive inference from deductive inference. For deductive inferences merely restate what we have already presumed or learned. There is no mystery in what powers the inference and permits the conclusion. We are just restating what we already have in the premises. The warrant lies fully within the premises. If we know all winters are snowy, it follows deductively that some winters are snowy.[4] This derives from the meaning of "all." If something is true of all, it is thereby true of some. The context in which we infer plays no role in powering the deductive inference. The inference succeeds no matter what a winter or snowy might be. The meaning of "all" is enough to empower the conclusion non-contextually. The inference is valid independently of whatever other facts may obtain about weather and climate.

It is quite different with inductive inference. From the premise that all past winters have been snowy in some location, we infer inductively that the next winter will be snowy there. It is entirely possible that this prediction fails. When we conclude in its favor, we assert more that the premises. It is prudent to do so only in certain sorts of worlds. Hospitable ones include those in which climate in the location is stable. An inhospitable world would be one undergoing a warming climate change. We can generalize the crystallographic family of a crystalline substance from one sample to all because our world is hospitable through the background fact of Haüy's principle. But we cannot generalize the size of the one sample to all, for there are no background facts providing for restrictions on possible sample sizes. Correspondingly, we can generalize sizes of living organisms, for different types of organisms are restricted by their physical constitutions to specific scales. Insects cannot grow to human scales because their structures would be too weak to support their weight and they could no longer breathe by diffusion. Correspondingly, humans cannot be shrunk to insect scales. Their shrunken brains would have too few neurons for human cognition. At least this is true in our world, which is hospitable to the generalization, but not in a science fiction world in which normal science is suspended.

These examples illustrate the general point: the factual assumption that ours is a hospitable world is the fact that, if true, warrants the inductive inference. It may not always be apparent that this fact warrants the inference. It may appear that the warrant is still provided by

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[4] For pedants: I follow the informal conversational presumption and tacitly assume that "All winters are snowy." is not true vacuously; that is, its truth requires that there are some winters.

some sort of schema. The inference to a future snowy winter, we may think, is still warranted by the schema:

All past A's have been B

Therefore, the next A will be B.

The supposition incomplete. This schema, if used at all, has a purely intermediate role. It does not have universal applicability. We can use it in the snowy winter case only because the requisite background facts authorize it, when we make the specific substitutions: "winter" for A and "snowy" for B. That is, there is a cascade of warrants that may pass through a schema. The cascade terminates in facts that are the final warrant of the inference.

It is essential here to distinguish two ways that an inductive inference can fail: losing an inductive bet in a hospitable world versus failure of an inductive inference in an inhospitable world. The first case arises because accepting a warranted inductive inference still involves a risk. In a hospitable world of stable climate, it is a warranted inductive inference to infer from a past history of snowy winters that the next winter will be snowy. The next winter, however, may turn out not to be snowy. Such fluctuations are rarer, but quite possible when the climate is stable. Losing an inductive bet like this must be distinguished from the second case in which it was imprudent to take the bet in the first place. If the background facts are of a warming climate in some location, then the background facts do not warrant the inference. If one persists and makes the inference, the conclusion may prove false. The failure reflects the lack of warrant of the inference, not from traditional inductive risk.

The material theory of induction arises when we assume that the truth of these background factual presumptions is all that is needed for the inductive inference to be warranted. One might imagine that this might not be so. The facts, we might suppose, play only a partial role in warranting the inductive inference. Might there still be a residual universal formal schema or inductive rule that contributes to the warrant? Such a schema or rule, however, would in turn be subject to the same analysis just given. If it functions to authorize an inductive inference, then it is amplifying what we have already asserted in the premises and all other background facts. It cannot be universal in application for there would be worlds inhospitable to it. We should only use the rule or schema in worlds hospitable to it. That is, the warrant for its use is the factual supposition that world is hospitable to it. Once again the inductive warrant has terminated in facts that should be included with the true background facts needed to warrant the inductive inference at issue. That is, the truth of the background factual assumptions, when construed broadly enough, is all that is needed to authorize the inductive inference. With that, we arrive at the major tenet of a material theory of induction.

Hence, inductive inferences are warranted by facts.

What remains open is the precise character of the warranting facts. Aside from the next step, there is little we can say at the general level about the nature of these facts. In particular cases, their character will be straightforward. Our inference to a future of snowy winters is warranted by the assumption that our local climate will persist pretty much as it has, so that winters without snow are possible but unlikely. If the climate warms sufficiently, however, these facts may fail and with it the inductive inference.

In some cases, the background facts may be such that the inductive inference would be deductive if we add explicitly the warranting fact as a premise. Then the inference is revealed to be an enthymeme, a deductive inference with a hidden premise. An example is this version of Curie's inference from the preceding chapter:

> This sample of radium chloride is monoclinic.
> (Weakened Haüy's Principle) *Generally*, each crystalline substance has a single characteristic crystallographic form.
> ____
> Unless exceptions encoded by the "generally" of the principle intervene, all samples of radium chloride are monoclinic.

However, it would also be entirely natural to detach the "Unless…" clause and have the inference:

> This sample of radium chloride is monoclinic.
> (Weakened Haüy's Principle) *Generally*, each crystalline substance has a single characteristic crystallographic form.
> ____
> All samples of radium chloride are monoclinic.

This inference is inductive for we are taking the risk that the exceptions suggested by the *generally* do not arise.

Corresponding complications arise if we infer inductively in the Bayesian framework. If we infer from prior probabilities to posterior probabilities by means of likelihoods using Bayes' theorem, then the inference is deductive. If we broaden the context, this ceases to be so. Propositions asserting evidence and background facts are not provided to us with probability measures. We add them. In doing so, we accept that we can represent their mutual relations of inductive support probabilistically and that their inductive consequences follow from the probability calculus. In doing so, we take an inductive risk that probabilistic analysis correctly represents these relations. If we also proceed as normal people do and accept a proposition with very high posterior probability as established, then we take a second inductive risk in detaching the qualification of high probability.

The second step places a restriction on the character of the warranting facts:

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

This premise itself requires support. Part of it is supplied by other arguments in this book that seek to establish that there is no universally applicable logic of induction. For, if there were a universally applicable logic of induction, then by the first step above there would a universally applicable warranting fact.

A more direct grounding for the premise lies in our failure to exhibit such a universally applicable warranting fact. It has been long sought, like the philosopher's stone, and with equal success. The best known attempt at characterizing it is Mill's principle of the uniformity of nature: "The universe, so far as known to us, is so constituted that whatever is true in any one case is true in all cases of a certain description; the only difficulty is, to find what description." (Mill, 1904, Bk III, Ch.III, p. 223) and "Whatever may be the proper mode of expressing it," he wrote, "the proposition that the course of nature is uniform is the fundamental principle, or general axiom of Induction." (p. 224) It is a general fact about the world holding in all domains in which we may seek to infer inductively. It is the one, universal fact that would power all inductive inference.

The trouble with Mill's principle is that, read literally, it is false; but read charitably it is so vague as to be unusable. Take the literal reading. Our world is *not* uniform in all its aspects. Indeed the world fails to be uniform in virtually all its aspects. Otherwise we would live in a largely homogenous environment. At best, the world is uniform in a very few, quite special properties that end up figuring in what we take to be laws of nature. This last statement is the charitable reading. The real challenge now for the principle is to specify just which are those special properties. Yet through the vague generality of its formulations, it provides no such specification. At best, the principle deflates to a weak existential claim: there are uniformly implemented properties in nature, but we do not know precisely which they are. Or, more generally, Nature is regular and orderly, but in a way that we cannot state or grasp compactly enough to implement as a principle that can be employed practically in a logic of induction.

That the principle needs this shield of ignorance to protect it from scrutiny suggests that there is no real content hidden behind the shield. Certainly it has ceased to have any practical value in our inductive investigations. Salmon (1953, p. 44) long ago wrote the principle's obituary

> …the general result seems to be that every formulation of the principle of the uniformity of nature is either too strong to be true or else too weak to be useful.

This completes the argument for the premise.

If the facts warranting inductive inference are not universal truths, then they must be truths of restricted domains and the inductive inferences they warrant will be restricted to those domains. It may well happen that the inferences warranted in some restricted domain has a regular structure. Then we have an inductive logic applicable to just that domain. For example, Haüy's principle warrants an inductive logic that looks formally like enumerative induction, but is restricted just to generalizations concerning the crystallographic family of samples of crystalline substances. That is, we have the second major characteristic of a material theory of induction.

Hence, all induction is local.

The next two sections will supply illustrations of the first and second steps of this argument respectively.

6. The Inductive Inference on 1, 3, 5, 7.[5]

A rapid way to see the importance of background warranting facts is through an inductive inference problem that, by contrivance, is bereft of background facts. The problem is this:

> Given the initial sequence of numbers 1, 3, 5, 7,
>
> how does the sequence continue?

It is a trivial mathematical fact that the sequence could continue in any of very many ways. If the only restriction is that these are the first four terms of an infinite series, then there is an uncountable infinity of distinct continuations. The emptiness of the problem specification makes it impossible to favor any one of them, that is, to pick among the deductively authorized possibilities. Without some specification of background facts, to infer inductively about the continuation is impossible.

The possibilities are greatly restricted if we make the natural assumption that the sequence is governed by some simple rule. There are still many possible continuations. The sequence may just be the odd numbers:

1, 3, 5, 7, 9, 11, 13, 15, …

Or it may be the odd primes, including one:

1, 3, 5, 7, 11, 13, 17, …

Or it may be the digits of the decimal expansion of 359/2,645:

1, 3, 5, 7, 2, 7, 7, 8, 8, 2, 8, …

While the possibilities are restricted, the inductive problem is still intractable since the notion of "simple rule" remains under-specified. That makes finding other continuations merely a challenge to our ingenuity in writing laws that look simple in some sense we happen to find congenial.

Another approach embeds the sequence in a context for which we have more information. The numbers may be drawn from a randomizing lottery machine. Then the fact of randomization authorizes a probabilistic analysis. Probabilistic inductive support is distributed uniformly over the remaining, undrawn numbers. Or perhaps the numbers appear in a question on an IQ test or in the interrogation of a psychologist we believe is intent on tricking us. These differing

[5] This example and a briefer version of the argument of the previous section are given in Norton (2014).

background facts would then authorize different inferences over the continuations, although the complexity of the background will make discerning their precise character troublesome.

7. The Law of Fall

It is easy to suppose that this inductive problem is merely a philosopher's contrivance, unrelated to real problems of inductive inference in science and thus one that we need not strive to accommodate in our account. That supposition is wrong. The problem turns out be one of the classic problems of inductive inference in science. This particular number sequence happens to figure in one of the great discoveries in the history of science. In his *Two New Sciences* (1638), Galileo Galilei presented his law of fall. In one form, the law asserts that the distances fallen in successive units of time stand in the ratios 1 to 3 to 5 to 7 to…; that is, in the ratio of the odd numbers. Galileo's pathway to this law was long and convoluted. However at least one part of it quite likely involved experimentally measuring the distances bodies fall and the times taken. The *Two New Sciences* (1638, pp. 178-79) describes such an experiment in which a ball is timed rolling down a grooved ramp. The ramp is a surrogate for free fall that slows the motion sufficiently to enable time measurements using Galileo's crude methods. Stillman Drake (1978, p. 89) has identified an early Galileo manuscript that, Drake argues, records the results of just such an experiment.

So let us pose a simplified Galileo-like inductive problem. Given that the incremental distances fallen in successive units of time are in the ratios 1 to 3 to 5 to 7, what will be the distances in subsequent times? Using resources available to Galileo, how might this be solved?

We have a good idea of Galileo's methods. One element was that he presumed that fall is governed by a rule that is expressible simply in the mathematical techniques available to him. The idea is indicated in *Two New Sciences*. Galileo reflects on the gains in speed of falling bodies asks of them (p. 161)

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

Galileo's inference is warranted by a fact: the simple nature of this part of the world. This one statement leaves the notion of simplicity at issue underspecified and thus leaves underspecified just which inference is authorized. If we read more broadly in Galileo's writings, we find a stronger statement that identifies the notion of simplicity at issue. He wrote in a famous passage in the Assayer (1623, pp. 237-38):

> 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.

This is mathematical Platonism. It asserts that the world is structured as a copy of the perfect mathematical forms. This factual statement about the world then warrants an inference to a simple mathematical rule as the continuation of the sequence 1, 3, 5, 7, …

This approach may at first be appealing. The world does admit simple mathematical description. Why can we not use this fact to underwrite inductive inferences? The appeal fades rapidly under closer scrutiny. There are three problems.

First, if one is not a Platonist, the warranting fact is a falsehood and thus the inference an inductive fallacy. The success of mathematical methods in science since Galileo to the present do not, in my view, justify the Platonic view. Rather, as I have argued elsewhere (Norton, 2000, Appendix D), the success merely reflects the post hoc adaptability of mathematics to new scientific discoveries.

Second, attempts to employ the Platonic idea fall prey to the problem that mathematical imagination can conjure up vastly more structures than are implemented in reality. Seek simple laws written in the wrong mathematical language and our investigations will stall and fail. Einstein became a mathematical Platonist as part of his later-life search for a unified field theory. His efforts were stymied by just this problem, since he sought laws that are simple when expressed in the mathematics of tensors and the like on four-dimensional spacetime manifolds. Subsequent theorizing in quantum gravity has branched out in the mathematical structures it uses and typically does not posit a four-dimensional spacetime manifold as a primitive.

Third, when Galileo investigated falling bodies, the mathematics accessible to him was limited to methods drawn from Euclid. They comprise the barest sliver of the mathematics we now employ. It is more than optimistic to expect that the Platonic blueprint of nature is drawn with the mathematics of this tiny sliver.

8. Invariance Under the Change of the Unit of Time

In the face of these mounting difficulties, we may well wonder whether Galileo had sufficient background facts to warrant what still seems like it should be a good inference. Fortunately he did assume another background fact, perfectly tuned to warrant the inference and eliminate all but one of the open possibilities, although this aspect of his work typically gets scant attention in the history.

Galileo's experimental methods were unable to fix a precise unit of time. At best he could determine that, in one experiment, successive intervals of time were equal. He realized that his experimental result was stable in spite of this variability of the unit of time. In measuring fall, he recovered the same ratios 1 to 3 to 5 to 7 to … no matter what unit of time is used. This important fact is stated by Galileo when he presents this odd number formulation of his law of fall. He wrote (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;…

The invariance of the result is asserted by the italicized text (my emphasis).[6]

With a little arithmetic, we can see how this invariance under the change of unit of time works. In successive units of time, the body falls distances.

1, 3, 5, 7, 9, 11, 13, 15, 17, 19, …

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[6] Galileo's Latin *quotcunque tempora aequalia* is literally "however so many equal times." Crew and de Salvio render it as "any equal intervals of time whatever." Their looser rendering fits with the overall context in allowing both the number and duration of the intervals to vary. An important part of the context is the earlier statement of the law of fall from which this corollary is derived. The law is first introduced as (p. 161) "…during any equal intervals of time whatever, equal increments of speed are given to it." Galileo's Latin *dum temporibus quibuscunque aequalibus* is correctly rendered by Crew and de Salvio as "during any equal intervals of time whatever," where *quibuscunque* has no restriction to number or duration. These unrestricted, equal time intervals are the ones that reappear in Corollary I.

Now replace the original unit of time with a new one equal to two of the old units. The distances fallen in successive units of time with the new unit are:

$$1+3,\ 5+7,\ 9+11,\ 13+15,\ 17+19,\ \ldots$$
$$= 4,\ 12,\ 20,\ 28,\ 36,\ \ldots$$
$$= 4\text{x}1,\ 4\text{x}3,\ 4\text{x}5,\ 4\text{x}7,\ 4\text{x}9,\ \ldots$$

Galileo's law requires only that these distances be in the ratios 1 to 3 to 5 to 7 to … . Hence we can neglect the factor of 4 and observe that they conform to the law. This invariance, Galileo asserts, obtains no matter which unit of time we select.

The remarkable fact is that there are very few laws of fall that respect this invariance. Using techniques in the calculus and functional analysis not available to Galileo, it is possible to prove that the *only* laws are these. If d(t) is the distance fall in the unit of time (t-1) to t, then[7]

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

where p is any real number greater than 0. (See Norton, 2014a.) This means that prior to any measurements, the scope of the laws admissible is already reduced to these very few possibilities.

What now gives the inference great strength is the fact that there is just one free parameter in the law, p. It follows that securing just one ratio of distances experimentally fixes the law uniquely. For example, take the first ratio that Galileo would have measured, d(2)/d(1) = 3. It follows that 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, so that

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

Hence for successive times t = 1, 2, 3, 4, …, we have d(t) = 1, 3, 5, 7, …, that is, the odd numbers.

This is a remarkable result worth restating: if invariance under changes of the unit of time is to be respected, the only continuation of the two-membered sequence of incremental distances fallen

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[7] There is a suppressed proportionality constant in the statement. It is suppressed since Galileo's law concerns ratios of the quantities d(t) and the constant will not affect those ratios.

$$1, 3$$

is the sequence of odd numbers

$$1, 3, 5, 7, 9, 11, 13, \ldots$$

Of course Galileo could not know this result in all generality. However it is quite likely that he was aware of how restrictive the invariance is. One needs only to try out a few alternatives to the odd number sequence arithmetically to realize that all simple alternatives fail. Drake (1969, pp. 349-50) notes a correspondent of Galileo's, Baliani, reported that Galileo had used the invariance as a "probable reason" for the odd number rule.

While Galileo did not elaborate in *Two New Sciences* on this result, Christiaan Huygens soon did. That is, a seventeen-year-old Huygens, prior to his reading of Galileo's *Two New Sciences*, independently found the result.[8] One statement of what he found is given in a letter of October 28, 1646, to Marin Marsenne (Huygens, 1888, pp. 24-28). There Huygens arrived at his result by considering two possibilities: that the incremental distances fallen in subsequent, equal intervals of time grows in an arithmetic progression or in a geometric progression. Only one case gave non-trivial results: an arithmetic progression in the ratios of the odd numbers, 1, 3, 5, 7, … The demonstration is creditable, but less than general since it overlooks the possibility of expressions for the incremental distances d(t) with values of p other than 2 in the formula $t^p - (t-1)^p$. Thus it precludes by supposition many other progressions that would give laws of fall whose ratios remained unchanged under changes of the unit of time. While one might imagine ways that the demonstration could be rendered more general, there seems to be no obvious way to arrive at the general proof without mathematical techniques stronger than those available then to Galileo and Huygens, such as used in Norton (2014a).[9] This may explain why Galileo did not elaborate on the result in *Two New Sciences*.

Our Galileo-like inductive inference problem admits a ready solution. We take as a premise that the ratios of incremental distances fallen in successive units of time are 1 to 3 to 5 to

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[8] I thank Monica Solomon for drawing my attention to Huygens' work and for sending me a copy of his letter and other supporting materials.

[9] One way is to consider not the incremental distances, d(t), but the total distance, s(t), fallen by time t. Then it is easy to show that the invariance is satisfied by setting s(t) proportional to t^p for any p>0. However, showing that these are the *only* laws satisfying the invariance is harder.

7. There are two warranting facts accessible to Galileo: the rule governing the sequence is expressible simply; and the rule is invariant under a change of the unit of time. Only a small amount of arithmetic exploration will show that this invariance likely rules out all extensions other than the odd numbers. A fuller analysis shows that the second invariance by itself is sufficient to warrant the inference.

9. Can Bayes Help?

One might imagine that the general inductive problem of extending the sequence, 1, 3, 5, 7, is one at which Bayesian methods would excel. Might a Bayesian analysis somehow succeed without the need for specific background facts, contrary to everything that has been said so far? In short, the answer is that it does not provide a successful, universal treatment of the problem. There are two striking failures in the analysis:

- Bayesian analysis fails to offer any inductive learning from the evidence of the initial sequence 1, 3, 5, 7.
- Prior probabilities control the analysis, but the requirement that they normalize prevents them being set in a manner that is universally benign.

To proceed, we will see how a Bayesian analysis might help us decide between two extensions of the sequence 1, 3, 5, 7:

The odd numbers: H_{odd}, 1, 3, 5, 7, 9, 11, 13, 15, …

The odd primes with one: H_{prime*}, 1, 3, 5, 7, 11, 13, 17, …

using the evidence

E: 1, 3, 5, 7

The ratio form of Bayes' theorem asserts:

$$\frac{P(H_{odd} \mid E)}{P(H_{prime*} \mid E)} = \frac{P(E \mid H_{odd})}{P(E \mid H_{prime*})} \cdot \frac{P(H_{odd})}{P(H_{prime*})}$$

Since each of H_{odd} and H_{prime*} deductively entails E, we have $P(E| H_{odd}) = P(E| H_{prime*}) = 1$. Therefore Bayes' theorem reduces to:

$$\frac{P(H_{odd} \mid E)}{P(H_{prime*} \mid E)} = \frac{P(H_{odd})}{P(H_{prime*})}$$

What, according to the theorem, have we learned from the evidence E? The prior probabilities $P(H_{odd})$ and $P(H_{prime*})$ represent our initial uncertainty about the two hypotheses; the posterior probabilities, $P(H_{odd}|E)$ and $P(H_{prime*}|E)$ represent their new values, once we have incorporated evidence E. The reduced form of Bayes' theorem just tells us that conditionalizing on the evidence makes no difference to our comparative uncertainty concerning the two hypotheses. The ratio of the prior probabilities is the same as the ratio of posterior probabilities. This will be true for any pair of hypothesized sequences that start with 1, 3, 5, 7. In short, we have learned nothing new from the evidence as far as our decision between the two hypotheses are concerned.

Hypotheses logically incompatible with the evidence will be eliminated. Take, for example:

The natural numbers: H_{nat}, 1, 2, 3, 4, 5, 6, …

Since H_{nat} is logically incompatible with E, we have $P(E|H_{nat}) = 0$ and the posterior probability will be $P(H_{nat}|E) = 0$. However this result is not an inductive result. We have simply eliminated all hypotheses deductively incompatible with the evidence. That deductive result is easily gained without the probability calculus or any other inductive manipulations. Where we need help is with the inductive problem. Does the evidence E favor some hypotheses among those deductively compatible with it? Here the Bayesian analysis has failed to provide anything useful. Our inductive preferences are exactly the same before we learn the evidence as they are after we learn it.

This is a quite discouraging start. However, it will be instructive to press on and ask how our posterior probabilities may be with specific prior probabilities. The analysis bifurcates according to whether we are subjective or objective Bayesians. If we are subjective Bayesians, then our prior probabilities are merely expressions of prejudice, constrained only by compatibility with the axioms of the probability calculus. We might decide that those prejudices dictate that the H_{odd} has three times the probability of H_{prime*}. Then we conclude for our posterior probabilities that

$$P(H_{odd}|E) = 3\ P(H_{prime*}|E)$$

Looking at the equation, it may seem we have learned something. But we have not. The threefold difference in posterior probabilities is a direct restatement of our prior prejudices.

If we are objective Bayesians, we will seek prior probabilities that objectively reflect what we know. In this case, by supposition, we know nothing initially, so we have no reason to prefer one hypothetical sequence over any other. Hence the appropriate prior probability will assign the same, small probability ε to each hypothesis. That is, we have

$$P(H_{odd}) = P(H_{prime*}) = \varepsilon$$

The reduced form of Bayes' theorem now tells us:

$$P(H_{odd}|E) = P(H_{prime*}|E)$$

Once again, we have learned nothing. Our initial assumption was that all hypotheses are equally favored and that remains true for any pair compatible with the evidence.

This last conclusion overlooks a complication that will gravely trouble both subjective and objective Bayesians. The prior probability distribution must normalize; that is, the prior probabilities assigned to all the possible sequences must sum to unity. There is an uncountable infinity of possible sequences.[10] This means that, in a strong sense of most, most sequences must be assigned zero prior probability. Once a sequence has been assigned zero prior probability, its posterior probability on any evidence whatever will also be zero. That means that no evidence, no matter how favorable, will move us to entertain the sequence in the slightest. Hence both subjective and objective Bayesians must make unavoidably damaging decisions, prior to any evidence, as to which few sequences will be learnable.

Of course there are ways we might try to work around the problem. We might try to retain the uniform prior probability distribution simply by dropping the requirement of normalization and using so-called "improper priors." This violation may be excused if it turns out that, after conditionalization, the posterior probability distribution is normalizable. That normalizability is not achieved in this case however. There are infinitely many sequences

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[10] To see that the set is at least continuum sized, note that a subset of sequences using the digits 1 and 2 only can be mapped one-one onto the reals in the interval [0,1]. The sequence 1, 1, 2, 2, 1, 1, 2, 2, … is mapped to the fraction in binary notation 0.00110011…, etc. To see that the set is no bigger, note that we can map any sequence to a real in [0,1] by replacing the symbol "," by the symbol "0". The sequence 1, 3, 5, 7, 9 , 11, 13, … is mapped to the real .1030507090110130…, etc. The map is not "onto" because some reals, such as 0.100010001 have no corresponding sequence.

beginning with 1, 3, 5, 7. After we conditionalize on this evidence, we will be assigning equal non-zero probability to each in this infinity of sequences. Normalization will fail.

More drastically, we might retain a uniform prior probability distribution by the artifice of simply choosing a finite subset of sequences and casting the rest into the darkness of zero probability. If we eschew the uniformity of prior probabilities for variable probabilities, we can expand the set of sequences with non-zero prior probabilities to a countably infinite set. As long as the prior probabilities diminish fast enough as we proceed through the set, the sum of the probabilities can be unity, as normalization requires. One way of achieving this diminution is to assign these varying non-zero probabilities only to sequences that are arbitrarily long, but always of finite length. If we do this, we need some rule to decide which sequences are more and which less probable. A popular choice is to use a prior probability distribution advocated by Solomonoff (1964). Briefly describable sequences like 1, 2, 1, 2, 1, 2, … have greater prior probability than ones with no simple description. This is implemented by penalizing each sequence's probability by an exponential factor $(1/2)^N$, where N is the length of the shortest description possible for the sequence.[11] Bayesian analysis that employs this prior probability distribution is celebrated with joyous but naïve enthusiasm as providing a "complete theory of inductive inference" (Solomonoff, 1964, p. 7) or "universal induction" (Rathmanner and Hutter, 2011)

The difficulty is that the comparative judgments of this prior probability distribution will never go away. They determine how we might discriminate between H_{odd} and H_{prime*} on learning evidence E = 1,3, 5, 7. Thus the selection of this prior probability distribution is not benign. It must be justified by something solid. Are we to suppose that, as a quite general proposition, our world favors sequences with short Turing machine programs? This favoring might be credible in specific contexts, such as one in which we know that people are thinking up the sequences. But we are to suppose this favoring is true prior to any restriction whatever on where these sequences may appear. It is hard to see any reason for why the world, as a quite universal matter, would prefer to present us with number sequences that are computable and do so in way that exponentially penalizes sequences with longer programs. The literature supporting

[11] N is usually taken to be the length of the shortest Turing machine program that would output the sequence.

the Solomonoff approach believes otherwise and matches its joy in its solution of the inductive problem with equally joyous pronouncements grounding the approach. They often resort to appeals to simplicity through "Occam's Razor" (Solomonoff, 1964, p.7; Rathmanner and Hutter, 2011, p. 1101). This reveals an inflated reverence for the reach of the insights of a medieval scholastic who wrote six centuries before Turing conceived the notion of a universal Turing machine. For more deflation of simplicity, see Chapter 6 here.

In short, the challenge of accommodating the requirement of normalizability greatly complicates the analysis. More generally, the Bayesian analysis itself creates troubles that multiply and whose intractability deepens the more we try to resolve them. We could continue to wrestle with them. Or we could see that the very fact that we face lingering problems of this gravity is telling us that Bayesian analysis is just the wrong instrument for this inductive problem. Compare that with the simplicity of the material analysis of the problem of extending 1, 3, 5, 7. Once we locate the appropriate context, as in Galileo's law of fall, we find that the requirement of invariance under a change of the unit of time fixes the extension all but completely.

10. Warranting Facts

What might other warranting facts look like? Once we realize that familiar facts may serve also to warrant inference, we see that we are surrounded by such warranting facts.

Cosmology seeks to discover the structure of the universe on the largest scale. If the universe is infinite in spatial extent, then the finite portion observationally accessible to us is miniscule. What we see is infinitely outweighed by what we cannot see. The essential assumption that allows us to proceed from what we can see to what we cannot is the "cosmological principle." It asserts that the universe is roughly homogenous in its large-scale properties. While this wording is a little vague, standard applications of the principle employ it unambiguously. In our vicinity, matter is distributed roughly uniformly in galaxies in a space of constant, possibly zero, curvature. The cosmological principle authorizes us to infer that this condition obtains everywhere in the whole universe. Much of modern cosmological theory proceeds from that authorization.

Assume we have some isolated system with a given quantity of energy and entropy. The principle of the conservation of energy, the first law of thermodynamics, authorizes us to infer that, however else it changes, this same isolated system will have the same energy at any future time. The second law of thermodynamics authorizes us to infer a similar conclusion about the entropy of the system: it will be the same or greater. A careful statement of the second law allows merely that, with very high probability, the entropy of such systems will be the same or greater. Hence the conclusion is warranted inductively, but with very great certainty.

Assume we have some experiment performed in an isolated laboratory. The principle of relativity authorizes us to infer that a uniformly moving replica of the experiment will yield the same result. A more careful factual statement of the principle allows that it holds only in regions of spacetime that are remote from intense gravitational fields and thus unaffected by the curvature of spacetime revealed through the general theory of relativity. So the factual principle informs us that, *mostly*, the same experimental result will obtain. Thus the inference is inductive.

This series of examples is designed to implement a progression in two aspects. First we progress from the more general to the more specific and local. Second, we progress from examples in which the mediating facts authorize the conclusion deductively to those in which they authorize them inductively. The next and final example extends the progression quite far to a case of greatly narrowed scope and greater inductive risk.

Assume we set up some simple chemical process whose feed includes nitrogen gas. A general fact of chemistry is that nitrogen gas is quite unreactive. Its diatomic molecules are held together by a strong triple bond that is hard to break. This general fact authorizes us to infer, at some relatively high level of inductive certainty, that the simple chemical process will leave the nitrogen gas unaltered. We are not assured of the conclusion with deductive certainty. There are extreme conditions under which nitrogen gas can be compelled to enter into reactions. Finding them was the Nobel Prize winning work of Haber and Bosch a century ago. Their Haber-Bosch process enables the chemical industry to synthesize ammonia from nitrogen and thereby to manufacture both fertilizers and explosives.

This progression gives us factual principles of increasingly narrower scope that warrant inferences inductively. The material theory of induction places no lower limit on the size of the domain over which these factual principles operate.

11. A Non-Contextual, Formal Logic is Exceptional

The scope of successful applications of deductive logics that are non-contextual and formal is enormous. It is one of the great achievements of human thought. Its success makes it easy to think that the right way and the only way to analyze inference is with non-contextual, formal theories. Correspondingly, then, one might think of a materially warranted logic as some kind of failure, perhaps the result of insufficient efforts to find that elusive, universal formal logic of induction. I will argue in this section that the success of non-contextual, formal accounts of deductive logic is exceptional. Hence, we should not use our familiarity with deductive logic to set our expectations for inductive logic. We should not allow it to make us expect that there is a non-contextual, formal logic of induction.

11.1 The Undeserved Success

Which are the good deductive inferences? As long as the problems are kept simple, most people have a pretty good instinctive grasp of which are the deductive consequences of their knowledge and they manage without external guidance. However the limits are readily breached. If each thing has a cause, does it follow deductively that there is one ultimate cause for all things? If for every moment of time there is a later moment of time, does it follow that time endures infinitely? Novices relying on instinct can readily falter in the face of such traps. Can we find an instrument that systematically and reliably separates the good deductions from the bad? The means of discerning the good deductions is so familiar to anyone who has had contact with modern logic that it is easy to underestimate the difficulty of the problem.

This problem was all but solved millennia ago with a simple, profound observation. If you know that

"All electrons have spin half."

then you know that

"Some electrons have spin half."

That deductive inference is assured even if you have no idea of what an electron is and even less of an idea of what "spin half" is all about. You can make the inference merely by attending to the form of the sentences and ignoring the material. You start with "All As are B." and know that you are then authorized to infer to "Some As are B." You can ignore all the fussy stuff about electrons and spin. All you need to watch is the form of the sentences.

That deductive inference can proceed in such a simple and efficient manner is a marvel. It is the basis of a formal theory of inference, for we separate out the allowed inferences from the prohibited inferences merely by looking at their form. Specifying the logic then merely amounts to providing a list of schema, such as

All As are B.

Therefore, some As are B.

To use them, we replace A by anything we like and B by anything else we like and—bingo!—there's a valid deductive inference.

One sees in this example that the success of the schema depends upon the non-contextuality of deductive inference. We can transport this schema to any domain, substitute anything for A and B and still be assured that a valid inference results.

This simple schema is just the beginning. Generations of logicians have supplied us a growing repertoire of schema that embrace many logical operators. We have sentential logics that employ the connectives "not," "or" and "and." One of De Morgan's laws is the schema

not-(A and B)

Therefore, (not-A or not-B)

Predicate logics include individuals and their relational properties and they allow us to quantity over the individuals. If all things "x" gravitate "G(x)," then it is false that something exists that does not gravitate. This is an application of the schema:

For all x, G(x).

Therefore, not-(there exists x, not-G(x))

Modal logics introduce modal operators like "It is possible that…" and "It is necessary that…" Tense logics introduce temporal operators such as "It is always…" and "It is sometimes…"

11.2 Context Dependence of Connectives

In the face of these successes, it may seem that the scope of formal methods in logic in unlimited. However, lingering, recalcitrant anomalies limit the scope of the formal approach. These anomalies manifest in deductive logic when the logical terms used have meanings that are context dependent. Does "some" just mean "at least one"? Or does it mean "more than one but not too many"? The answer varies with the context. Consider the mathematical assertion:

For some x, the quotient 1/x is undefined.

Here "some" can mean "one or more" and the single case of x=0 is the one that makes the sentence true. However take the "some" of:

Some voters disapprove of the governor's decision.

This "some" refers to more than one voter, but probably not a majority. This difference matters to the formal theory, for not all the schema we may wish to use for "some" will apply everywhere. Consider

Some As are B.

Therefore, more than one A is B.

It applies to the "some" of the voters but not to the "some" of division by x. The schema is context dependent; it is not universally applicable.

The humble conditional "If A then B." has proven to be a more notorious locus of this sort of trouble. A natural understanding is that this conditional is true when knowing A authorizes you to know B as well. That is, the conditional can be a premise in the argument form "modus ponens":

If A then B.

A.

Therefore, B

That function is served by the "material conditional." According to it, "if A then B" is just the same as "Either A is false or B is true." Thus, if we happen to know that A is true, then we know the first option ("A is false.") fails. So that leaves the second, "B is true." Hence the material conditional has done the job of allowing us to proceed from knowing A to knowing B.

That may seem quite fine until one realizes that, under this understanding, the conditional "if A then B." comes out true whenever B is true, no matter what A says. That is, both

"If pig have wings, then the sky is blue."

"If the grass is green, then the sky is blue."

turn out to be a true, material conditionals, just because the sky is blue. The natural objection is that an "if A then B" statement can only be true if there is something in the antecedent A that makes the consequence B true. That fails in these last examples. Whether pigs have wings or the grass is green is irrelevant to the blueness of the sky. But

"If the sunset is red, then the sky is blue."

can be a true conditional. For the sunset is red because the blue light from a setting sun has been scattered away by the air; and that blue light comprises the blue sky. The blue of the sky is directly relevant to the red of the sunset.

Ingenious systems of relevance logic have sought to formalize the schemas into which "if…then…" properly enters, if understood relevantly. However deciding just what is relevant to what is a delicate issue that may embroil us in significant portions of science. The blueness of the sky results from Rayleigh scattering of blue light by the nitrogen and oxygen atoms of the air that just happen to be the right size for the job. So arcane facts in atomic theory are also relevant, but perhaps they are not as directly relevant as the redness of the sunset. That tells us that relevance is context dependent and may vary in strength. Indeed relevance may prove to be so diffuse that it may not be possible to separate off a small, tight formal logic of relevance as anything other than a crude gloss on a richer relation that is inextricably connected with the factual material of the science.

More generally, the success of universally applicable formal logics of deduction depends on deductive inference being non-contextual. Whenever simple connectives fail to have a non-contextual meaning, as in these examples, the logics in which they appear cease to be universal.

11.3 Sellars' and Brandom's Material Inference

The anomalies for a formal theory of deductive inference above focused narrowly on logical connectives (if…then…) and operators (Some…). They have a context dependent meaning, I argue, that is incompatible with their universal applicability, or least they cannot have it if we fix their meanings once and for all. Wilfrid Sellars and Robert Brandom have developed a broader and more powerful critique of a formal approach to inference in general, not just deductive inference.

Their concerns are not limited to connectives but to all the terms appearing in the inferences. Their core idea is that the meaning of the terms in propositions in what makes good the inferences in which they correctly appear. Brandom (2000, p.52) displays the inference from:

"Pittsburgh is to the west of Princeton"

to

"Princeton is to the east of Pittsburgh."

We recognize this as a good inference, but not for formal reasons. Rather it is good because of the contents of the concepts of east and west. That is, the matter of the inference makes it good.

When I developed the material theory of induction, I was not aware of Sellars' and Brandom's notion of material inference and, in particular, Brandom's use of the term "material inference." I learned of it through a lovely note written by Ingo Brigandt (2010), which usefully develops and applies the notion of material inference.

The difficulty is that our notions of material inference differ slightly, as far as I can see. That means that it would have been better at the outset if I had chosen another name. For Brandom, the above inference is material since it is made good by the concepts invoked in the premises. In my view, it is material since I locate the warrant for the inference in the background material fact: if something is east of something else, then the second is west of the first. Here I leave open whether this difference is consequential or merely a different entry point into a collection of views that largely agree.

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