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The Material Theory of Induction, Briefly

John D. Norton
http://www.pitt.edu/~jdnorton

Chapter for a book provisionally titled
The Large Scale Structure of Inductive Inference

1. Introduction

This volume describes how relations of inductive support are structured on the large scale. It does so in the context of a particular view of inductive inference, the material theory of induction. This account of inductive inference has been elaborated extensively in my earlier *Material Theory of Induction* (ms) to which the reader is referred. This chapter offers only a brief introduction to the material theory. It is a preliminary. The main claims of this volume are presented in the next chapter.

Section 2 below gives a motivation, summary and argument for the material theory of induction. The standard approach to inductive inference characterizes inductive inferences or relations of inductive support formally, by means of schemas or calculi that are purported to hold universally. They all fail to apply universally, or so I argue. For facts peculiar to the each domain determine which are the good inductive inferences or proper relations of inductive support. There is no way to combine these disparate warranting facts into a single, universally applicable system. This is the central claim of the material theory of induction.

The remainder of the chapter illustrates how standard, formal approaches to inductive inference fail; and that a material approach can capture what made the formal approach seem viable without succumbing to the formal approaches' difficulties. Since there are so many approaches to inductive inference, this chapter can discuss only a few of them. They are sampled from a survey of accounts of inductive inference in Norton (2005).

This survey divides accounts into three families. The first, “inductive generalization,” is based on the principle that we may infer from an instance to a generalization. It includes enumerative induction, discussed in Section 3, and analogical reasoning, discussed in Section 4. The second family, introduced in Section 5, is “hypothetical induction.” It is based on the principle that the capacity of an hypothesis to entail the evidence is a mark of its truth. Section 6 reviews one example in which we are to accept the hypotheses that most simply entails the evidence. The third family has accounts in which a calculus governs strengths of inductive support. The probability calculus is overwhelmingly the most popular candidate. Section 7 uses the example of Laplace’s rule of succession to sketch some limits of the account and shows how the material approach can escape them.

2. The Material Theory of Induction

2.1 Inductive Inference

Induction and inductive inference are understood here in their broadest senses. They apply to any inference that leads to a conclusion deductively stronger than the premises from which it proceeds. This conception automatically includes traditional forms of ampliative inference, such as enumerative induction. (“This *A* is *B*. Therefore all *A*s are *B*.”) Ampliation is understood in its broadest sense as referring to any expansion of the conclusion beyond the deductive consequences of the premises. The terms “induction” and “inductive inference” will also be taken to encompass what is often called confirmation theory. It applies to accounts in which one does not proceed in the traditional manner of an inference to infer the truth of some conclusion, detached from the premises from which it was derived. Rather one merely reports a relation of inductive support of such and such a strength between two propositions. The most familiar application is probabilistic analysis. The measure $P(A|B)$ is the strength of support proposition *A* accrues from proposition *B*.

The account here is restricted to the logical notion of inference. According to it, the relation of inductive support obtains between *A* and *B*, independently of human desires, beliefs and thoughts. It is not the “psychologized” notion of inference. In the latter, reporting an inference from *A* to *B* is merely reporting a fact of our psychology. If we hold *A* true then we will assert *B* as well. Discussions of people inferring from *A* to *B* will appear in the text that follows, especially in the historical narratives. However they will be treated throughout as attempts by the figures in question to conform with the appropriate standards of inductive inference.

2.2 An Unmet Challenge

Any account of inductive inference must do two things. First, it must provide a means of distinguishing good inductive inferences from bad ones. Second, it must provide a demonstration that the division is made properly. That is, the inferences it designates as good must be good.

My contention is that all principal accounts of inductive inference so far have failed to meet these challenges. Their failure derives from a pervasive presupposition: they assume that an account of inductive inference must be based on formal rules that can be applied everywhere. In this they copy a standard approach in deductive inference. Here is a deductive argument schema:

All *A*s are *B*.

Therefore, some *A*s are *B*.

The schema is universally applicable since we can substitute any noun for *A* and any adjective for *B* and end up with a valid inference. The simplest account of inductive inference mimics this approach. Enumerative induction just inverts the order of the sentences in the schema:

Some *A*s are *B*.

Therefore, all *A*s are *B*.

The account is universal in the sense that this schema can be applied everywhere. It is formal in the sense that the schema specifies the form only of valid inferences. It does not constrain the matter in the sense that any nouns and adjectives can be substituted for *A* and *B*. Probabilistic treatments of inductive support are similarly formal and universal. Sentences derived within the probability calculus play the role of universal schema. Consider for example the sentence

$$P(\text{not-}A|E) = 1 - P(A|E)$$

It will remain a theorem in the calculus no matter which propositions are substituted for *A* and *E*. These two examples reflect the standard practice in the literature. It is to seek schemas that are universal and formal.

The difficulty is that all these schemas eventually fail somewhere; and, as I shall argue below, the failure is inevitable. The failure of enumerative induction is widely known. Indeed the schema almost *never* works. One has to choose substitutions for *A* and *B* very carefully if one is to recover any acceptable inductive inference at all. There are similar problems with the sentence in probability theory, although they require more analysis to be fully developed. The sentence is unproblematic if the "*P*" represents a physical chance. If the chance of outcome *A* happening given background *E* is small, say $P(A|E) = 0.01$, then the chance of outcome *A* not happening is large:

$$P(\text{not-}A|E) = 1 - P(A|E) = 1 - 0.01 = 0.99$$

But now let "*P*" measure the inductive strength of support for the proposition *A* from the evidence *E*, where *E* is the totality of all evidence available. This last relation precludes the total evidence *E* from being neutral in its inductive support of *A*. That would mean that it supplies no support for either *A* or its negation not-*A*. We would want that lack of support to be represented by a small or even zero magnitude for *both A* and not-*A*. However, if we set $P(A|E)$ to some number close to zero or to zero itself, then the statement in the probability calculus forces us to set $P(\text{not-}A|E)$ close to one or to one itself.[1]

2.3 The Material Solution in Three Slogans

The material theory of induction addresses these problems at their root: they derive from the false presumption that good inductive inferences or relations of support can be identified by a single set of rules or formal schemas that are applicable universally. That is:

There are no universal rules of inductive inference.

Instead, the core claim is:

All inductive inferences are warranted by facts.

That is, what distinguishes a good inductive inference is not its conformity with some general schema, but with background facts of the pertinent domain.

The idea that an inference can be warranted by a fact is familiar from deductive inference. The factual proposition "If *A* then *B*." is both a mundane fact but also a warrant for a deductive inference from *A* to *B*. The warrant derives fully from the meaning of the hypothetical, "if . . . then . . ." To assert "If *A* then *B*." is also to assert that we can infer from the truth of the antecedent *A* to that of the consequent *B*. In the case of the material theory of induction, a corresponding background fact might be "Generally, *A*." Such a proposition authorizes us to

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[1] Experts will recognize that this consideration is the starting point of a decades-long debate over the representation of the neutrality of support. My view is that it cannot be done satisfactorily using probabilities. See Norton (2008, 2010).

conclude *A*. The import of the "Generally" is that the inference is inductive. It conveys that there is a small possibility that the conclusion *A* may fail to be true.

Finally, there are no background warranting facts with universal scope. The warranting facts of each domain will, in general, warrant inductive inferences that are peculiar to that domain. This is expressed in the third slogan

All inductive inference is local.

There may be similarities in the inductive inferences from different domains. However these similarities will prove to be superficial. We must always seek the warrant for an inductive inference within the background facts of its domain.

To continue with the oversimplified example of "Generally, *A*." it may seem that this fact might somehow be applied across all domains. However the meaning of "generally" will vary from domain to domain, so that any similarity is superficial. In a probabilistic domain, we would assert "Generally, ten successive coin tosses will not all be heads." The "generally" encodes an objective probability of the possibility of failure such that we expect failure on average at a rate of $1/2^{10} = 1/1024$ in many cases of ten successive coin tosses. In particle physics we may assert "Generally, the laws of particle interactions are time reversible." In chemistry, we may assert "Generally, metallic elements are solids at room temperatures." In these last two cases, we have no possibility of repetition. The laws of particle interactions of the standard model of particle physics are fixed, as is the set of metallic elements. Setting aside dubious contrivances, the "generally" does not lead to a meaningful notion of an expected rate of failure. Once we have scoured the periodic table for metallic elements, there is no other periodic table with different elements where we can repeat the search anew.

What is left open is the extent of the domains in which each specific sort of inductive inference is warranted. A narrowly specific warranting fact may only warrant a few inductive inferences in some narrow domain. A broader warranting fact may warrant a mathematical calculus, which would be applicable across a large range of cases, but still in some limited domain.

In sum, the two challenges for inductive inferences are met as follows. In any domain, the licit inductive inferences are those warranted by the facts of the domain. That they are properly warranted follows from the truth of those facts and is recovered from the meaning of the terms expressing the warranting facts.

2.4 The Background Facts Decide, Not Our Beliefs About Them

Inductive warrants work in the same way as the formal schema of deductive inference. They pick out which are the licit inductive inferences or relations of inductive support, independently of our beliefs. If we reason deductively in accord with the schema modus ponens, we reason validly, even if we know nothing of deductive logic and its schemas. If we reason in accord with the fallacy of affirming the consequent, we commit a deductive fallacy, even we mistakenly believe that affirming the consequent is a licit deductive schema.

Correspondingly, we infer well inductively if our inference is warranted by a fact of the domain, independently of whether we know it. We infer poorly inductively if there is no fact of the domain that warrants the inference, even if we believe erroneously that there is such a fact.

In practice, conceived materially, our inductive inferences are guided by our best judgments of which are the prevailing facts in any domain. They are defeasible. Those judgments may prove incorrect and we be inferring poorly. If we differ in our judgments and arrive at incompatible inductive inferences, at most one of us is correct. Which of us inferred well is decided by which truly are the facts of the domain.

2.5 The Case for the Material Theory

There are two components of the material theory to be established: first, that facts provide the warrant for inductive inferences; and second, that each domain has its own set of warranting facts ("locality").

First, that facts warrant inductive inferences follows from the inevitable failure of accounts of inductive inference that aspire to apply universally. They must fail because of the defining feature of inductive inferences: they are ampliative. That is they authorize us to more than can be deduced from the premises. Thus there will always be domains, inhospitable to each schema, in which the schema will fail systematically. Characterized most generally, the factual warrant for each inductive inference amounts to the factual contingency that the inference is conducted within a domain hospitable to it.

Here standard connective-based deductive inferences differ. They are not prone to this mode of failure. Their warrant lies fully within the premises in the meaning of the connectives. It is present whatever the domain of the inference.

Domains inhospitable to each formal account can arise in many ways. Philosophy's fabled deceiving demon is a simple if contrived way to see that inhospitable domains are unavoidable in principle. The demon secretly intervenes to frustrate our inferences. The applicability of each account depends on a factual matter: that we are not in the grip of such a demon. While deceiving demons are fantasies, something close to them is not. Experimentalists must assume that their lab assistants are not disgruntled employees maliciously selecting and suppressing data such as to deceive them into false conclusions. Or they must assume that they are not in the grip of a mechanical equivalent: a loose connection in their cabling that introduces enough noise in the results to obscure a regularity or create a spurious one.[2]

These are contrived examples, but with the mitigating virtue that they can be expressed tersely. They display the key point. Any account of inductive inference can only succeed if the conditions in the domain are hospitable. That they are so is a factual matter.

Second, the locality of inductive inference follows from there being no universally applicable warranting fact. An old hope, now long abandoned, was that the regularities of the world might be simple enough that they could be expressed in some sort of universal fact that would then underwrite all inductive inference. This was Mill's principle of the uniformity of nature (Mill, 1904, Bk III, Ch. III, p. 223):

> 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.

In the abstract, this principle has momentary appeal. However Mill himself had already identified the difficulty that proved fatal. For the principle to be something more than idle posturing, we have to find the universal description that picks out when we can advance from one case to all. Finding this description has proven to be an intractable problem. Any description that is precise enough to be applied is rife with counter-examples. A description that is immune to counterexamples can only do so by adopting vagueness to the point of vacuity.[3]

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[2] In September 2011, the OPERA collaborative reported faster-than-light-neutrinos. As Reich (2012) reported, they were misled in part by a loose cable connection.

[3] For more of this critique, see Salmon (1953).

2.6 An Illustration

An example illustrates this general argument. Consider the deductive inference:

Winters past have been snowy AND winters future will be snowy.

Therefore, winters past have been snowy.

The warrant for this deductive inference resides entirely within the premises. It lies fully within the meaning of the connective "and." It can only be used when the truth of the conjunction derives from the truth of each of the conjuncts individually. Hence we are warranted to infer to each of them individually. Since the entire burden is carried by the connective "and," we can write a fully general schema for deductive inference that can be applied in any domain:

A and *B*.

Therefore, *A*.

Now consider a related inductive inference:

Winters past have been snowy.

Therefore, winters past have been snowy AND winters future will be snowy.

The conclusion amplifies the premise. Thus there will be domains hospitable to the inference; and there will be inhospitable domains in which it fails. An inhospitable domain is one in which there is considerable climate change, including significant warming. A hospitable domain is one in which climate is unchanging. That ours is one of these hospitable domains is the fact that warrants the inference.

More generally, this fact licenses a schema for inductive inference that is restricted to a specific domain:

In domains with unchanging climates,

If climatic fact *A* has always held in the past,

Climatic fact *A* will continue to hold.

We can substitute with facts applicable to domains with unchanging climates to recover a licit inductive inference:

In domains with unchanging climates,

If summers past have always been hot and dry,

Then summers past and future will be hot and dry.

This example also illustrates the inherently inductive character of the inference. We can make the warranting fact explicit and even add it to the premises displayed. However we have not thereby converted the argument into a deductive argument. For the climate can be unchanging and authorize us to expect a continuation of snowy winters. However a rare, anomalously warm winter without snow is still compatible with an unchanging climate, since climatic conditions pertain to long-term regularities.

The following sections illustrate at greater length the failure of universal applicability of some formal accounts of inductive inference. We shall also see how identifying the warranting material facts in some domain helps us delimit the domains of applicability of each inductive inference.

3. Enumerative Induction

Enumerative inductions—the familiar inferences from "some… to all…"—are pervasive in science. Just as pervasive in the philosophy literature is a denunciation of the argument form. Francis Bacon's (1620, First Book, §105) riposte is just the best known of many from antiquity to later times:

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

This poses a puzzle. How is it these "some-all" inferences are used pervasively in science yet denounced pervasively by philosophers?

The puzzle is readily solved if the some-all inferences are approached materially. The whole problem derives from the mistaken assumption that all these some-all inferences are warranted by a single formal schema. For there is no formal schema that can serve to warrant them all. Efforts to formulate one that works universally collapse. It is that difficulty to which the philosophical literature responds. Rather, in so far as the some-all inference is warranted at all, that warrant derives from facts peculiar to the domain in which each some-all inference is executed. The unity of form of the many some-all inferences in science is superficial. It is not reflected in a unity of the warrants for the inferences.

3.1 Curie's Enumerative Induction

This material solution to the puzzle is illustrated in an enumerative induction of striking scope in Marie Curie's doctoral dissertation, presented to the Faculté des Sciences de Paris in

June 1903.[4] There she reported on years of work with her husband, Pierre Curie. It included the laborious separation of tiny quantities of radium chloride from several tons of uranium ore residue. Mentioned only briefly were the crystalline properties of radium chloride (p. 26): "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." This remark on the crystallographic properties of radium chloride became standard in the new literature that quickly sprang up around the excitement generated by Curie's discovery of radium.

Since the remark is unlimited in scope, it results from an enumerative induction. Indeed it is one of rather extraordinary scope. Curie had initially prepared just a few tenths of a gram of radium chloride. Subsequent preparations would not have produced large quantities. Yet a general statement on the crystallographic properties of radium chloride was widely accepted without hesitation. Rutherford surveyed what was known of radioactive substances in 1913 and noted (1913, p. 470) without qualification that: "Radium salts crystallise in exactly the same form as the corresponding salts of barium."

3.2 Failure of Formal Analysis

What can support an induction of such strength from these very few samples of radium chloride? We can see quite quickly that the universal schema proposed for enumerative induction above falls far short of what is needed:

Some *A*s are *B*.

Therefore, all *A*s are *B*.

There are simply too many substitutions possible for *A* and *B* that lead to failed inductions:

Some samples of radium chloride were prepared by Marie Curie.

Some samples of radium chloride are in Paris.

Some samples of radium chloride are at 25^{o}C

Some samples of radium chloride are less than 0.5g.

Some radioactive substances crystallize like barium chloride.

Some substances in Curie's laboratory crystallize like barium chloride.

None of these lead to credible inferences. One might be tempted to propose restrictions on what can be substituted for *A* and *B*. Might we insist that no nouns or adjectives with essentially

[4] For further details on this example, see Norton (ms, Ch.1).

spatiotemporal character can be substituted? That would block the substitution “substances in Curie’s lab” for *A* and “in Paris” for *B*. However it would also block what otherwise would be quite credible enumerative inductions.

All known kangaroos are indigenous to Australia.

Therefore, all kangaroos are indigenous to Australia.

And

All known moons and planets in our solar system orbit in the same direction as Earth.

Therefore all moons and planets in our solar system orbit in the same direction as Earth.

The pattern here is evident. For each restriction we might contemplate on substitutions for *A* and *B*, it takes only a little imagination to find otherwise credible inferences that are blocked and arbitrarily so. We must abandon hope for an embellished version of the schema that can serve universally.

3.3 Material Analysis

This failure should not make us pessimistic over the prospects of inductive inferences like Curie’s. It is a vanity of inductive logicians to imagine that Curie and Rutherford relied on the pronouncements of logicians in forming their inferences. Rather Curie and Rutherford knew precisely which crystallographic properties of radium chloride could enter into some-all inferences through a century of research in mineralogy on crystals.

Crystals grow in such a bewildering array of shapes that it was initially hard to see that any regularities could be found. If some crystalline sample of a mineral adopted a particular shape, it would be extraordinary to find another sample with exactly that shape. The problem is reminiscent of the old saw that no two snowflakes are alike. The problems are similar. What regularities can be found among snowflakes when they all differ?

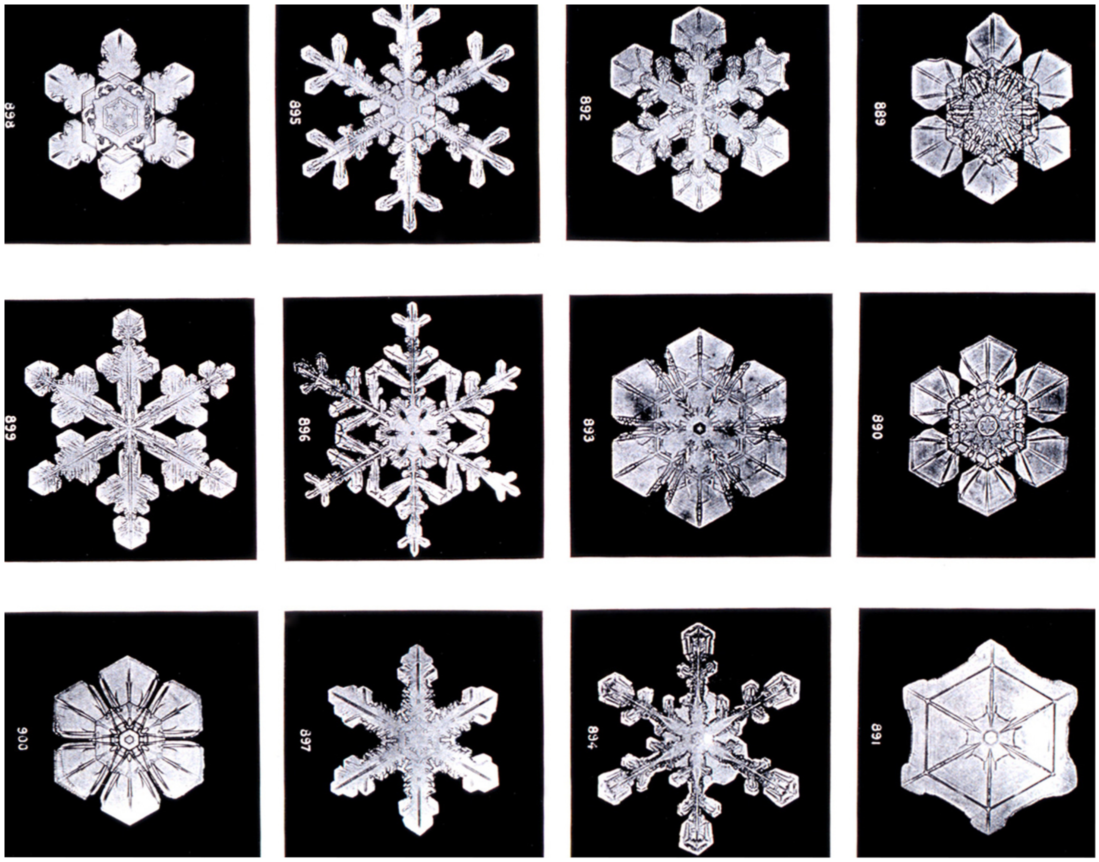

Figure 1. Snowflakes[5]

The answer is widely know and easily seen in Figure 1. Snowflakes all reflect the same regular hexagonal shape. More abstractly, they exhibit a discrete rotational symmetry. The shapes map back into themselves if we rotate them by 60^{o}.

Essentially this is the regularity that was discovered during the 19th century investigation of crystalline forms, but promoted from the two dimensional forms of snowflakes to the three dimensional forms of most other crystals. Snowflakes are built around one shape, the regular hexagon. The more general three-dimensional theory, however, calls for six[6] crystallographic systems, each with its own fundamental form and symmetries. The most familiar system is the

[5] Image source: https://commons.wikimedia.org/wiki/File:SnowflakesWilsonBentley.jpg which gives a provenance: Wilson Bentley, "Studies among the Snow Crystals ... " Plate XIX, "The Snowflake Man." From Annual Summary of the "Monthly Weather Review" for 1902.

[6] So was the count in Curie's time as provided by Miers (1902, p. 38).

“cubic” system to which sodium chloride, common table salt, belongs. This membership does not mean that all the crystals of common table salt are just little cubes. Rather it means that they are all derived by geometric operations from the basic cubical form, just as all snowflakes derive from the regular hexagon.

By Curie’s time, it was a standard result that each crystalline substance generally belongs to a unique crystallographic system. The complication that underwrites the “generally” is that some crystalline substances manifest dimorphism or polymorphism. Through it they may crystallize under different conditions into two (“di-“) or more (“poly-“) systems. This regular association of crystalline substances with one of the six systems is the material fact that warranted Curie’s inference. If she can identify the crystallographic system to which one sample of radium chloride belongs, then she can infer to the general crystallographic system of all samples of radium chloride. Because of its importance in this example, in Norton (ms, Ch.1), I distinguished this warranting fact as a principle named after René Juste Haüy, who is acknowledged as an early 19th century founder of crystallography:

> (Weakened Haüy’s Principle) *Generally*, each crystalline substance has a single characteristic crystallographic form.

The “generally” that weakens the principle ensures that Curie’s inference is inductive. She takes the inductive risk of assuming that there is no polymorphism for radium chloride.

Curie does not mention by name the monoclinic system to which radium chloride belongs. Rather she avails herself of an indirect locution: radium chloride crystallizes as does barium chloride. That is, the system to which radium chloride belongs is just the same as that to which barium chloride belongs. That they should belong to the same system is quite plausible since the two salts are very similar in their chemical properties; and such similarities often manifest in crystallographic similarities.

What initially appeared as a simple enumerative induction by Curie can now be seen to be something a great deal richer. The specific generalization Curie makes on the crystalline form of radium chloride is informed by and warranted by facts uncovered in a century of research in mineralogy. That research solved the difficult and delicate problem of just which properties of crystals can be generalized in a some-all inference. The warranting fact of the Weakened Haüy’s Principle rested in turn on a considerable amount of science. It exploited the atomic theory of matter in picturing crystals as atoms arranged in regular lattices; and the mathematics of group theory in discerning how spatial symmetries led specifically to the different crystallographic families. Curie's inference was not grounded in any abstract logical schema, but in a considerable range of scientific facts.

4. Analogy

Reasoning by analogy, like enumerative induction, is a long recognized form of inductive generalization. It too is recounted by Aristotle. It asserts in its simplest form that, when some system with property *P* also has property *Q*, this particular fact can be taken as an instance of the generalization that other systems with a similar property *P* will also have a similar property *Q*. We shall see that the difficulties analogical reasoning faces are quite similar to those faced by enumerative induction. Simple schemas for analogical reasoning are not serviceable. A bare schema is too permissive in part through its simplicity and in part through the vagueness of essential terms like "similar." The obvious repair is to strengthen the schema by careful elaborations, tuned to canonical examples of analogical inference. The results, however, are schemas of increasing complexity that turn out still to be prone to the same troubles. That this should happen is predicted by the material approach. According to it, the best we can have are different schemas that succeed only in different, factually delimited domains. There is no way to synthesize them into a single coherent schema that applies universally.[7]

A curiosity of analogy is a systematic difference in the way philosophers approach analogy and the way scientists do. Philosophers treat analogy as a form of inductive inference. Their task is to find the general rules governing it. Scientists treat analogies as facts that can guide inferences. For them the fact of analogy is itself an empirical matter subject to normal scientific investigation. If one thinks formally about inductive inference, this difference is hard to accommodate. It makes perfect sense, however, if one approaches inductive inference materially. For the scientists' facts of analogy are the material facts that warrant analogical inference. These facts are just ordinary facts of science, themselves subject to inductive analysis.

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[7] Here I discount the trivializing device of simply taking a huge, likely infinite disjunction of all the distinct locally applicable schemas and offering it as a single schema.

4.1 The Bare Formal Schema

In his logical treatise, Joyce (1936, p. 260) gives a standard schema for analogical inference in its bare form:

S_1 is P.

S_2 resembles S_1 in being M.

[therefore] S_2 is P.

This schema fits many inferences in science. In the eighteenth century, it was noted that electricity resembled gravity in manifesting as a force between bodies that diminishes with distance. The analogy supported the conclusion that electrical forces like gravitational forces diminish with the inverse square of distance. This conclusion was experimentally affirmed by Coulomb.

As with the simple schema for enumerative induction, this bare analogical schema only returns good results when one makes careful substitutions. With little effort one finds many examples of failed analogical inference. Heat flows like a conserved fluid from hot to cold, but contrary to the eighteenth century supposition of the caloric fluid, it is not conserved and is not a fluid substance. Perhaps the most famous analogical failure concerns whales. They resemble fish in swimming in the oceans. However, since they are mammals they neither breathe with gills nor lay eggs as do fish.

As with enumerative induction, there is a long-standing tradition of deprecation of analogical inference, complete with sage warnings of the dangers of false analogies. Here is one example (Thouless, 1953, Ch. 12):

> Even the most successful analogies in the history of science break down at some point. Analogies are a valuable guide as to what facts we may expect, but are never final evidence as to what we shall discover. A guide whose reliability is certain to give out at some point must obviously be accepted with caution. We can never feel certain of a conclusion which rests only on analogy, and we must always look for more direct proof. Also we must examine all our methods of thought carefully, because thinking by analogy is much more extensive than many of us are inclined to suppose.

4.2 The Two-Dimensional Model

If one thinks formally about analogical inference, the remedy is to embellish the bare schema in a way that will exclude the plethora of troublesome counterexamples. The dominant approach in the literature develops a two-dimensional account, so named by me because it lends itself to display in a two-dimensional array. It draws on Keynes' (1921, Ch. XIX) notion of "positive analogy" and "negative analogy" and has been developed by Hesse (1966). The account uses these notions to support inferences about a target system through its analogical relations with a suitable source system. It can be represented in a general tabular schema, provided by Bartha (2010, p.15):

Source	Target	
P	P^*	(positive analogy)
A	$\sim A^*$	(negative analogy)
$\sim B$	B^*	
Q		
	Q^* (plausibly)	

The goal is to infer to some as yet unaffirmed property Q^* of the target that corresponds with some property Q of the source. Whether we can do this is decided by the relative strengths of the positive and negative analogies. The positive analogy lies in properties P and P^* of source and target agreeing. The negative analogy lies in the source exhibiting property A but the target lacking the analogous property A^*; and conversely with properties B and B^*.

Properties P and Q of the source stand in some relation, which may be causal, explanatory or something else. If the strength of the positive analogy outweighs the strength of the negative analogy, then that relation can be carried over to the analogous properties P^* and Q^* of the target. We can then affirm that the target system does indeed carry the property Q^*.

While the bare schema has been considerably enriched, this tabular schema still falls well short of what is needed in a formal account that can mechanically separate the good from the bad analogical reasoning, the true from the false analogy. Rather it still relies throughout on users of schema just knowing intuitively when certain relations obtain. They are not given formal specifications that can be applied unambiguously. In the case of the relations laid out vertically in the table, just what is it to be a causal or explanatory relation between *P* and *Q*? And which other relations are admissible? The horizontal relations between *P* and *P**, between *A* and *A**, and so on, are relations of similarity. In formal terms, when are two properties similar? Finally and most troublesome, how are we to assess the relative strengths of the positive and negative analogies? For that balance decides whether we have a true or false analogy overall. These incompletenesses leave sufficient room for us to continue concocting dubious inferences that nonetheless conform with the explicit conditions of the schema.

Joyce's bare schema for analogical reasoning contained just one term—"resembles"—in need of external, formal specification. In an effort to resolve the bare schema's problems, the two-dimensional account has introduced many more terms and notions. They are each in turn in need of further formal specification. One might, as did Bartha, take this as a challenge to be resolved by still further elaboration. In this vein, Bartha's (2010, Ch.4) "articulation model" adds considerably more structure to the two-dimensional model. The pattern already established continues. Each elaboration brings new conceptions with it; and each such conception requires in turn a further formal specification.

There is considerably more detail in both Hesse's two-dimensional and Bartha's articulation model than can be presented here. Norton (ms, Ch. 4) is my best effort to provide a richer account of both. However the overall trend is quite evident. Each effort to conform the schema better to good and bad cases requires elaborations that employ new conceptions and artifices that are in turn in need of formal specification. Each effort to repair an inadequate schema does not solve the problems but multiplies them.

4.3 The Material Approach to Analogy

It is inevitable, according to the material approach, that attempts to find a serviceable schema for analogical reasoning will degenerate into a multiplicity that is ever growing but always incomplete. For no single schema can embrace all the cases. A material approach has no trouble, however, accommodating inferences that are generally identified as analogical reasoning. These inductive inferences, as are all inductive inferences, are warranted by facts peculiar to the domain of the target system. What gives the facts an analogical character is that they are expressed by pointing out similarities to other systems. So we might call them the "fact of analogy." Nonetheless, whatever role similarity to a source plays in expressing the fact, it is crucially a fact that pertains to the target system. It is only in virtue of this that the fact can warrant the inductive inference.

Failed analogical inferences arise when there is no suitable fact of analogy. Relativity theory showed us that we should abandon the absoluteness of motion in favor of the relativity of inertial motion. By analogy, should we abandon the absoluteness of truth and of moral rectitude in favor of their relativity? The analogical argument fails since there is no fact of analogy connecting motion with truth and moral rectitude. The analogical inference attempted depends on a verbal coincidence in the repeated presence of the word "absolute."

These facts of analogy may appear in the formal analysis. There, the identification of the fact of analogy can only be an intermediary. The task that remains is to show how the fact somehow conforms with a schema in which the warrant for the inductive inference ultimately resides. The material approach simply says that we have found all the warrant that can be had in the fact of analogy. The search for a warrant should stop there. An example below illustrates the role of a fact of analogy.

4.4 The Mountains on the Moon

Galileo's (1610) *Siderius Nuncius*—the *Starry Messenger*—reports an extraordinary finding among Galileo's telescopic investigations of the heavens: there are mountains and seas on the moon. The mountains manifest when one tracks how the division between light and dark on the moon grows in a waxing moon. As the bright edge advances, bright points of light appear ahead of it, grow and merge with the advancing edge. This is just how mountains on the earth are illuminated by a rising run. Similar observations and analogies support the presence of depressions or "seas" on the moon.

Galileo's analysis draws on an analogy between the moon and the earth. His inference fits the bare schema of analogical inference:

The earth (S_1) has mountains and seas (P).

The moon (S_1) resembles the earth (S_2) in both showing the same patterns of surface illumination (M).

Therefore, the moon (S_2) has mountains and seas (P).

The inadequacy of the schema as a warrant is easy to see. Nothing in the schema prevents us replacing

P = "has mountains and seas."

with

P = "has mountains with alpine ski resorts and water-filled seas with submarines."

It is hard to imagine anyone endorsing the resulting inference to ski resorts and submarines on the moon. The obvious objection is that the presence of ski resorts on earthly mountains plays no role in the formation of patterns of light and dark on the earth. The analogical inference succeeds only in so far there is the right sort of connection between the "*M*" and the "*P*" of the schema. With that remark, we have introduced the fact of analogy that warrants the inference:

> The process that produces the patterns of light and dark on the moon is the same as the process that produces them on the earth.

The similarity to the process on earth is inessential to the fact's power to warrant the inference. What matters is that:

> The patterns of light and dark on the moon are produced as shadows in rectilinearly propagating light by opaque bodies.

For that is how the patterns on the earth are produced. In principle, Galileo could proceed entirely using this reduced form of the fact of analogy. He could demonstrate by some simple geometric constructions that lunar mountains would illuminate in just the patterns he observed. The earth need never be mentioned. However there is a shortcut. Galileo does not need to develop these constructions afresh for his readers. They are already familiar to earthbound observers who have experienced a sunrise. It is a rapid expository convenience to recall that experience.

This development oversimplifies Galileo's analysis in that this last warranting fact in conjunction with his observations enables a deductive inference to the presence of mountains on the moon. The inductive character of Galileo's investigation resides in an uncertainty over whether this warranting fact is true. We restore the inductive character of the analysis by inserting the word "likely" into the fact so it merely asserts "… are likely produced…" This reflects Galileo's efforts to show that other possible accounts of the origin of the patterns of light and dark are unlikely. For further discussion, see Norton (ms, Ch.4, Section 8).

5. Hypothetical Induction

5.1 Saving the Appearances

Enumerative induction and analogical reasoning are forms of inductive generalization: we infer from an instance to the generalization. The weakness of this form of inductive inference is that the generalizations are most naturally expressed in the same vocabulary as are the instances. That makes it difficult to infer from evidence to hypotheses formulated with a quite different vocabulary.[8]

Another form of inductive inference that I have called "hypothetical induction" is quite free from this limitation. According to it, the fact that some hypothesis with suitable adjuncts entails true evidence is a mark of the truth of the hypothesis itself. This form of inductive inference has long been used science. In ancient Greek Astronomy, "saving of the appearances" meant having hypotheses about the motion of celestial bodies whose observable consequences match and correctly predict what is seen in celestial motions. The Copernican planetary system used the astonishing hypothesis of the motion of the earth to save the appearances of the motion of the planets. This, according to the Copernicans, indicates its truth. Critics of this conclusion, such as Osiander writing in a preface to Copernicus' work, urge that it merely shows the pragmatic utility of the hypothesis, but not its truth.

As scientific theories grew more remote from the evidence that supports them, the need for something stronger than mere inductive generalization grew. It was inescapable by the time of Einstein's general theory of relativity. The planetary motions that provide evidence for the theory are expressed in the vocabulary of observational astronomers. It is quite remote from the vocabulary used to express the core statements of Einstein's theory: metrical and stress-energy tensors, Christoffel symbols and Riemann's four index symbols (now the curvature tensor). In November 1915, a jubilant Einstein reported the success of his theory with the long-standing astronomical anomaly in the perihelion motion of mercury. That anomalous motion could be deduced within his theory. It was, to use Einstein's word of 1915, "explained."[9] There was no

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[8] It is difficult, but not impossible, as a survey (Norton, 2005) shows.

[9] The title of Einstein's (1915) paper translates to "Explanation of the Perihelion Motion of Mercury by the General Theory of Relativity."

generalization from an instance. Einstein's new theory saved the appearances and that was enough to make it one of the revered evidential coups of the twentieth century.

5.2 Its Limitations

The strength of hypothetical induction is that it can lead to confirmation of hypotheses remote from the evidence. That is also its weakness. It can lead to the confirmation of too much. We can keep adding as many epicycles and other devices as we wish to Ptolemy's geocentric system. Do it cleverly enough and we create a suitably adjusted version that can also save the appearances of planetary motion just as well as Copernicus' heliocentric system. Indeed so also can a Ptolemaic geocentric cosmology, larded with fanciful crystalline spheres, each powered in its rotation by angels. Does that fanciful hypothesis also earn a mark of truth? If saving the appearances is all that matters, then we must answer yes.

The near universal response is that merely saving the appearances is too permissive. They must be saved in the right way. Selecting this "right way" becomes pretty much the full substance of the rescued account. For otherwise, the appearances *A* are saved by *every* proposition of the form *A&X*, where *X* can be anything at all. The "right way" is what selects, among this overwhelming infinity of possibilities, just which is best favored by the evidence of the appearance.

A leading candidate is the requirement that the hypothesis must not merely entail the appearances but must explain them. This notion is the basis of abduction or "inference to the best explanation."[10] It was, according to this account, what distinguished Einstein's treatment of the anomalous motion of Mercury from mere saving the appearances. His theory explained them. As my survey (Norton, 2005) recounts, there are other candidates for this "right way" promoted in different sectors of the literature. We shall pursue just one here. It is that the favored hypothesis is the one that saves the appearances in a simple and harmonious way.

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[10] Providing a material explication of inference to best explanation is difficult. There are many notions of explanation, so the approach is not univocal. My best efforts are given in Norton (ms, Ch. 8-9). Successful inferences to the best explanation do not draw on any special inductive powers of explanation. Rather their success comes from deprecating alternatives to the favored hypothesis, either as inconsistent with the evidence or as taking on undischarged evidential debts.

This is explicitly Copernicus' argument. In the Preface to his *On the Revolutions of the Heavenly Spheres*, he censures the Ptolemaic geocentric cosmology as monstrous (1543: 1992, p.4):

> [the geocentric astronomers'] experience was just like some one taking from various places hands, feet, a head, and other pieces, very well depicted, it may be, but not for the representation of a single person; since these fragments would not belong to one another at all, a monster rather than a man would be put together from them.

A little later he exults in the harmony of his heliocentric system (p. 9):

> In this arrangement, therefore, we discover a marvelous symmetry of the universe, and an established harmonious linkage between the motion of the spheres and their size, such as can be found in no other way.

Copernicus' foremost proponent and expositor, Galileo, points directly to simplicity as the guide to probability in his dialog, *Two Chief World Systems* (1632). Having reviewed the virtues of the Copernican system, Salviati concludes in triumph (p. 327):

> See also what great simplicity is to be found in this rough sketch, yielding the reasons for so many weighty phenomena in the heavenly bodies.

Sagredo immediately summarizes Salviati's logic (p. 327, my emphasis)

> I see this very well indeed. But just as *you deduce from this simplicity a large probability of truth* in this system, others may on the contrary make the opposite deduction from it.

Needless to say, Salviati proceeds to a devastating criticism bordering on cruelty of those who resist his deductions.

6 Simplicity[11]

6.1 Principles of Parsimony

Invocations of simplicity are so common that we may barely be aware of how frequently they smooth the passage of our inductive inferences. We are interested in how a variable T is

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[11] The analysis of this section is developed in greater detail in Norton (ms, Ch. 6).

related to a variable *t*. We collect measurements and find that the measured *T* values increase linearly with the *t* values, near enough. We infer without apology to a linear relationship between *T* and *t*. The move is rarely challenged. If it is, who could resist the impatient retort: "It's the simplest. What else could it be?" This instinctive retreat to simplicity falls short of what is needed if we seek explicit principles that separate the licit from the illicit inferences. Merely being told to choose the simplest is empty without some specification of which is the simpler. And it has no inductive force unless some basis is provided for why that choice does lead to licit inferences.

When explicit statements of a governing principle of parsimony are required, perhaps the most commonly invoked is "Ockham's razor." It is usually reported as[12]

Entia non sunt multiplicanda praeter necessitatem.

Entities must not be multiplied beyond necessity.

Edifying as is William of Ockham's sentiment, we may worry that it is merely the abstract speculation of a scholar who did not himself use it in any major scientific discover. We can have no similar hesitations over a formulation by Isaac Newton, surely one of the most accomplished scientists of all eras. In composing his magisterial *Principia*, he declared a principle of parsimony that would then be used in the development of his "System of the World." Book III of this work introduces "Rules for Reasoning in Philosophy." The first is a principle of parsimony (Newton, 1726, p. 398):

> Rule I
>
> *We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.*
>
> To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

What are we to make of principles such as these? We cannot find much fault in them as pieces of homely advice. We may lighten the work of our inferential quests if we check the easy options first. However, that practicality falls short of what is needed if the principle is to be a guide to the truth. For the facts of the world feel no obligation to conform themselves to what is

[12] William of Ockham's original wording differed but conveyed essentially the same sentiment.

pragmatically convenient for us. To serve as this guide, the principle must express some fundamental fact about the world: the simpler is more likely true since nature is simple. And it must do it in an unambiguous manner so that it can be applied unambiguously.

These principles fail to meet both requirements. First, as a factual matter, Nature is often not pleased with simplicity and may employ a multiplicity of entities or causes. For millennia, traditional matter theories favored less to their detriment. The ancient Greeks presumed four elements: earth, air, fire, and water. The later alchemists presumed fewer still: Mercury, sulphur and salt. As long the element count was this small, there was little possibility of a serviceable chemistry. Matters were only rectified when Antoine Lavoisier proposed 33 elements in his *Elements of Chemistry* (1790, pp. 175-76 "Table of Simple Substances"). That set us towards the modern count that exceeds 90 elements. Even with this count secured, there are further multiplicities. All instances of each element are alike chemically. Thus parsimony would tell us that carbon is made of entities all of the same type. However all carbon is not the same. It manifests in physically distinct but chemically identical isotopes: ^{12}C, ^{13}C and ^{14}C.

Second, these proclamations are too ambiguous to be serviceable since they provide no definite means of counting causes and entities. Is the gravitational force of the sun one cause because it is the force exerted by one large object? Or is it very many causes, one for each gravitational force exerted by each atom of the sun? Is the designer god against whom Darwin railed, one cause of the many adaptations of living things? Or do we count each individual design decision as a separate cause? Do we understand the electric force of attraction between bodies as an action at a distance effect? Or is it as an interaction mediated by an electric field? In one way of counting, the action at a distance theory posits fewer entities. It posits electric charges only. The field view posits these charges and adds the mediating field. In another way of counting, the numbers reverse. If we consider the electric force on some a particular body the field view attributes it to one thing, the surrounding electric field. The action at a distance account, however, presents the force as the sum of all forces exerted by all of the very many charges in the universe.

There is a further ambiguity. We should not multiply entities "beyond necessity." We should admit no more causes than "are both true and sufficient to explain the[…] appearances [of natural things]." While we may have some intuitive notions of the key words "necessity" and "explain," the principles are not objective rules until these terms are given unambiguous meanings. Until then, one person's necessity may be another's superfluity.

6.2 Simplicity as a Surrogate

We face a familiar problem. Common inductive practice routinely employs appeals to simplicity. Yet we are unable to articulate an explicit principle upon which this practice can rely. From the perspective of the material theory of induction, this failure is inevitable. For it asserts that there can be no such universally applicable principle of inductive inference.

Understood materially, inductively efficacious appeals to simplicity are always indirect appeals to further inductive inferences. Sometimes these further inductive inferences are sufficiently convoluted that a proclamation of simplicity is a convenient way of avoiding a long convoluted narrative, or of summarizing one just given. We shall see below that this is how the Copernican appeal to simplicity should be understood. In the most straightforward cases, appeals to simplicity are simply veiled appeals to specific background facts that provide the warrants for the inductive inferences at issue. We shall see below that this case arises in appeals to simplicity in curve fitting.

Simplicity is a surrogate for further inductive inferences; and examination of inductively efficacious appeals to simplicity will reveal them. Appeals to simplicity are otherwise so varied in their details that I do not believe that a more specific statement can be given of the material analysis.

This material understanding resolves some of the ambiguity in Ockham's razor. His "necessity" makes sense as a veiled reference to something inductive: we should not infer to more entities than those to which we are authorized inductively by the evidence. Similarly Newton's Rule limits causes to those sufficient to explain the appearances. If we understand explanation in the abductive tradition, the minimal causes sufficient to explain the appearances are just those to which we should infer as the best explanation. In both cases, the principles of parsimony amount to a simple assertion: infer only to what the evidence permits. Do not go beyond. This assertion is merely a truism of inductive inference. It is good practice to follow it. However the connection to a principled parsimony is lost. The evidence may well require us to adopt something far from simple. Our best model of particle physics, the standard model, has nineteen independent constants.

6.3 Curves, Tides and Comets

Perhaps the most straightforward and most familiar appeal to simplicity arises in curve fitting. We plot measured data points for two variables *x* and *y* and then seek the curve that fits them best. Routinely, the curves explored are given by polynomial functions *y* of *x*:

linear, quadratic, cubic, quartic, …,

where the functions become more complicated as we proceed up the list, in the sense that their definitions require more independent parameters.

The familiar difficulty is that we can always secure a better fit to the data by employing functions further up this list. At some point, inevitably, our curve fit is merely accommodating noise in the data. We are overfitting. The familiar solution is that we forgo some accuracy of fit by choosing a function earlier in the list, usually guided by some explicit statistical criterion. This decision is conceived as balancing accuracy against simplicity.

This description of a familiar inductive practice makes no explicit reference to any particular case. It appears to implement some sort of universal inductive rule that is grounded in simplicity. This appearance is an illusion however. For without a context, the above prescription gives incoherent results. We can represent these same data by transformed variables such that the results of the analysis of the transformed problem contradict those of the original problem. For example,[13] we can replace *x* in the data set by another variable $z = \sin^{-1}(x)$ and then proceed as before. If we found in the first problem that the simple linear function, $y = x$ is the curve of best fit, that same function in the second problem is $y = \sin(z)$. It is not to be found anywhere among the finite order polynomial functions *y* of *z* since it corresponds to an infinite order polynomial

$$y = \sin(z) = z - (1/3!)z^3 + (1/5!)z^5 - (1/7!)z^7 + \dots$$

The standard procedures will never find this infinite order polynomial for inevitably a procedure will halt at some finite polynomial.

The material theory of induction offers a straightforward escape. The decision over which is the right variable—*x* or *z*—is determined by the particular facts of the case at hand. Indeed the entirety of the analysis is governed by these facts; and they do it without resorting to an independent principle of parsimony. These facts control even the most basic supposition of whether it makes sense to seek a curve of best fit at all. Take the example of the variables *T* and *t*

[13] A quantitative illustration of this example is given in Norton (ms. Ch.6).

mentioned above. Suppose that T is the air temperature taken at times t that happen to coincide with midday over the period of a week or two in the spring. This T may increase linearly with t. A curve of best fit would interpolate linearly between the successive temperature measurements and give us quite incorrect results for times t corresponding to the intervening midnights.

Along with the choice of variables, these facts must also specify the list of functions to be used in the curve fitting procedure. The family chosen must be such that we should expect the true curve to lie earlier in the list. These curve-fitting procedures also depend upon a statistical model of the errors confounding the data. A common model assumes independent, normally distributed errors. Any such model is applicable only in so far as it reflects the conditions factually prevailing in the case at hand.

Comet hunting, at least as practiced in the nineteenth century, gives a simple example of how the background facts provide the list of functions to be used in curve fitting.[14] Newtonian mechanics tells us that the trajectory of a comet is a conic section: an ellipse, an hyperbola or the intermediate parabola. Since the background facts tell us that comets tend to have highly eccentric trajectories, it is hard to distinguish whether they are ellipses or hyperbolas. So the first curve fitted is the intermediate parabola. Then, if the fit is poor, the next curve fitted is an ellipse. This reflects the fact that this is the trajectory of comets gravitationally bound to our sun. Such comets return regularly and are more likely to be encountered by us. Should the ellipse not fit then finally the comet hunter reverts to an hyperbola, which is the trajectory of a comet that will visit us just once.

While polynomials are familiar in curve fitting, they are inappropriate for systems with periodic behaviors, such as tides at various coastal locations. Since these tides are periodic, one might expect that the appropriate functions of time t are just $\sin(t)$ and $\cos(t)$ and their harmonics, $\sin(2t)$, $\sin(3t)$, …, $\cos(2t)$, $\cos(3t)$, … For we know from the theory of Fourier analysis that linear combinations of these harmonics will return even the most complicated of the possible periodic tidal motions. This expectation underestimates how strongly background facts control the choice of functions fitted to tidal data in the actual practice of tide prediction. The functions routinely fitted to tidal data consist of a sum of harmonics, each with an identifiable physical

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[14] This example and the example of tidal prediction are developed in greater detail in Norton (ms, Ch.6).

basis in the background facts. The most important harmonic constituent is the "principal lunar semidiurnal M2" that arises from the tidal bulge raised by the moon. The next most important is the "principal solar semidiurnal S2" that arises from the lesser tidal bulge raised by the sun. These two harmonic constituents are just the first of very many. In the nineteenth century, Thomson, who initiated this form of analysis, employed 23 constituents, each with a physical basis. For tidal predictions in US coastal regions, the United States National Oceanic and Atmospheric Administration (NOAA) expanded this set to a standard set of 37 constituents. Difficult locations may require over 100 constituents.

6.4 Ptolemy and Copernicus, Understood Materially.

The Copernican heliocentric system is favored inductively over the Ptolemaic geocentric system. That favoring is not secured, however, by a factual simplicity of the world. Whatever may be the simple merits of the geometry of Copernican astronomy, those simple merits must be balanced against something that is far from simple. It requires a sixteenth century natural philosopher to accept that, contrary to all appearances, the earth spins on its axis and careens through space around the sun. Making sense of that—dare I say—is no simple matter. Providing a proper foundation for the invisibility of this compound motion required the creation of a new science of dynamics in over a century of work by Galileo, Newton and others.

Until this thorny dynamical problem was solved, Tycho Brahe's astronomical system was momentarily a credible compromise. In it, the planets orbit the sun; and the sun orbits the earth, carrying the planets with it. This compromise keeps all the geometric advantages of the Copernican system while avoiding its dynamical drawbacks. While in some informal sense, Brahe is trading simplicity and complexity, there is no formal scheme balancing them and there is no appeal to a fact of worldly simplicity whose import was unambiguous. Other natural philosophers such as Galileo found a different balance. Brahe was merely seeking an account that fitted best with his background facts: the appearance of the motions and the appearance of a resting earth.

Nonetheless the Copernicans were indicating correctly an evidential superiority of the Copernican heliocentric system over the Ptolemaic geocentric system as far a purely astronomical considerations were concerned. If we view the comparison materially, we can see that it was just a matter of the specifics of the Copernican system being better supported evidentially than those of the Ptolemaic system. The background assumption that warrants inferences in the Ptolemaic system is that, qualitatively, the retrograde motion of the planets is explained in each case by an epicycle-deferent construction. The corresponding inferences in the Copernican system are warranted by the assumption that the planets maintain roughly circular obits, but that the retrograde motion of the planets arises from an imposition of the motion of the earth upon them.

In the Copernican system, the appearances of planetary motion then fix many of the details. Corresponding details must be set by independent stipulation in the Ptolemaic system. The relative sizes of the planetary orbits are fixed in the Copernican system; but these sizes must be set by independent stipulation in the Ptolemaic system.[15] In the Copernican system there are only two possibilities for planets: either their mean positions align with the sun and their retrograde motions carries them to and fro across the sun; or they exhibit retrograde motion only when in opposition to the sun. This conforms with the appearances. The Ptolemic system can make no corresponding assurance. This conformity must be built in by independent supposition for each planet. These and more differences give the Copernican system a strong evidential advantage.

These last remarks are merely a sketch of a lengthy and complicated collection of inferences that demonstrate the evidential superiority of Copernican system. Laying it out in detail is challenging, especially if one is engaged in polemics. There the rhetoric calls for a compelling synopsis. How better to convey the Copernican advantage than by pointing to its simplicity and harmony in comparison with the Ptolemaic system? Yet it is simpler only in requiring fewer independent posits and more harmonious in that the determination of some features necessitates others. There is no manifestation of a deeper principle of parsimony in nature.

[15] For an extended account, see the Chapter, "The Use of Hypotheses in Determining Distances in Our Planetary System."

7. Bayes

7.1 The Problem

The examples of forms of inductive inference so far have given only qualitative assessments. If the Copernican system is better supported by the astronomical evidence than the Ptolemaic because it requires fewer independent assumptions, just how much better is that support? Merely reciting "much better" may be all we can say. To many that will fall far short of what is wanted. Can we not measure support quantitatively? And if we can, might questions of strength of support be reduced to objective computations?

Something like this is the promise of Bayesian analysis. The founding tenet of objective Bayesianism is that degrees of inductive support are measured by conditional probabilities. A typical analysis begins with some prior probability distribution, which represents the support accrued by some hypothesis prior to inclusion of the evidence at issue. The import of the evidence on the inductive support of the hypothesis is found by conditionalizing on the evidence, usually through Bayes' theorem, to form the posterior probability. There is, I hope, no need to elaborate since, of all schemes in the modern literature, this one is now best known.

The difficulty with the Bayesian system is that it is too precise and irremediably so. There will be cases in which degrees of support can be represented responsibly by probabilities. They arise in narrowly prescribed problems. For example, since we can recover population frequencies for various genes, we can ask what is the probability that this sample of DNA was drawn from some donor randomly selected from the population. However evidential questions of a more foundational character are rarely given to us in a context rich in probabilities. Then insisting on a Bayesian analysis can be satisfying in the sense that we replace vague notions of strength of support by precise, numerical probabilities. However the impression of progress is an illusion. The prized numerical precision has been introduced by our own assumptions that do not reflect a corresponding precision in the system investigated. We risk mistaking our manufactured precision for that of the world.

The standard view of a Bayesian account is that probabilities are supplied by default and in abundance. The material approach reverses this. According to it, we are not authorized to any probabilities by default. Probabilities can only be introduced when the background facts warrant it; and a thorough analysis can display the pertinent warrants. Adopting that new default protects us from the spurious precision that troubles so much of Bayesian analysis. For we can only introduce precise probabilities if the precision of the facts of the context allow it. To do otherwise is to risk asserting results that are merely artefacts of applying an inductive logic ill-suited to the problem at hand.[16]

7.2 Sunrises and Laplace's Rule of Succession

The problem has been with Bayesian analysis from the outset. It can already be seen in one of the earliest Bayesian analyses. Laplace asked after the probability that the sun will rise tomorrow morning, given the past history of sunrises. This was already an established question. Before him, Hume had urged that our past history of sunrises gave no assurance of future risings. Richard Price, author of an appendix to Bayes' posthumously published paper, applied Bayes' inverse method to the problem to compute the odds of a future sunrise.[17] Laplace would now give his application of the probability calculus to the problem. His 1814 analysis (1902, p. 19) is a celebrated application of his "rule of succession." To put some formulae on Laplace's non-symbolic narrative, the analysis depended on several assumptions. We assign a probability q to the rising of the sun.

$$P(\text{rising}) = q \tag{1}$$

Antecedent to all evidence of any risings, we allow that q can have any value from 0 to 1. We represent that latitude by assign a uniform probability density p to the interval. That is,[18]

$$p(q) = 1 \quad \text{for } 0 \leq q \leq 1 \tag{2}$$

Next Laplace assumed that the individual occurrences or otherwise of a sunrise are probabilistically independent events. These assumptions were sufficient to enable Laplace to

[16] *The Material Theory of Induction*, Ch. 10, §4 gives examples of such spurious results in the form of the inductive disjunctive fallacy ("Why is there something rather than nothing?") and the lamentable doomsday argument.

[17] For more on Hume and Price, see the chapter, "The Problem of Induction," below. See Zabell (1989) for more of the history of the rule of succession.

[18] Lest it pass unnoticed, the probability P and probability density p are distinct and should not be conflated.

compute the probability of a sunrise on the n+1th occasion, given a history of s risings on n past occasions:[19]

$$P(n+1\text{th rising} \mid s \text{ risings on } n \text{ past occasions}) = (s + 1)/(n + 2) \quad (3)$$

If the sun rose on all past n occasions, then the rule of succession gives us

$$P(n+1\text{th rising} \mid n \text{ risings on } n \text{ past occasions}) = (n + 1)/(n + 2) \quad (4)$$

The more risings we see, the better supported evidentially is the next rising. Its probability approaches one arbitrarily closely with enough risings. Laplace immediately translated this probability into a wager:

> Placing the most ancient epoch of history at five thousand years ago, or at 182623 days, and the sun having risen constantly in the interval at each revolution of twenty-four hours, it is a bet of 1826214 to one that it will rise again to-morrow.[20]

7.3 What is Wrong With It?

This precise quantitative result and its operationalization in a bet is momentarily satisfying and perhaps even thrilling, if numerical precision is the goal. Yet a moment's more reflection reveals that the precision attained is fabricated and fanciful. There are two problems, to be addressed in the next two sections:

- First, the impression of recovery of a result of some generality is illusory.
- Second, a probabilistic analysis is the wrong analysis for the problem as actually posed by Laplace.

Laplace's analysis has been chosen for scrutiny here since its simplicity enables us to see both problems quickly. We might imagine that the development of the Bayesian approach after Laplace has addressed and resolved these problems. To some extent, this has happened. Where these problems persist most notably, however, is in Bayesian analyses in philosophy of science.

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[19] See the Appendix for a summary of the computation.

[20] The computation of the number of days in 5000 years as 182623 is an obvious error, too low by a factor of 10. Five thousand years corresponds to 5,000 x 365 = 1,825,000 days or 5,000 x 365.2422 = 1,826,211 days depending on how one counts days in the year. The odds reported by Laplace of 1,826,214 to one indicate that Laplace's real estimate of the number of days in 5,000 years is 1,826,213. The erroneous 182,623 results from dropping the tens digit 1.

There these methods are routinely applied to problems with vague specifications. The goal is to supplant their vagueness with mathematical precision. This laudable goal, however, can only be achieved by imposing assumptions whose precision is unwarranted by the problems posed. As with Laplace's sunrises, the precision of the ensuing analysis is an illusion of our own manufacture.

7.4 Failure of Generality

Laplace's "rule of succession" is presented with a suggestion of some sort of general applicability. Perhaps it is a general demonstration that probabilistic analysis defeats Hume's skeptical challenge to inductive inference. While the application to sunrises specifically is far-fetched, perhaps it shows that probabilistic analysis can solve the sort of inductive problems Hume charged as insoluble. Or perhaps more modestly it is, at least in simple cases, a convenient starting point for how we are to think of projecting a record of successes and failures inductively into the future.

From the perspective of the material theory of induction, it does none of these. It is a theorem in probability theory, untroubling merely as a piece of mathematics. However, as an instance of inductive inference, it is untethered from real problems in the world. Any inductive rule, such as the rule of succession, can only be applied to some particular problem if the background facts of the domain warrant it. Without that tethering, it is just a piece of mathematics.

To which inductive problems can the rule be tethered? That is, which problems are such that their background facts warrant the rule. We find that there are very few and they are artificial.[21]

It is no surprise that the rule of succession fails for the real problem of sunrise prediction. The pertinent background facts are rich. Sunrises come about from the rotation of the earth on its axis; and this rotation is one that can only be disrupted by the most cataclysmic of cosmic events. Absent such a cataclysm, successive risings are perfectly correlated; and after such a cataclysm, successive failures to rise are perfectly correlated. The assumption of probabilistic independence

[21] We might compare this rule with the ideal gas law in the thermodynamics of gases. It is derived from highly idealized assumptions. Unlike the rule of succession, the ideal gas law applies to a wide range of ordinary gases in ordinary circumstances.

for each sunrise fails. If we are serious about predicting such a cataclysm from, say, an errant galactic body, then our analysis would need to inquire after the distribution of such bodies in our neighborhood and to hope that sufficient information is forthcoming so that probabilistic predictions of cataclysmic collisions with earth can be mounted.

Laplace had no illusions that his analysis was close to one that accommodated what we know factually of sunrises. He continued the report on the bet quoted above by saying:

> But this number is incomparably greater for him who, recognizing in the totality of phenomena the principal regulator of days and seasons, sees that nothing at the present moment can arrest the course of it.

This does not appear to be a retraction of his analysis, but may merely be a statement that it gives an excessively modest lower bound to the probability appropriate to our real epistemic situation.

If not sunrises, then might Laplace's analysis apply to the expectation of live human awakening? Then biological facts as summarized in mortality tables provide the background facts needed to assess the probability of a human awakening tomorrow, given some past history of awakenings. A 20 year old male has a 20 year history of successful awakenings. Mortality tables[22] tell us that a male has a probability of 0.998827 of surviving the next year. Taking the approximation that the probability of a successful awakening each morning in the year is the same, the probability of success on the next morning is $0.998827^{1/365} = 0.999996784$. The same computation for a 100 year old female gives us a probability of success in awakening the next morning as $0.69845^{1/365} = 0.99901722$. The rule of succession does not apply.

These examples may be multiplied. Laplace's analysis is almost never warranted by background facts. Where does it apply? Laplace's own text shows us a way. The problem of sunrises comes at the end of Laplace's Chapter 3. Virtually all the other examples in that chapter are of familiar games of chance and associated randomizers: the tossing of coins, the throwing of dice and the drawing of black or white balls randomly from an urn. Consider this problem:

> An urn contains a very large number of coins, which are biased in all possible ways. The biases are uniformly distributed over all possible values: coins with a chance of heads *q* appear in the urn with the same frequency for all *q* in the entire range from

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[22] Provided by the US Social Security Administration at https://www.ssa.gov/oact/STATS/table4c6.html

0 to 1. We select a coin at random from the urn.[23] We toss it 1,826,213 times and find heads on every toss. What is the probability that the next toss is a heads?

It requires only a little reflection to see that all the conditions for Laplace's rule of succession are satisfied. The background facts warrant the application of Laplace's rule of succession. It assures us that the odds of a head on the next toss are 1,826,214 to one.

Laplace's analysis illustrates a common problem with Bayesian analysis. It has a small repertoire of tractable templates. They include sampling problems, such as drawings from urns; and problems in games of chance, which are based on physical randomizers, like thrown dice, shuffled cards and tossed coins. The supposition is these templates can be applied to problems that bear only superficial resemblance to the original problems of sampling or games of chance. This supposition mostly fails. Inductive problems in the real world—especially the more interesting ones—are rarely structurally like simple problems of sampling or games of chance.

7.5 Probabilities are Inapplicable

Laplace's mention of his analysis as applying to sunrises can and, indeed, should be taken only as a colorful embellishment intended to make an arid technical problem appear less dry. For the problems is posed *by assumption* in a factually barren landscape. The problem's formulation fails to provide the background facts that are required to warrant an inductive inference. To describe the problem as inferring from the evidence of 182623 sunrises is misleading, if taken seriously. Calling them "sunrises" triggers the sorts of background knowledge mentioned above that we are supposed to discount. Successive sunrises are very strongly correlated, yet Laplace's analysis makes them probabilistically independent. A better description might be the vaguer evidence statement:

We have 1,826,213 successes. Will the next occasion be a success?

The only answer we can give is that we cannot say. The evidence is given in a vacuity of background facts. It supports no inductive inference. We need background facts on the nature of the occurrences to warrant an inductive inference. When they are supplied, we can determine just

[23] I follow Laplace in overlooking the practical and principled difficulties of selecting randomly from an urn with an infinity (here uncountable) of balls or coins. A safer system spins a pointer on a dial to select a number randomly between 0 and 1. We then construct a coin with that number as its bias.

which inductive inferences are warranted. Which they are will vary from circumstance to circumstance. Laplace's analysis will almost never apply.

If we persist in applying a Bayesian analysis and recover results of any strength, where none are warranted, all we can conclude is that these results are artefacts of a misapplied inductive logic. Once we are alerted to the danger, it is easy to see how Bayesian analysis introduces factual presumptions under the guise of benign analytic machinery. The idea that the unspecified occurrence can be represented by a probability distribution at all is an example. It commits us to factual restrictions that go beyond the factual barrenness presumed. To assign a middling value to the probability, $P(\text{rising}) = q = 0.5$, is not to be neutral. It is to say that, loosely speaking, in situations similar to that of the analysis, we should expect an occurrence in roughly half of them.

Then there is the attempt to represent the complete openness over which value of q applies. Laplace does his best here by assuming a uniform probability distribution (2) over q. This uniform distribution once again goes beyond the factual barrenness presumed. For that distribution makes many strong claims. It says that a value of q in the interval $(0, 0.1)$ is as probable as a value of q in the interval $(0.5, 0.6)$ but only half as probable as a value of q in the interval $(0.5, 0.7)$. The interval $(0, 0.99)$ is highly probable and its complement $(0.99, 1.0)$ highly improbable. These are strong statements. The absence of background facts means that none of them are authorized.

The difficulty of representing evidential neutrality in a probabilistic analysis is well-known. Various techniques known as "imprecise probability" can be used to ameliorate the failure of a uniform probability density to represent adequately a complete indifference over the values of the parameter q.[24] In one approach, we replace the single prior probability density (2)

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[24] Might we escape these problems by adopting subjective Bayesianism? Then the prior probability distribution is merely uninformed opinion and may be freely chosen, as long as it preserves compatibility with the probability calculus. This popular approach has had a malign effect if one's interest is inductive support and bearing of evidence. For once one allows opinion free admission into one's system, it becomes very difficult to remove its taint from one's judgments of inductive support. The limit theorems that are supposed to purge the subjectivity apply in limited, contrived circumstances that do not match the real practice of science.

over q by the set of all[25] probability densities over the interval [0, 1]. When we apply the rule of succession, instead of recovering a single probability for the next occurrence, we recover a set of probabilities. In general, there is one for each of the probability densities in the set. That we admit all probability densities gives the appearance of the requisite independence from background facts. That appearance is an illusion since we are still assuming that the probability calculus applies at all, even in weakened form. The introduction of this imprecision is fatal, however, to the recovery of a non-trivial result. For, as we see in the Appendix, the set of all prior densities includes ones that lead to all possible probabilities from zero to one for the next sunrise. We start assuming that this probability can lie anywhere between 0 and 1 and must end without any restriction on this range. We will have learned nothing from the evidence, no matter how extensive our history of sunrises may be.

7.6 Bayesian Analysis within the Material Theory of Induction

What are the prospects for Bayesian analysis from the perspective of the material theory of induction? Bayesian analyses can be applied profitably to many, specific inductive problems. Given what we know about errant galactic bodies, what should our expectations be for a cataclysmic collision with the earth that will disrupt our sunrises? Given patients with such and such prognosis, what is their life expectancy? These and many more problems like it are all welcomed by the material theory of induction. For in each case there are identifiable background facts that warrant the application of a probabilistic analysis.

Where Bayesian analysis fails is that it cannot provide an all-embracing framework with formal rules applicable to all problems of inductive inference. It will work well in specific problems, where the background facts warrant it. But any claim of general applicability, such as is sought in the philosophy of science literature, requires that the framework must be applicable to inductive problems whose background facts fail to authorize a probabilistic analysis. In these cases, persisting in applying a probabilistic analysis risks producing results that are artefacts of an inapplicable inductive logic.

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[25] The scope of "all" is vague, but that vagueness is immaterial to the points made here. As a first pass, it designates all integrable functions with unit norm.

8. Conclusion

In reviewing the material theory of induction, this chapter has been restricted to particular instances of inductive inference. In each case, the warrant for the inferences is found in background facts. For the inference to be licit, these background facts must be truths. Since these facts make claims that commonly extend well beyond direct experience, we must ask what supports the truth of these background facts. The material theory of induction is uncompromising in its answer. The only way these facts can be supported is by further inductive inferences; and those further inductive inferences will in turn require a warrant in still further inductive inferences. How do all these inferences fit together? That is the subject of this volume and is taken up in the next chapter.

Appendix: Laplace's Rule of Succession

Consider n+1 probabilistically independent trials, each with a probability of success q, where q is itself uniformly distributed over the interval [0,1] according to (2). If there are s successes only among the first n trials, then the probability of success on the n+1th trial is given by

P = P(success on n+1th trial | s successes in first n trials)

= P(success on n+1th trial AND s successes in first n trials) / P(s successes in first n trials)

Since the number of successes s is binomially distributed, we have:

$$P = \frac{\int_0^1 q \cdot \frac{n!}{s!(n-s)!} q^s (1-q)^{n-s} p(q)\, dq}{\int_0^1 \frac{n!}{s!(n-s)!} q^s (1-q)^{n-s} p(q)\, dq} = \frac{\int_0^1 q^{s+1} (1-q)^{n-s}\, dq}{\int_0^1 q^s (1-q)^{n-s}\, dq}$$

The integrals may be evaluated using the integral identity

$$\int_0^1 q^A (1-q)^B\, dq = \frac{A!B!}{(A+B+1)!} \qquad \text{(A1)}$$

for whole numbers A and B. We recover

$$P = \frac{(s+1)!(n-s)!}{(n+2)!} \cdot \frac{(n+1)!}{s!(n-s)!} = \frac{s+1}{n+2} \qquad \text{(2)}$$

It is the rule of succession (2) of the text.

To show that alternatives to the prior probability distribution (1) can lead to $P = r$ for any r between 0 and 1, consider the family of prior probability distributions:[26]

$$p(q) = \frac{(A+B+1)!}{A!B!} q^A (1-q)^B \quad \text{where } 0 \le q \le 1$$

for A and B whole numbers. Repeating the above calculation for P, we find

$$P = \frac{\int_0^1 q^{A+s+1}(1-q)^{B+n-s}\, dq}{\int_0^1 q^{A+s}(1-q)^{B+n-s}\, dq} = \frac{(A+s+1)!(B+n-s)!}{(A+B+n+2)!} \cdot \frac{(A+B+n+1)!}{(A+s)!(B+n-s)!} = \frac{A+s+1}{A+B+n+2}$$

Rewriting P as

$$P = \frac{A}{A+B} \cdot \frac{1+(s+1)/A}{1+(n+2)/(A+B)}$$

it follows that $P \rightarrow r$ in the limit of $A, B \rightarrow \infty$ such that $A/(A+B) \rightarrow r$. That is, we can bring P arbitrarily close to any nominated $0 \le r \le 1$, merely by selecting A and B large enough in this limiting process. The prior probability $p(q)$ masses all the probability arbitrarily closely to $A/(A+B)$ in the process of taking the limit. The limit itself is no longer a function, but a distribution, the Dirac delta "function." That is

$$\underset{\substack{A,B\rightarrow\infty \\ A/(A+B)\rightarrow r}}{Lim}\; p(q) = \delta(q-r)$$

Selection of this distribution as a prior would force P to the value of r exactly, since all intervals of values not containing r would be assigned a zero prior probability.

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[26] Identity (A1) assures normalization to unity.

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